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The American Political Science Review

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Published Quarterly by

The American Political Science Association

Vol. LXVIII September 1974 No. 3

POLITICAL SCIENCE ★★★★★★★★★★★★★★★★★★

Kenneth Prewitt and Sidney Verba

AN INTRODUCTION TO AMERICAN GOVERNMENT

This introduction to the foundations, spirit, and operation of the American political system includes innovative discussions of how the U.S. economic system has shaped our democracy, how political capitalism effects the class structure, the degree to which the principle of democracy has its roots in the hearts and minds of the American people, and how the American citizen participates in political life. Policy is discussed in depth with applications made to taxes, medical care, desegregation, and Vietnam. There is a postscript on Watergate. 630 pages; $9.95. 1974. *Instructor's Manual.*

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Political Democracy and Social Equality

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Monte Palmer, Larry Stern, and Charles Gaile

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This text deals with the problems of clarity, precision, operationalization, and measurement inherent in the use by political scientists of concepts, theories, and conceptual frameworks developed by members of other social science disciplines. The intent is to provide students with a basic conceptual understanding which will facilitate their acceptance of concepts and conceptual frameworks. Tentative: 177 pages; $4.95; paper. August 1974.

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FROM HARPER & ROW

10 East 53d Street, New York 10022

The American Political Science Review

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Office of publication: **Curtis Reed Plaza, Menasha, Wisconsin.**
Foreign Agent: **P. S. King and Staples, Ltd., Great Smith Street, Westminster, London.**
Second class postage paid at Washington, D.C., and at additional mailing offices.
Printed in the United States of America by George Banta Company, Inc., Menasha, Wisconsin.
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ARTICLES

951 Political Issues and Trust in Government: 1964–1970. National survey data demonstrate that support of the federal government decreased substantially between 1964 and 1970. Policy preference, a lack of perceived difference between the parties, and policy dissatisfaction were hypothesized as correlates of trust and alternative explanations of this decrease. Analysis revealed that the increased distrust in government, or cynicism, was associated with reactions to the issues of racial integration and U.S. involvement in the Vietnam war. A curvilinear relationship was found between policy preference on these and other contemporary social issues and political cynicism. The minority favoring centrist policies was more likely to trust the government than the large proportion who preferred noncentrist policy alternatives. This complex relationship between trust and policy preference is explained by dissatisfaction with the policies of *both* political parties. The dissatisfied noncentrists formed highly polarized and distinct types: "cynics of the left," who preferred policies providing social change, and "cynics of the right," who favored policies of social control.

By Arthur H. Miller, Study Director, The University of Michigan, Center for Political Studies.

Comment. By Jack Citrin, Assistant Professor of Political Science, University of California, Berkeley.

Rejoinder. By Arthur H. Miller.

1002 Theory and Voting Research: Some Reflections on Converse's "Change in the American Electorate." Philip E. Converse has challenged the findings of a 1965 article, "The Changing Shape of the American Political Universe," and other work by Burnham. Converse asserts that most of the very high voter participation which occurred before 1900 can be explained by a combination of electoral corruption, the absence of personal-registration requirements and other "undramatic" factors, and thus that the anomalies which Burnham reported are largely spurious. Issues of major importance for social-science explanation are joined. The present article attempts to demonstrate that intervening structural variables cannot come close to explaining all the post-1900 decline in voting participation and that the genuine existence of universal nineteenth-century rural corruption has yet to be demonstrated. These efforts to explain away anomaly are held to be unpersuasive. The weight of evidence supports the objective reality of the phenomena originally reported. This in turn means that more adequate conceptualizations are needed to integrate empirical findings than those which have hitherto dominated the voting-behavior research community.

By Walter Dean Burnham, Professor of Political Science, Massachusetts Institute of Technology.

Comment. By Philip E. Converse, Professor of Political Science, University of Michigan.

Comment. By Jerrold G. Rusk, Associate Professor of Government, University of Arizona.

Rejoinder. By Walter Dean Burnham.

1058 Social Change and Political Mobilization in the United States: 1870–1960. The central problem of the general search for explanations of political change has been the lack of adequate explanations of the relationship between social change and political change. This research proposes and tests a six-variable causal model of the process of social change and political mobilization in the United States during the period 1870 to 1960. The variables used are based on previous theoretical efforts which have indicated that the process of social and political change is a syndrome. From these previous efforts a new model is synthesized. The model is found to operate as proposed during the period 1870 to 1910, and a simplified version in four variables is identified for the period 1920 to 1960.

One of the central questions explored by this research is the degree to which the pattern of social change alters as the process of change proceeds through time. The transition in the United States is explained by reference to the threshold effect of two social infrastructures—urbanization and government activity in education.

In light of the identification of the model, an attempt is then made at revising certain aspects of modernization theory.

By Jesse F. Marquette, Assistant Professor of Political Science, The University of Akron.

1075 Institutionalization, the Ombudsman, and Bureaucracy. This article proposes an authority based conception of the phenomenon of institutionalization. Whereas most social science writing on the subject focuses either upon the organization's internal structural integration (such criteria as well-boundedness, universalism, complexity, and coherence often are employed) or upon its ability to cope with environmental challenges (the concepts of adaptability and autonomy have been suggested), I propose that an organization's *offensive capabilities* vis à vis environmental actors be viewed as a measurement of its institutionalization.

Principally based upon fieldwork in New Zealand bolstered by additional research in Scandinavia, Britain, and Hawaii, this study focuses upon the institutionalization of the ombudsman—an increasingly popular bureaucratic control mechanism. A sociometric analysis of ombudsman-bureaucratic interaction is undertaken, and four questions are investigated: How extensive is the interaction? How consequential is the threat posed by complaints? What demands does the ombudsman make? How cooperative is the agency in responding? The investigation provides answers which are indicative of the ombudsman's successful institutionalization. That is, the office performs its mission and has established itself with the environmental actors as an authority figure.

By LARRY B. HILL, Associate Professor of Political Science, University of Oklahoma.

1086 **Hegel's Critique of Liberal Theories of Rights.** In "Abstract Right," the first part of the *Philosophy of Right*, Hegel criticizes the usual content and formulations of liberal theories of rights. In terms of content, Hegel argues that the subject of rights is only a narrow abstraction from the full human being; that he has limited self-determination and limited political freedom; and that, when he acts on his rights, he produces terror and destruction. In terms of formulations, Hegel argues that the pervasiveness of contract relations is inaccurate and undesirable; that the state cannot be derived from the natural man's alienating his right to punish; and that it is inaccurate to conceptualize civil society as only limiting natural man's freedoms. By transforming natural to abstract rights, Hegel retains much of the substance of rights, while concurrently preparing for the later sections of his text which try to overcome the inadequacies of a political theory based only on rights.

By PETER G. STILLMAN, Assistant Professor in Political Science, Vassar College.

1093 **Party Coalitions in Multiparty Parliaments: A Game-Theoretic Analysis.** This study focuses on A. Lawrence Lowell's classic thesis that a parliamentary democracy must possess a majority party system if durable cabinets are to exist. The argument of this study is that majority party government is not essential to cabinet durability. Rather, in line with the British analyst W. L. Middleton as well as more contemporary game-theoreticians, the critical factor is held to be the coalitional status of the cabinet: (1) cabinets of minimum winning status should be durable; as cabinets depart from minimum winning status, cabinet durability decreases; (2) the coalitional status of the cabinet that forms is partially a product of party system fractionalization, instability, and polarization. Hypotheses derived from the theory are tested with data drawn from 17 Western parliamentary democracies, from 1918 to 1940 and from 1945 to 1970. The findings generally support the theory. A key to durable government is the minimum winning status of the cabinet. Minimum winning cabinets are possible in multiparty and majority party systems.

By LAWRENCE C. DODD, Assistant Professor of Government, University of Texas, Austin.

1118 **The Mediating Influence of State Legislatures on the Linkage Between Interparty Competition and Welfare Policies.** The purpose of this paper is to reformulate the linkage between interparty competition and welfare policies in the American states. Specifically, it is hypothesized that this linkage should be substantially greater among states with strong, effective legislatures than among states with weak, ineffective legislative systems. When legislative effectiveness was defined in terms of professionalism and welfare effort was assessed by seven specific measures, it was found that the link between party competition and welfare expenditures was indeed stronger among states with effective, as opposed to ineffective, legislative systems. Further, when controls were introduced for several socioeconomic factors, the linkage continued to be stronger among states with professional legislatures. It is concluded, consequently, that the influence of party competition on state welfare policies is mediated by the differential effectiveness of state legislatures.

By EDWARD G. CARMINES, Ph.D. Candidate in Political Science, State University of New York at Buffalo.

1125 **Urbanization and Political Demand Making: Political Participation Among the Migrant Poor in Latin American Cities.** This paper investigates the proposition that rapid urbanization produces significant changes in the kinds, volume, and intensity of demand making aimed at local and national governments, leading to political system "overload" and pressure for major shifts in resource allocation. Drawing upon data gathered among low-income migrants to Mexico City and other Latin American cities, the paper analyzes the process through which objective needs are converted into demands upon government. The findings indicate that there are often major lags in the process of demand creation among cityward migrants, and that many kinds of felt needs are viewed by migrants as needs to be satisfied primarily through individual rather than governmental action.

Data are presented on the incidence of demand making among the migrant population and the substantive nature of the demands they make upon government. Strategies used in attempting to influence government decisions are described, and the attitudes and perceptions underlying the migrant's preference among alternative strategies are analyzed. The long-term propensity of migrants and their offspring to engage in demand making with regard to broad social and economic issues rather than individual or community-related needs is assessed.

By WAYNE A. CORNELIUS, Assistant Professor of Political Science, Massachusetts Institute of Technology.

1147 **Clientelist Politics in the Philippines: Integration or Instability?** Philippine data are presented which indicate that a contradiction exists between changes induced partly through capital accumulation by the indigenous elite and foreign investment, and both increased political factionalism and declining voting participation. While national elites become more powerful through capital accumulation, local political machines confront structural changes weakening their power. More specialized patron-client structures diminish local elites' ability both to deliver votes to national patrons and to stimulate electoral participation. Growth of the middle class in a stagnant economy increases competition for lucrative local political office Factions proliferate and with the increased concentration of private income, become more dependent on national patronage resources. Unable to meet rising patronage demands, the government resorts to extensive deficit spending which stimulates inflation and further undermines economic growth. The national elite's economic activities thus undermines its authority base as the state becomes increasingly less able to provide security to individuals dislocated by changes generating profit for the elite.

By THOMAS NOWAK, Assistant Professor of Political Sociology, Justin Morrill College, Michigan State University, and KAY SNYDER, Assistant Professor of Sociology, Michigan State University.

1171 **An Organizational Approach to the Study of Political Culture in Marxist-Leninist Systems.** This paper is addressed to three tasks and the analysis operates at three levels. First, there is an attempt to specify an analytic approach to Marxist-Leninist sociopolitical systems that integrates regime and sociocultural units. This approach rests on a structural conception of political culture, a conception that stresses the informal adaptive quality of political culture, and that includes behavioral as well as attitudinal patterns. The second task consists of analyzing the paradoxical character of development in Soviet-type systems; development that simultaneously reinforces and undermines traditional-peasant political cultures at the community, regime, and elite levels. Finally, this pattern of development is examined in the context of a single Soviet-type regime and society, the Romanian.

By KENNETH JOWITT, Assistant Professor of Political Science, University of California, Berkeley.

1192 **Foreign Policy and Empirical Democratic Theory.** This paper compares the research on the determinants of domestic policy outcomes, primarily in American state governments, with similar research in the area of foreign policy. Using seven foreign policy measures, it then tests a hypothesis based on Cutright's cross-national analysis of social insurance programs, that political representativeness is more important in accounting for policy outcomes among developed than among undeveloped nations. Finally, it suggests what implications the reported findings may have on empirical democratic theory.

By DAVID W. MOORE, Assistant Professor of Political Science, University of New Hampshire.

1198 **The ABM Issue in the Senate, 1968-1970: The Importance of Ideology.** Three hypotheses suggesting why senators might adopt or change positions on such an issue as the ABM are compared. The empirical analysis clearly substantiates the contention that position reflects ideology, not party commitment or potential state economic benefits. Furthermore, the influence of ideology is seen to have grown more apparent each year the issue was contested in the Senate. Virtually all the senators who changed position between 1968 and 1970 had initial positions that did not accord with their ideology, and they moved so as to bring them in accord. Virtually all those senators whose initial position was in accord with their ideology maintained that position.

by ROBERT A. BERNSTEIN, Assistant Professor of Political Science and WILLIAM W. ANTHONY, Lecturer in Political Science, Texas A&M University.

1207 **Need for Achievement and Competitiveness as Determinants of Political Party Success in Elections and Coalitions.** Need for achievement and strategic predispositions among political party elites are hypothesized to have an important impact on the success parties enjoy in elections and in coalitions. More specifically, this study develops and tests a model which suggests that parties whose leaders have high need for achievement and are predisposed to pursue a mixed competitive/cooperative strategy are more likely to do well in elections and in coalitions than are parties whose leaders are low in need for achievement and oriented to either cooperative or competitive strategies.

When the Indian political party system between 1967 and 1971 is used as the data base, the success or failure of political parties is correctly predicted by need for achievement for thirteen out of fourteen variables. By means of multiple regression analysis, as much as seventy-two per cent of the variance in the electoral success of Indian parties is explained by the model.

By BRUCE BUENO DE MESQUITA, Assistant Professor of Political Science, University of Rochester.

1221 **Bases of Budgetary Incrementalism.** This essay analyses the explanatory power of Davis, Dempster, and Wildavsky's theory of budgetary incrementalism. By means of sensitivity testing, it demonstrates that inferences to "gaming" or strategic explanations of budgetary incrementalism are not warranted on the basis of correlational analysis.

To explain budgetary incrementalism more satisfactorily, recourse is made to concepts and variables explicit in the vocabulary of the budget process participants. When mandatory requests are distinguished from programmatic requests, the differential treatment of the two by Congress is observed to allow good explanation of budgetary relations. In particular, the inexorable but small mandatory request, which is almost automatically granted, is adequate by itself to explain why requests always increase and why one year's appropriation surpasses the previous one.

By JOHN WANAT, Assistant Professor of Political Science, University of Kentucky.

1229 **Environment and Party: The Impact of Political and Demographic County Characteristics on Party Behavior.** While many scholars have recognized that decentralization encourages American party organizations to tailor activities to the local environment, few have studied systematically the relationships between that environment and party behavior. This study examines the impact of certain political and demographic county characteristics on the activities of a national sample of county party organizations in 1964. Three dimensions of party behavior—organization, mobilization, and persuasion—are utilized as dependent variables. The relationships between the environment and these dimensions of party behavior in the North support a revised "machine theory" of environment and party: organizational effort does not vary with environmental conditions, while mobilization and persuasion activities are opposites in their relationships with the concentration of parochially-oriented voters. Additionally, the division of partisan strength influences party activity: parties perform their "natural" activities well where they have strong support and the other party's "natural" activities well under competitive conditions. Few significant relationships are found in the South, but their similarity in direction to those in the North suggests that the normal relationships may have been attenuated by circumstances unique to that region, particularly one-partyism and decades of "whites only" politics.

By PAUL ALLEN BECK, Assistant Professor of Political Science, University of Pittsburgh.

1245 **Conflict in the Community: A Theory of the Effects of Community Size.** Two theories of community conflict are examined in this paper with data from elections in 89 cities in the San Francisco Bay area. One theory is developed from the work on group conflict by Georg Simmel and Lewis Coser while the other is a rational choice theory based on assumptions about the costs and risks of conflict in different size cities. Both theories suggest that conflict, while more frequent in larger communities, is likely to become most severe in smaller communities. Both theories are confirmed by the pattern of findings in the analysis, but the rational choice theory proves to have the greater generality, i.e., that it can explain more of the findings in the paper.

By GORDON S. BLACK, Associate Professor of Political Science, University of Rochester.

1262 COMMUNICATIONS

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By GEORGE H. QUESTER, Professor of Government, Cornell University.

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Political Issues and Trust in Government: 1964–1970*

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Recent Trends Toward Political Alienation

A democratic political system cannot survive for long without the support of a majority of its citizens. When such support wanes, underlying discontent is the necessary result, and the potential for revolutionary alteration of the political and social system is enhanced.[1] The existence of a substantial degree of political discontent within a society at any one point in time does not necessarily, however, signify a decaying of the social and political order. On the contrary, in a democracy such discontent may lead to political and social change or may result in the electoral practice of "throwing the rascals out." In either case, the satisfaction of the unfulfilled needs which presumably led to the discontent gives testimony to the flexibility of the political system and the ability of government to manage conflict. It has, in fact, been argued that having a minority of politically apathetic individuals (who are presumably satisfied with the status quo) adds to that flexibility.[2] When dissatisfaction with the existing situation leads, however, to pervasive and enduring distrust of government, this flexibility is greatly curtailed,[3] thereby increasing the potential for radical change. Extensive periods of high political discontent imply that needs among certain segments of the population are not being met; their discontent is thus an indicator of dissatisfaction with the quality of life they are experiencing.

A period of sustained discontent may result from deep-seated social conflict which, for some segment of the population, has been translated into a negative orientation toward the political system because their sense of insufficient political influence implies a futility in bringing about desired social change or control through political efforts; hence, they feel government is generally not to be trusted because it does not function for them. Such feelings of powerlessness and normlessness are very likely to be accompanied by hostility toward political and social leaders, the institutions of government, and the regime as a whole. In such case, "throwing the rascals out" will have little, if any, effect on restoring confidence in government or the political system. While discontent that exists only for a short time and acts as a catalyst for needed change may reflect a functional political system, extended periods of widespread political malaise suggest that the normal means by which conflict is managed in the political system are not fully operative.

A situation of widespread, basic discontent and political alienation exists in the U.S. today. Support for this contention can readily be found in the daily reports of the mass media, in an examination of recent political events—witness the substantial support for George Wallace in both the 1968 election and the 1972 primaries—and in the 1972 political campaign rhetoric. But, more convincingly, it is also found in national survey data.

Survey questions dealing with political efficacy and political trust, two dimensions of alienation,[4] provide evidence substantiating this contention. Converse[5] has recently reported that between 1960 and 1968 there was a steady decline in political efficacy—the feeling that an individual can have an impact upon the political process—despite large overall increases in education. In 1960, for example, nearly 42 per cent of the population disagreed (the efficacious response) with the following statement: "Sometimes politics and government seem so complicated that a person like me can't really understand what's going on." In 1968, the comparable figure was 30 per cent and in 1970, it was only 26 per cent.

A similar and equally dramatic change was found in five survey questions designed to measure

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* Thanks are extended to Warren E. Miller for his encouragement and valuable advice and to C. Richard Hofstetter, John Kessel, John Stucker and Herbert Weisberg for comments on an earlier draft, as well as to Thad Brown and Al Raine of the Institute for Social Research and Gary Gartin of The Ohio State University for their assistance in the data analysis.

[1] William A. Gamson, *Power and Discontent* (Homewood, Ill.: Dorsey Press, 1968), chapter 9.

[2] Bernard R. Berelson, Paul F. Lazarsfeld, and William N. McPhee, *Voting* (Chicago: University of Chicago Press, 1954), chapter 14.

[3] Gamson, pp. 42–48.

[4] For a discussion of the dimensions of political alienation, see Ada W. Finifter. "Dimensions of Political Alienation," *American Political Science Review,* 64 (June, 1970), 389–410.

[5] Philip E. Converse, "Change in the American Electorate," in *The Human Meaning of Social Change,* ed. Angus Campbell and Philip E. Converse, (New York: Russell Sage Foundation, 1972).

political trust.[6] The percentage distribution of responses to these items is presented in Table 1. These data reveal a strong trend of increasing political cynicism for the general population between 1964 and 1970. The change in the responses to all but one of the items is somewhat more than 20 percentage points in the direction of increased distrust of the government. Even with all five items, the average increase in cynical responses was slightly more than 17 percentage points. The magnitude of this change can be better appreciated in light of the finding that responses to the same five items had showed an average change of only two percentage points in a cynical direction during the previous six-year period from 1958 to 1964. That some segment of American society believes that officials violate legal procedures in dealing with the public or in arriving at policy decisions is not surprising. What is startling, and somewhat alarming, is the rapid degree of change in this basic attitude over a period of only six years.

The Study of Political Cynicism. Political trust can be thought of as a basic evaluative or affective orientation toward the government,[7] and, more specifically for this study, the government in Washington.[8] The dimension of trust runs from high trust to high distrust or political cynicism. Cynicism thus refers to the degree of negative affect toward the government and is a statement of the belief that the government is not functioning and producing outputs in accord with individual expectations.

As noted, cynicism and efficacy have been viewed as components of the more general concept of political alienation,[9] and have been subject to increased empirical study recently. These studies tend to emphasize either background and personality characteristics or, to a lesser extent, reactions to political decisions as the main explanations of political trust and cynicism.[10] The focus of this paper is on the impact that reactions to political issues and public policy have on the formation of political cynicism.

The Data. The data used in this report are based on The University of Michigan Survey Research Center election studies (now conducted by the Center for Political Studies—CPS) of a national cross section of eligible voters for the years 1964, 1966, 1968, and 1970.[11] Each of these surveys included a battery of questions dealing with public policy on race relations, foreign affairs, and a variety of domestic problems. These issues provide a wealth of data on which to test the hypothesized relationships between political issues and cynicism. Since all of the policy questions were

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[6] John Robinson, Jerrold Rusk, and Kendra B. Head, *Measures of Political Attitudes* (Ann Arbor, Michigan: Institute for Social Research, 1969), pp. 626–647.

[7] Donald E. Stokes, "Popular Evaluations of Government: An Empirical Assessment" in *Ethics and Bigness: Scientific, Academic, Religious, Political and Military*, ed. Harlan Cleveland and Harold D. Lasswell (New York: Harper & Brothers, 1962), p. 64.

[8] See Joel D. Aberbach, "Alienation and Political Behavior," *American Political Science Review*, 63 March, 1969), 36–99 for a discussion of the importance of a specific referent when measuring political alienation. See also Kenneth Keniston, *The Uncommitted: Alienated Youth in American Society* (New York: Harcourt, Brace & World, 1965), pp. 453–455.

[9] Cynicism and efficacy correspond very closely to Seeman's conceptualizations of normlessness and powerlessness and have been treated as theoretically separate components of political alienation by a number of analysts. Melvin Seeman in "On the Meaning of Alienation," *American Sociological Review*, 24 (December 1959), 783–791 defines normlessness as ". . . a high expectancy that socially unapproved behaviors are required to achieve given goals." Gabriel A. Almond and Sidney Verba, *The Civic Culture* (Boston: Little, Brown & Co., 1965) have emphasized the difference between these two concepts by drawing the distinction between "input affect," that is, political efficacy, and "output affect," or political cynicism (discussed in terms of trust). Gamson, in *Power and Discontent*, p. 42, has likewise stressed their conceptual differences. He notes that the efficacy dimension of political alienation refers to "people's perception of their ability to influence." There currently exists an extensive body of literature dealing with the conceptual problem of studying political trust, political efficacy and, more generally, political alienation. See, in particular, Aberbach, "Alienation and Political Behavior," and Finifter, "Dimensions of Political Alienation."

[10] The empirical literature investigating trust is characteristically lacking in two respects: longitudinal considerations are rarely entertained, and little use is made of political issues as explanatory variables. Furthermore, an overwhelming majority of those few studies dealing with any form of relationship between issues and cynicism were either based upon restricted populations, such as college students or particular cities, or done under special conditions, such as at times of riots. Some studies that have dealt with issues but used limited samples are: William A. Gamson, "The Fluoridation Dialogue: Is it an Ideological Conflict?," *Public Opinion Quarterly*, 25 (Winter 1961), 526–537; Edgar Litt, "Political Cynicism and Political Futility," *Journal of Politics*, 25 (May, 1963), 312–323; Joel D. Aberbach and Jack L. Walker, "Political Trust and Racial Ideology," *The American Political Science Review*, 64 (December, 1970), 1199–1219.

[11] The sample of respondents for each study is representative of a cross section of eligible voters living in private dwelling units within the continental United States. In 1970 the sample also included 18 to 20 year olds although they were not at the time eligible to vote. In the analyses making comparisons across the four different studies, only eligible voters were used. The 18 to 20 year olds, however, were included in any analysis which dealt with the 1970 data alone. The data were made available by the Inter-university Consortium for Political Research. Neither the Center for Political Studies nor the Consortium bear any responsibility for the analyses or interpretations presented here.

Table 1. Responses to Cynicism Items, 1964–1970

How much of the time do you think you can *trust* the government in Washington to do what is right—*just about always*, *most of the time*, or *only some of the time?*

	1964	1966	1968	1970
Always	14.0%	17.0%	7.5%	6.4%
Most of the Time	62.0	48.0	53.4	47.1
Only Some of the Time[a]	22.0	31.0	37.0	44.2
Don't know	2.0	4.0[b]	2.1	2.3
Total	100.0%	100.0%	100.0%	100.0%
(*N*)[c]	(4658)	(1291)	(1557)	(1514)

Would you say the government is pretty much run by *a few big interests* looking out for themselves or that it is run for the *benefit of all* the people?

	1964	1966	1968	1970
For Benefit of All	64.0%	53.0%	51.8%	40.6%
Few Big Interests[a]	29.0	34.0	39.2	49.6
Other; depends; both checked	4.0	6.0	4.6	5.0
Don't know	3.0	7.0	4.3	4.8
Total	100.0%	100.0%	100.0%	100.0%

Do you think that people in the government waste *a lot* of the money we pay in taxes, waste *some* of it or *don't waste very much of it?*[d]

	1964	1966	1968	1970
Not Much	6.5%	—	4.2%	3.7%
Some	44.5	—	33.1	26.1
A Lot[a]	46.3	—	57.4	68.7
Don't know; not ascertained	2.7	—	5.3	1.5
Total	100.0%		100.0%	100.0%

Do you feel that almost all of the people running the government are smart people who usually *know what they are doing*, or do you think that quite a few of them *don't seem to know what they are doing?*[d]

	1964	1966	1968	1970
Know What They're Doing	68.2%	—	56.2%	51.2%
Don't Know What They're Doing[a]	27.4	—	36.1	44.1
Other; depends	1.9	—	1.8	2.3
Don't know; not ascertained	2.5	—	5.9	2.4
Total	100.0%		100.0%	100.0%

Do you think that *quite a few* of the people running the government are a little crooked, *not very many* are, or do you think *hardly any* of them are crooked at all?[d]

	1964	1966	1968	1970
Hardly Any	18.2%	—	18.4%	15.9%
Not Many	48.4	—	49.3	48.8
Quite a Lot[a]	28.0	—	24.8	31.0
Don't know; not ascertained	5.4	—	7.5	4.3
Total	100.0%		100.0%	100.0%

[a] Indicates response interpreted as "cynical."
[b] Includes 1% coded "It depends."
[c] The sample size for each of the years applies to all five items. The 1964 *N* is weighted.
[d] These items were not included in the 1966 election study interview schedule.

not similarly measured in each of the studies, however, analysis was limited to those issues for which comparable measures existed at different time points.

The five survey questions regularly used in the CPS election studies to measure political cynicism (see Table 1) form a single dimension.[12] They were measured by means of a Guttman scale ranging from least cynical (score of zero) to most cynical (score of five). Whenever reliability of statistical analysis required fairly substantial numbers of respondents in each category, the six categories of the cynicism scale were collapsed into three by combining 0+1, 2+3, and 4+5.

Throughout this paper percentage difference indices will be used in the presentation of data. A percentage difference index (PDI) value indicates the preponderance of one type of response over another at a given time point for a single variable. For example, a trust in government PDI based on the first item of Table 1 may be computed by subtracting the percentage of "most cynical" responses ("only some of the time") from the percentage of "least cynical" responses ("always"). Resulting negative index values indicate a preponderance of "most cynical" responses over "least cynical" responses, while positive index values indicate the reverse. The possible range of PDI values is from +100 to −100. In the specific example for Table 1, the magnitude of the negative value indicates the *degree* of political cynicism; conversely, positive values indicate trust. If all respondents said they could trust the government "only some of the time," the resulting PDI value would be −100, whereas a +100 would result if all respondents said they can "just about always" trust the government to do what is right. The actual values that would result for the item from Table 1 are:

	1964	1966	1968	1970
PDI	−8.0	−14.0	−29.5	−37.8

These PDI values very clearly and parsimoniously display the trend toward increased cynicism previously discussed with respect to Table 1.

Cynicism PDI values can similarly be computed for categories of an independent variable rather than for a single item at different time points. This can be accomplished from the column percentages of a cross-tabulation in which the collapsed three-category political cynicism scale is the row variable and the independent variable (such as one of the racial issues from Table 3) is the column variable. The PDI values for Table 3 were computed by subtracting the percentage of "most cynical" (a score of 4 or 5 on the cynicism scale) from the percentage of "least cynical" (0 and 1 on the cynicism scale) for each category of the issue question. Resulting negative index values indicate degree of cynicism or a preponderance of "most cynical" responses over "least cynical" responses; conversely, positive values indicate degree of trust.

Racial Comparisons. Previous research has suggested the possibility of substantial differences in the attitude of political trust for blacks and whites.[13] Such differences, if they exist, could have confounding effects on other findings; thus racial comparisons are presented at this point. Figure 1, which presents the trust in government PDI based on the first item of Table 1 by race, indeed justifies this comparison as it provides a vivid portrayal of racial differences.[14] Blacks demonstrated more trust in the government than whites prior to 1968, with a sharp divergence and reversal occurring between the races after 1968. During the two-year period from 1964–1966, while blacks were registering more faith in the government, whites exhibited a rapid decline in trust. Since 1966, however, political cynicism has apparently been increasing among blacks and whites, although the rate of change for whites has been at a somewhat slower pace. That this trend does not occur because of some idiosyncratic aspect of the single survey item used in Figure 1 is fully substantiated by similar findings using the full cynicism scale. For example, the percentage of blacks giving cynical responses to all five scale questions in 1970 (40 per cent) was nearly four times the percentage doing so in 1964 (11 per cent). For whites, on the other hand, the percentage of respondents giving cynical answers to all five items between 1964 (11 per cent) and 1970 (20 per cent) had nearly doubled. Such differences in level of political cynicism and rate of change suggest the possibility of several alternative explanations for the highly revealing racial subgroup differences found in the subsequent analysis.

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[12] Factor analysis (with about 20 items for each study) indicated that for each of the four election studies under consideration, the five trust items formed a single dimension. The factor loadings for the five items were all well above .70 and the reproducibility coefficient was greater than .90 in all cases. On the basis of these criteria the five items were judged to satisfy the requirements of a one-dimensional Guttman scale. The scale scores were obtained with the Guttman scale scoring program in the OSIRIS package of social science software. The items were dichotomized as indicated in Table 1; only two items of missing data per individual were allowed; only two errors per individual were allowed; the "median" method of error correction was utilized for scoring error cases when appropriate.

[13] Several studies have dealt explicitly with racial differences and political trust but with conflicting results. Finifter, for example, found that in 1960 blacks manifested a higher degree of normlessness than whites, a finding that runs counter to the results obtained from 1964 SRC data.

[14] The item asking "How often can you trust the government in Washington to do what is right?" was selected for this portion of the analysis because it was one of the two items asked in all four years. It is also very much an "average" item for the set and has the greatest face validity as a measure of political trust. All responses including "Don't Know" were incorporated in the percentage base used to compute the percentage difference index.

Policy Position as an Explanation of Cynicism

The first possible explanation of political cynicism is that individuals are cynical because they prefer an unpopular policy alternative on some political issue or issues. Key[15] has described the distribution of public attitudes on political issues in terms of two general models of consensus and conflict. According to Key, the development of public opinion is from conflict to consensus, with consensus resulting either out of compromise and the movement of individuals to a middle position on the issue or coalescense in favor of one policy alternative. Presumably, the majority opinion would be enacted into policy and, according to the hypothesized relationship, the dissenting minority would fall into the most cynical category because their alternative was not adopted.

Racial Issues. Public policy dealing with racial integration provides a prime subject area for analysis here because the government's policy position, and even more so its rhetoric, can be equated with active enforcement of integration since passage of the 1964 Civil Rights Act. Hypothetically, therefore, individuals in agreement with an integration position should be least cynical while those opposed should be most cynical, leading one to expect a correlation between cynicism and issue position. The correlations (gamma) between political cynicism and racial issues dealing with the role of the government in racial integration, as well as the style and progress of the Civil Rights movement, are displayed in the final column of Table 2.

The difference in the pattern of correlations for 1964 and 1970 is striking. The 1964 correlations are all moderate in size and fit the prediction of the hypothesis. This is true for those items which reflect questions of government action, such as integration of schools and public accommodations, as well as for those concerning the style—whether peaceful or violent—and progress of the Civil Rights movement. For the 1970 data, however, the relationships are very weak.

What are we, therefore, to conclude from the 1970 racial integration items? Had the Civil Rights movement become so integral a part of the American consciousness that a depoliticization of the issue had occurred through the simple acceptance of integration as a way of life, and, more significantly, that even the dissenters were no longer cynical? Probably not, since such an interpretation would completely negate what is readily apparent from even a cursory examination of the racial situation in America today. On the contrary, a much different phenomenon appears to have occurred, altering the basic form of the relationships between political cynicism and racial issues.

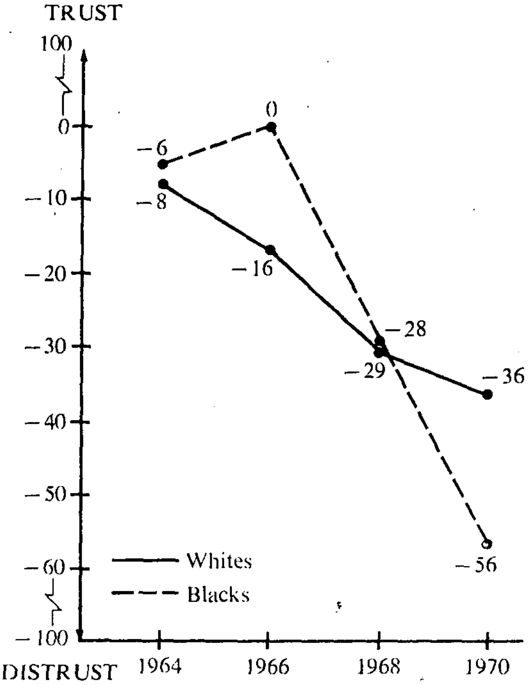

Figure 1: Index of trust in government by race, 1964–1970

The percentage difference indices (PDI) presented in Table 2 more completely describe the relationships between racial issues and cynicism and clearly display the changes which have occurred in this relationship between 1964 and 1970. In 1964, as hypothesized, there was a linear relationship between cynicism and issue position: those opposed to government intervention in race relations were considerably more cynical than those favoring such intervention. For example, in 1964, the 38 per cent of the population believing that integration of schools was not the federal government's business had a PDI value of 25.7; for the 41 per cent in favor of government intervention, it was 57.9; and for the seven per cent favoring a compromise position, 29.3. In 1970, the most cynical respondents were still those who

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[15] V. O. Key, Jr., *Public Opinion and American Democracy* (New York: Alfred A. Knopf, 1965), 27–76.

Table 2. Cynicism Percentage Difference Indices for 1964 and 1970

Do you think the government in Washington should see to it that White and Negro children are allowed to go to the same schools or should the government stay out of this area as it is not its business?[a]

	Same Schools	Other; depends	Stay Out	Gamma
1964 PDI[b]	57.9	29.3	25.7	.32
1970 PDI	−0.6	−1.0	−15.6	.07
Change	(58.5)	(30.3)	(41.3)	

Should the government support the right of Negroes to go to any hotel or restaurant they can afford or should it stay out of this matter?[a]

	Go to Any	Other; depends	Stay Out	Gamma
1964 PDI	55.9	35.6	23.3	.33
1970 PDI	−2.9	3.7	−21.2	.13
Change	(58.8)	(31.9)	(44.5)	

Which of these statements would you agree with: White people have a right to keep Negroes out of their neighborhoods if they want to *or* Negroes have a right to live wherever they can afford to, just like anybody else?[a]

	Whites have right	Don't know; depends	Negroes have right	Gamma
1964 PDI	23.4	50.4	50.3	−.29
1970 PDI	−18.9	−6.7	−0.2	−.15
Change	(42.3)	(57.1)	(50.5)	

Do you think the civil rights leaders are trying to push too fast, are going too slowly, or are they moving about the right speed?[a]

	Too fast	About right	Too slowly	Gamma
1964 PDI	36.1	57.9	48.7	−.22
1970 PDI	−10.8	13.3	−27.3	−.08
Change	(46.9)	(44.6)	(76.0)	

During the past year or so, would you say that most of the actions Negroes have taken to get the things they want have been violent, or have most been peaceful?[a]

	Most peaceful	Some violent; some not	Most violent	Gamma
1964 PDI	32.3	46.1	60.1	−.29
1970 PDI	−12.6	24.5	4.3	−.16
Change	(44.9)	(21.6)	(55.8)	

[a] Full wording of the question can be found in the 1964 or 1970 CPS election study questionnaires.

[b] Negative values of the cynicism PDI indicate degree of cynicism; positive values indicate degree of trust.

opposed government integration of schools (−15.6), but the difference between these individuals and those favoring intervention (−0.6) had narrowed appreciably. Indeed, the difference in degree of cynicism between those favoring a compromise position and those favoring government intervention had disappeared. For the question on integration of public accommodations, the relationship had reversed, with those choosing compromise manifesting the least cynicism.

The shift in cynicism for those opposing and favoring intervention can be roughly estimated by subtracting the 1970 index values from those for 1964. The results of this operation are presented as the "change" values in Table 2. Understandably, "change" in the PDI from 1964 to 1970 is based upon aggregate shifts; statements about individual-level change from such a statistic are necessarily replete with untold hazards. The data obtained from such an index are presented and used here, however, because, in the absence of panel data, they are not only suggestive but also theoretically interesting.[16]

The change in the attitude of trust indicated in Table 2 suggests that the increase in cynicism from 1964 to 1970 for supporters of government intervention was considerably greater than that for opponents of intervention; "change" values are 58.5 and 41.3, respectively. Again, similar patterns of change in cynicism can be observed for the issues dealing with integration of public accommodations and housing integration. In each case, there has been a greater degree of change in the direction of increased cynicism for supporters of government intervention than for opponents of intervention. Likewise, in each case the cynicism seems to have increased at a slower rate among the relatively small subset of the population favoring a compromise position.

Turning to the questions dealing with the style and progress of the Civil Rights movement, a similar pattern of change can be noted. In 1964, the five per cent of the population who thought the movement had been going "too slowly" and the 24 per cent who thought it was going at "about the right speed" were considerably less cynical than the 63 per cent believing that civil rights leaders were pushing "too fast." By 1970, a curvilinear pattern had developed with those (by then 9 per cent of the population) finding the progress of the movement too slow being much more cynical (−27.3) than the 52 per cent thinking it had been going too fast (−10.8); those who felt that the movement was going at about the right speed were still the least cynical (13.3). By far the largest increase in cynicism (indicated in Table 2 by a "change" value of 76.0) occurred among respondents impatient with the progress being made by the Civil Rights movement.

Furthermore, individuals in both 1964 and 1970 who associated actions taken by blacks "to get the things they want" with violence gave evidence of greater cynicism than did those perceiving such behavior as peaceful. In 1964, the difference in degree of cynicism between these two groups was astonishing. Those perceiving the movement as peaceful were highly trusting in their attitude toward the government (60.1), while those who saw black actions as mostly violent were much less so (32.3). By 1970, this difference had greatly narrowed, indicating a much greater increase in cynicism among people perceiving civil rights actions as peaceful. Notably, about two-thirds of the white respondents *in both years* thought the actions of the Civil Rights movement had been violent, whereas about an equal percentage (68%) of blacks felt that they had been peaceful.[17]

In summary, it can be said that consensus had not occurred on racial issues. In 1970, despite a gradual shift in a pro-integrationist direction,[18] attitudes on school integration were still distributed in a bimodal fashion, particularly for whites. There had not been a coalescing of support for enacted policy, as has so often been the case with controversial programs such as, for example, the sweeping social welfare programs of the New Deal era. Nor has the discontent among the dissenters subsided. On the contrary, political cynicism has consistently been most prevalent among those favoring segregation and believing that the federal government should not play a role in the integration of schools and public accommodations. The surprising and unexplained phenomenon disclosed by the data analysis, however, is that individuals in favor of forced integration had also become discontented, and apparently at a faster rate.

A Partial Explanation. A clue to this observed increase in discontent and cynicism among blacks and white integrationists is suggested by the pattern of change presented earlier in Figure 1. Trust in the government may have increased among blacks between 1964 and 1966 out of a reaction to the Civil Rights Bill of 1964; the decrease for whites may also be similarly explained. The Bill may have increased the hope among blacks and white integrationists that a real change in racial integration, along with a reduction in discrimination, would be forthcoming. At the same time, the security of white segregationists might have been threatened by the fear that their social and economic positions would be challenged by blacks with the support of the government. Or, they may have resentfully thought, "The government is helping the blacks, why doesn't it help me?" Such reactions would partially explain the change in cynicism between 1964 and 1966 while setting the stage for the changes which ensued after 1966.

It is suggested that actual modification of the

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[16] It should be noted that a model based upon the replacement of older, less cynical individuals in the population (who have presumably died) with more cynical, newly eligible voters does not explain the general increase in cynicism for the population. This replacement hypothesis was rejected because the data analysis shows that in 1970 for the total population those under 30 years of age were less cynical than those over 60.

[17] For an insightful study of what various people perceive to be violence, see Monica D. Blumenthal, Robert L. Kahn, Frank M. Andrews, and Kendra B. Head, *Justifying Violence: Attitudes of American Men* (Ann Arbor, Michigan: Institute for Social Research, 1972).

[18] Angus Campbell, *White Attitudes Toward Black People* (Ann Arbor, Michigan: Institute for Social Research, 1971).

Table 3. Support for Vietnam Policy, 1964–1970

Which of the following do you think we should do *now* in Vietnam?	1964	1966	1968	1970
Pull out of Vietnam entirely	8.3%	9.1%	19.8%	32.3%
Keep our soldiers in Vietnam but try to end the fighting	25.0	35.6	36.0	32.0
Take a stronger stand even if it means invading North Vietnam	31.6	35.9	33.5	24.2
Other; depends	0.1	2.2	3.3	5.1
Don't know; not ascertained	35.0	17.2	7.4	6.4
Total	100.0%	100.0%	100.0%	100.0%
(*N*)	(4658)[a]	(1291)	(1557)	(1514)
Percentage Difference Index[b]	23.3	26.8	13.7	−8.1

[a] Weighted *N*.
[b] Positive values indicate greater support for escalation; negative values indicate more support for withdrawal.

racial situation and the conditions of racial discrimination have not paralleled the rising expectations of the black community or those sympathetic to their plight, nor at the same time have they been as drastic as feared by white segregationists. Trust in the government may have thus declined sharply among blacks after 1966 because of frustration arising out of the unfulfilled expectation of more active government involvement in the area of integration.[19] Coincidentally, the Civil Rights movement was making blacks, as a whole, increasingly more aware of the real constraints and discrimination existent in the system, thereby increasing their discontent and reaffirming for them even more staunchly the notion that only a change in the system would improve the "black condition."[20] On the other hand, those opposed to integration were also dissatisfied with governmental performance in this area, presumably in part because between 1968 and 1970 the Nixon administration did not attempt to, nor could it, completely reverse the process of integration.

The Vietnam Issue. While significant differences in the degree of cynicism have been found to exist among people favoring alternative policy positions on racial issues, these issues do not completely explain all the increase or the variation in cynicism occurring since 1964. The Vietnam war, an issue with steadily increasing political potency since the early 1960s, appears also to be related to the attitude of trust in government. Table 3 presents the distribution of support for Vietnam policy alternatives from 1964 to 1970. A percentage difference index for the Vietnam issue, computed by subtracting the percentage of respondents favoring withdrawal from the percentage favoring escalation whereby a negative value indicates greater support for withdrawal, is also presented.

The Vietnam index in Table 3 summarizes the trend in attitude among the general population on this issue. This trend toward support for withdrawal certainly comes as no surprise, for it has previously been documented by various polling agencies and publicized extensively in the mass media. However, the *source* of increased support for withdrawal requires more careful attention. (These data, of course, are only suggestive, since the interpretation cannot be termed conclusive without recourse to panel data.) From 1964 to 1970, the percentage of individuals favoring withdrawal from Vietnam increased by 24 per cent while the percentage preferring escalation remained relatively stable, except for an approximate 9 per cent decline from 1968 to 1970. This suggests that most of the increased preference for withdrawal from Vietnam may have come less from converting "hawks" to "doves" than either from attitude changes among those preferring a compromise position or from the uninformed public taking a dovish position as the Vietnam issue became more salient.[21]

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[19] This explanation is given additional support by the degree of change in political trust found for blacks who felt the Civil Rights movement was going too slowly. In 1964, 27 per cent of the blacks fell into this category; six years later, the figure was 39 per cent. The growing impatience on the part of the blacks appears to be reflected in their increased cynicism, as indicated by the respective cynicism index values of 57.8 and −45.2. The magnitude of attitude change suggested by the difference between these two values is extraordinary, particularly for such a basic and presumably stable attitude as political trust. The aggregate increase in distrust of the government was substantially less for the 63 per cent of black respondents who felt that Civil Rights was moving at about the right speed. In 1964, the cynicism index value for these blacks was 53.6—not very different from impatient blacks—but by 1970 the blacks who were satisfied with the speed of the movement (now down to 54 per cent) were very much less cynical (−19.6) as a group than blacks who thought progress was too slow.

[20] Patricia Gurin, Gerald Gurin, Rosina Lao, and Muriel Beattie, "Internal-External Control in the Motivational Dynamics of Negro Youth," *Journal of Social Issues*, 25 (July, 1969), 29–53, investigates whether an individual places the blame for social or economic failure among blacks on the individual or the system.

A model that relates policy dissent with discontent would predict that dissenting "doves" are more cynical than "hawks." Such a relationship would help explain the increase in cynicism occurring between 1964 and 1970, the same period during which the ranks of the "doves" increased. The trust in government (from Figure 1) and Vietnam percentage difference indices were plotted with respect to each other for whites and blacks and are presented in Figure 2.

Figure 2 reveals that the pattern of change in support for withdrawal from Vietnam is very similar to change in trust, especially for blacks who have been consistently more "dovish" than whites. The slight increase in support for escalation for both groups in 1966 probably reflects a leadership effort that rallied early support for Johnson's policy before attitudes on Vietnam became more informed. This would seem to be particularly true for blacks as about 29 per cent of them did not have an opinion on Vietnam in 1966, compared to 16 per cent of the whites. After 1966, however, the black community quickly moved in the direction of support for withdrawal, with only about nine per cent of the total black population in favor of escalation and 50 per cent supporting withdrawal.

The whites, on the other hand, have been more reticent in their support for withdrawal. Furthermore, a closer look at the Vietnam and trust curves for whites reveals certain discrepancies at odds with the original hypothesis. Not only are the Vietnam and trust curves less parallel for whites than for blacks, but there is also a definite divergence of the two curves for whites in 1966. While support for escalation was increasing relative to support for withdrawal among white respondents, trust was declining. Inspection of the relationships between Vietnam policy preference and political cynicism for 1964 and 1970 provides a partial explanation for these discrepancies. Table 4 presents the percentage responses and cynicism index for the Vietnam question for whites only for these years.

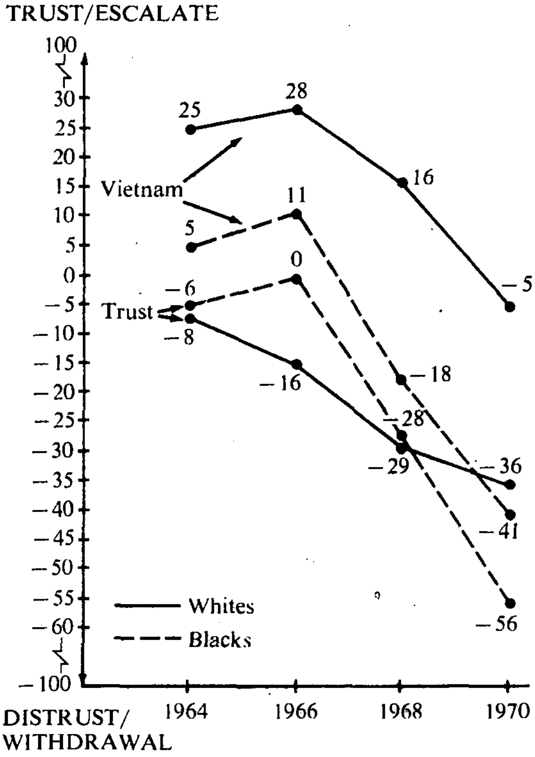

Figure 2: Indices of trust in government and support for withdrawal from Vietnam, 1964–1970

The most immediate observation made from Table 4 is that the original prediction that the most cynical would be those favoring withdrawal is partly false. In 1964, respondents favoring escalation were less trusting of the government (cynicism index value of 27.5) than those preferring withdrawal (37.1); in 1970, the reverse was true (−7.2 and −12.6, respectively). The most striking difference found in Table 4 is not, however, between those supporting withdrawal and those preferring escalation, but rather between the latter two groups and those wanting to keep the soldiers in Vietnam while trying to end the fighting. Respondents choosing this category were definitely less cynical than either the "hawks" or the "doves," both in 1964 and in 1970. Hence, a curvilinear rather than a linear relationship is

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[21] Two pieces of information lend credence to this interpretation. First, the very substantial decrease in the percentage of respondents saying "don't know" corresponds more closely to the percentage increases in the middle and withdrawal categories than it does to the percentage fluctuation found in the escalation category. Second, as Milton Rosenberg, Sidney Verba and Philip E. Converse point out in *Vietnam and the Silent Majority: The Dove's Guide* (New York: Harper & Row, Publishers, 1970), p. 55, it is the "hawks" who have been the more informed on the Vietnam issue; hence, their attitudes on Vietnam would be more crystalized and less susceptible to change. What these data suggest, therefore, is that while there had been a trend toward support of withdrawal from Vietnam, there had not been a concurrent reduction in the polarization of attitudes on Vietnam policy. Continued conflict over Vietnam policy is understandable, given continued dissent among opinion leadership—identified by Richard A. Brody and Sidney A. Verba in "Hawk and Dove: The Search for an Explanation of Vietnam Policy Preferences," *Acta Politica*, 7 (July, 1972), 285–322, as among the most important independent variables in explaining opinions on Vietnam—as well as changes in official policy, a factor which tends to reduce the impact "current policy" would have on a converging policy preference.

Table 4. Current Vietnam Policy Preference in Relation to Trust in Government for Whites, 1964 and 1970

Trust in Government	Vietnam Policy Preference: 1964 Withdraw	Maintain	Escalate
High	58.3%	74.6%	53.8%
Medium	20.4	14.9	19.9
Low	21.3	10.5	26.3
Total	100.0%	100.0%	100.0%
(*N*)	(108)	(315)	(433)
Percentage Difference Index[a]	37.1	64.1	27.5

Trust in Government	Vietnam Policy Preference: 1970 Withdraw	Maintain	Escalate
High	29.5%	46.8%	32.5%
Medium	28.4	28.4	27.8
Low	42.1	24.8	39.7
Total	100.0%	100.0%	100.0%
(*N*)	(437)	(455)	(363)
Percentage Difference Index[a]	−12.6	22.0	−7.2

[a] Negative values of the Cynicism Percentage Difference Index indicate degree of political cynicism; conversely, positive values indicate trust.

found between Vietnam policy preference and political trust, thereby increasing the complexity of the interpretation.

As Rosenberg and others[22] have demonstrated, individuals preferring escalation have often been dissatisfied with Vietnam policy even while a stronger stand was being taken because in their opinion the escalation was not sufficient. Increased dissatisfaction with the intensity of the escalation may explain the increase in cynicism among "hawks" in 1966 and thus the divergence noted in Figure 2. Cynicism among "doves" can similarly be explained on the basis of dissent in 1964 and on the basis of growing impatience in 1970.

The Social Issue. Does the opinion polarization and corresponding curvilinear relationship with cynicism also exist for other contemporary issues? In the 1968 election study, a new type of structured question was used to measure attitudes on contemporary issues.[23] Respondents were presented a series of questions utilizing a seven-point scale running from one policy alternative specified at point "1" to an opposing policy alternative at point "7." (See Appendix.) The 1968 respondents were asked to place themselves and several political leaders (the presidential contenders) on the scale. In 1970, they were asked not only to place themselves and three political leaders (Nixon, Wallace and Muskie) on the scale, but also the Democratic and Republican parties.

The data obtained from these new measures are excitingly more vast than those provided by traditional structured issue questions which elicited the respondent's preference for one of a limited number of alternative policy choices. The new items furnish information not only on the respondent's policy position, but also on measures of agreement between what the respondent would like to see accomplished and what he perceives each of the parties and various political leaders as doing with respect to particular issues. For this reason, greater emphasis is given to those issues comparable over time, as well as to the contemporary problems underlying the issue measures of the 1970 election study.

It was felt that the relationship between these contemporary social problems and the attitude of trust should be investigated to determine if present discontent is related to particular issues only or if it arises out of a general pattern of policy preference.[24] Percentage distributions for the respondents' self-placement on the various issues, along with the cynicism PDI for each issue, are presented in Table 5.

The distributions of policy preference for the eight issues in Table 5 have two basic patterns. Skewed distributions are found for campus demonstrations, government action against inflation, and pollution; the remaining five issues have trimodal distributions. The trimodal distributions disclose a great deal of conflict over policy alternatives. On the average, approximately twice as many respondents preferred the policy alternatives suggested by the extremes of the scales as

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[22] Milton Rosenberg, Philip E. Converse, Sidney Verba, *Vietnam and the Silent Majority;* as well as Philip E. Converse, Warren E. Miller, Jerrold Rusk and Arthur Wolfe, "Continuity and Change in American Politics: Parties and Issues in the 1968 Election," *American Political Science Review*, 63 (December, 1969), 1083–1105.

[23] The new measures were developed by Richard A. Brody and Sidney Verba.

[24] The saliency of the contemporary issues is important, and an attempt was made to assess its impact on the relationships investigated here. The association between political cynicism and the seven-point issue measures was obtained for respondents who mentioned the particular issues as a problem in an open-ended question referring to the most important problems facing the government today. A comparison of the correlations for this presumably "more aware" subset and the remainder of the respondents consistently showed only a *very slight* increase in the correlations for the "more aware." This suggests that although people may not volunteer particular contemporary issues in response to open-ended questions, they are still aware of them.

Table 5. Policy Preference and Cynicism Percentage Difference Indices for 1970

	1	2	3	4	5	6	7	Total
	Solve problems of poverty & unemployment						*Use all available force to quell urban unrest*	
Row %	20.1%	9.5	12.4	26.4	9.5	5.6	16.5	100.0%
PDI[a]	− 5.1	0.0	20.4	3.4	6.1	−10.5	−22.6	(*N*=1535)
	Immediate withdrawal						*Complete military victory*	
Row %	21.7%	8.4	11.0	24.2	8.9	6.4	19.2	100.0%
PDI	−27.7	7.1	7.8	10.3	27.9	10.2	−14.1	(*N*=1522)
	Sympathetic with students & faculty						*Use force to stop campus disturbances*	
Row %	6.2%	5.0	6.0	17.6	10.8	14.5	40.0	100.0%
PDI	−16.5	− 1.4	−15.0	9.3	15.9	15.6	−11.8	(*N*=1459)
	Government help minority groups						*Minority groups help themselves*	
Row %	14.9%	8.3	7.9	25.1	12.1	10.9	20.8	100.0%
PDI	−11.4	7.6	15.2	8.7	16.9	− 4.5	−21.7	(*N*=1419)
	Total government action against inflation						*No government action against inflation*	
Row %	46.4%	20.1	13.7	10.5	3.1	2.2	4.0	100.0%
PDI	− 8.8	5.6	10.7	10.0	− 2.2	19.4	−17.2	(*N*=1433)
	Protect rights of accused						*Stop crime regardless of rights of accused*	
Row %	19.7%	7.9	6.8	17.2	12.4	14.3	21.6	100.0%
PDI	− 2.7	18.5	13.9	8.2	8.9	1.1	−16.4	(*N*=1359)
	Government force private industry to stop polluting						*Industries should handle pollution in their own way*	
Row %	52.2%	13.6	9.0	12.2	3.3	3.3	6.3	100.0%
PDI	− 3.4	14.4	− 6.4	2.4	19.6	− 8.7	−11.5	(*N*=1377)
	Government health insurance plan						*Private health insurance plans*	
Row %	28.4%	8.9	8.2	14.5	6.4	9.7	23.9	100.0%
PDI	−10.0	− 1.7	3.7	− 7.2	21.0	20.0	− 1.9	(*N*=1337)

[a] Negative values of the Cynicism Percentage Difference Index (PDI) indicate degree of political cynicism; conversely, positive values indicate trust.

did those favoring a centrist solution. From 36 to 52 per cent supported the extreme alternatives (#1 and #7) while only 14 to 26 per cent chose a compromise solution (#4) to the five issues. In fact, the combined total of respondents preferring the three center alternatives (#3, #4, #5) never exceeded 50 per cent, a finding which contradicts the notion that a centrist policy alternative to these issues is preferred by a majority of Americans.[25] On the contrary, the distributions suggest a tendency toward bipolarization rather than unanimity.

While the distribution of policy preferences varied over the issues from skewed to multimodal, there was a clear pattern to the relationships between issue position and trust in government. Those individuals preferring the opposing policy alternatives identified with the extreme ends of the scales were, on the whole, more cynical than those placing themselves in the center for each item in Table 5. Thus, the curvilinearity found in the relationships between policy preference on the Vietnam and civil rights issues and trust in government is also found for the social and economic issues of Table 5, regardless of the actual distribution of policy preferences.

The preceding analysis clearly demonstrates that no monolithic description of cynicism arises out of policy preferences for the issues investigated. Rather, what evolves is that those in the center were the least cynical, and that the most cynical formed two ideologically distinct types:

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[25] Richard M. Scammon and Ben J. Wattenberg, *The Real Majority* (New York: Coward, McCann and Geohegan, Inc., 1971). The implications of their major premise—that a presidential candidate must move to the center in order to win—is that a centrist policy is preferred by a majority of the voters.

"cynics of the left" and "cynics of the right." Operationally defining "cynics of the left" and "cynics of the right" as those both "most" cynical (#4 or #5 on the cynicism scale) and preferring the respective policy alternatives #1 through #3 and #5 through #7 on the five most highly clustered (interitem Pearson *r* ranging from .20 to .50) of the eight issues, allows for a refined comparison of the two groups. The five issues included in the cluster were: urban unrest, Vietnam, campus demonstrations, protecting the rights of the accused, and government aid to minorities. Such an operational definition provides fairly pure types of cynics, with 11 per cent of the population ($N=176$) classified as "most" cynical on the left and 19 per cent ($N=290$) on the right.

"Cynics of the left" may be described as predisposed to social and political change. Witness their support of policy alternatives indicating that solutions to contemporary social problems lie in the alteration of the existing system: the prevention of riots in cities by solving the problems of unemployment and poverty, government aid to improve the social and economic conditions of minority groups, a pro-integrationist stand on civil rights, and protection of the rights of the accused. This predisposition is supported by their apparent belief that constraints in the system, rather than individual shortcomings, are to blame for the failure of people, particularly minority groups, to "get ahead" in society. This system-blame attitude is evident in their strong preference for government action to improve the social and economic conditions of minority groups as well as that of all those who suffer from poverty and unemployment. As is further suggested by their responses to the student unrest issue, they appear to be somewhat favorably inclined toward the use of collective action for obtaining that change; hence, they are sympathetic with demonstrating students.

"Cynics of the right," may, on the other hand, be described as more predisposed toward social control[26] than social change. That is, they are bent upon maintaining the system as it exists and support authorities and policies which act to control those who may be potentially disruptive of the orderly functioning of the system. They are inclined toward segregation policies and policies tending to disregard the rights of the accused in an attempt to prevent crime, and are strongly in favor of complete military victory in Vietnam. The Protestant Ethic seems engrained in "cynics of the right" as evidenced by their greater stress on the individual rather than the system for one's failure to attain social status; they were firmly against government assistance aimed at improving the social and economic position of minority groups. Their corresponding support for using police force against demonstrators suggests that since they apparently believed that unsuccessful attempts at social advancement were individual shortcomings, such disruptive actions were attempts by a few to gain special privileges for themselves.

The two types of cynics were as different demographically as they were attitudinally. One-third of the "cynics of the left" were under 30 whereas only 12 per cent of the most cynical on the right were young. Blacks comprised 38 per cent of the "cynics of the left"; 99.7 per cent of the "cynics of the right" were white. While "cynics of the left" tended to identify more with the Democratic (71 per cent) than the Republican party (12 per cent), those on the right were more evenly split: 48 per cent were Democrats and 35 per cent were Republicans. The "cynics of the right" had higher incomes, with only 21 per cent making less than $4,000 a year; 38 per cent of those on the left were below that figure. Those on the left, however, were much better educated: 28 per cent had some college training or held a college degree, as compared with only 18 per cent among those of the right. Again, with blacks removed, the percentage of those on the left who had had some college rose to 38 per cent.

Alternatively, the curvilinearity in the relationship between policy preference and trust may be explained in retrospect by the increased political awareness and the type of issues prevalent today rather than by the distribution of preferences. It may be suggested that these issues, unlike those of earlier years that were more removed from an individual's immediate experience, seem to affect the very basic values and fears that form the foundation of everyday existence for the average citizen. Attitudes on these issues would, therefore, tend to be more crystallized and more firmly held than on those more remote from an individual's daily existence. Thus, it would be understandable that respondents with the most completely formed attitudes—presumably those favoring the extreme alternatives[27]—should be the most cynical. It is they who would be most dissatisfied were their well-formulated policy expectations not only left untended but also threatened by enactment of opposing policies. This "new issues" hypothesis is, however, considerably more complex an explanation of the observed curvilinearity than the "distance from current policy" interpretation. Fortunately, a comparison of the "new issues" versus the "all policy" hypotheses can be parsimoniously arranged: Consider two indices of policy distance, that is, stated preference for policy different than current—the one composed of responses to all policy items; the other of only the new issues (Vietnam, youth protest, and rights of the accused). If the newer, more meaningful, and thus more "crystallized" issues hypothesis is correct, only that index should display the requisite curvilinearity. On the other hand, if the "all issues" index shows the same curvature, the "poor-performance/dissatisfaction/cynicism" hypothesis would seem to be affirmed. The results of such a comparison do indeed demonstrate a marked curvilinearity for both indices, as well as a third index which incorporates all issues except the "new issues." These results would, therefore, seem to substantiate the hypothesis that the curvilinearity arises out of a reaction to current policy rather than a qualitative difference between contemporary and past issues.

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[26] For a definition of social control, see Gamson, *Power and Discontent*, pp. 11–19.

[27] Angus Campbell, Philip E. Converse, Warren E. Miller and Donald E. Stokes, *The American Voter* (New York: John Wiley & Sons, Inc., 1960), p. 178.

By 1970, Americans to a considerable degree had withdrawn some of their trust from the government because they had become widely divided on a variety of issues, for in the normal attempt to satisfy the greatest numbers, the government had generally followed a more or less centrist policy which in reality appears to have displeased a substantial proportion of the population. Issue position was, therefore, found to be related, in a complex manner, to trust in government. There is, however, nothing intrinsic in support for a particular policy alternative, or even a pattern of policy preferences, which provides a theoretical interpretation for such a finding. It was hypothesized that dissenters, that is, those favoring the policy alternative endorsed by a minority of individuals, would be the most dissatisfied and thereby the most cynical. This hypothesis proved to be inadequate in explaining either the widespread distrust of government in 1970 or the increase in distrust occurring between 1964 and 1970, because often the majority favored opposing policy alternatives and was more cynical than the minority preferring the centrist policy. Whenever an explanation was offered for the findings, it was couched in terms of expectations and policy dissatisfaction. Such an interpretation cannot, however, be tested by the data analyzed thus far. To test such explanations of political cynicism directly, the concepts of policy expectation and dissatisfaction need to be clearly operationalized and incorporated in hypotheses. Two further hypotheses which may explain the relationship between cynicism and the issues are:

(1) Cynicism arises because individuals do not perceive any difference between the policy alternative offered by the Democratic and Republican parties or their leaders.

(2) Cynicism is a result of dissatisfaction with the expected or perceived policy performance of the party currently in control of the Presidency.

Lack of Party Differences

Anthony Downs in *An Economic Theory of Democracy* contends that a rational voter casts his ballot for the political party which will provide him with "a higher utility income than any other party during the coming election period"; should the voter not feel that he would benefit relatively more from either party, he abstains.[28] Rational abstention because of a lack of perceived difference between the candidates or parties suggests the possibility that alienation, and hence political cynicism, may, in part, be related to a perceived lack of choice between the parties.

The seven-point issue scales from the 1970 election survey provide excellent measures for determining the degree of perceived party difference on the issues. As noted, respondents were asked to place both parties on each issue continuum; perceived difference between the parties can thus be computed as $|\text{Democratic}_j - \text{Republican}_j|$, that is, as the absolute difference between scale placements for the Democratic and Republican parties on the scale for issue "j" ($j=1$ to 8). The values for the computed difference range from 0, implying no difference, to 6, maximum difference.

The percentage of the population perceiving no difference between the parties in 1970 varied from 33 to 53 per cent, depending on the issue. The number perceiving no difference was greater than 40 per cent on only two: pollution and protecting the rights of those accused of crime. On the remaining six issues, 61 to 67 per cent of the population perceived some difference between the parties. Republicans tended to perceive slightly more difference between the parties than did Democrats, whereas Independents (Independent as defined here and throughout this article does not include those leaning toward either party) perceived the least difference, with 50 to 69 per cent perceiving no difference at all between the parties.[29]

The data analysis reveals that a fairly substantial degree of difference is, in fact, perceived between the parties. This does not, however, discount the possibility that political cynicism increases as perceived difference between the parties decreases. If such were the case, a negative correlation would be found between political cynicism and perceived difference. The predicted inverse relationship between cynicism and a measure of the average perceived party difference for the eight issues is found, however, only for Independents, and then to a very slight degree ($r = -.10$). There is a near-zero relationship between cynicism and perceived party difference for Republicans ($r = .03$), a weak positive correlation for white Democrats ($r = .14$), and a slightly stronger positive relationship for black Democrats ($r = .20$). Only among Independents, therefore, can cynicism possibly be attributed to a lack of perceived difference between the parties; the correlation is so small, though, that such a conclusion must be tenuous.

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[28] Anthony Downs, *An Economic Theory of Democracy* (New York: Harper & Row, Publishers, 1957), p. 39.

[29] On the whole, the Democratic party was placed more toward the left end of the issue scales, while the Republican party was perceived as endorsing policy alternatives found at the right end. Only 9 to 25 per cent (with an average of approximately 15 per cent for the eight issues) of those who were able to place both parties on the issue scales perceived the Democratic party to the right of the Republican party.

Nevertheless, it is interesting to note that Independents may feel less trusting of the government because they believe they are not given an adequate choice, whereas Democrats, and particularly black Democrats, who perceive a greater difference between the parties and hence presumably a clearer choice, are somewhat more cynical than Democrats who perceive less difference. What these correlations suggest is that while Independents are cynical because of a lack of choice arising out of the absence of party differences, blacks are cynical because they perceive the policy gap between the Democratic and Republican parties as so large that the Republican party is not a viable alternative, thus, ironically, also resulting in a lack of choice for them.

On the other hand, the positive relationship between cynicism and perceived party difference found for Democrats may be linked to dissatisfaction with the policies of the Republican administration which was in power in 1970. It may be presumed that Democrats would prefer Democratic to Republican party policies, and that Democrats who perceive the greatest difference between the two parties would be the most dissatisfied because their policy preferences were least likely to be enacted. Cynicism would thus be expected to be greatest among those perceiving the largest differences between the Republican and Democratic parties. A more complete investigation of alternative explanations for the positive relationship between cynicism and perceived party differences is presented in the following sections. It can be said here, however, that cynicism among Americans in 1970 was not a function of perceiving little difference between policy alternatives identified with either political party. On the contrary, most people perceived substantial differences between the parties on the issues, and, indeed, there was a slight tendency for those who perceived the largest differences to be more cynical. This trend suggests that cynicism may arise out of dissatisfaction with the policy alternatives offered by one's own party and the belief that the other party is not a viable alternative.

Policy Dissatisfaction

Gamson contends that distrust of the government arises out of "the nature of the decisions made and the satisfaction or dissatisfaction with them . . . ,"[30] particularly when dissatisfaction is generalized across a number of decisions which result in undesirable outcomes. It may be hypothesized, therefore, that discontent is related to dissatisfaction with the perceived policies of the party in power on issues that are relevant to the values of a substantial segment of the population.

Data used to test the hypotheses in this section come almost exclusively from the eight 1970 election study seven-point issue questions which provide measures for operationalizing the concept of policy dissatisfaction over a set of issues in a more complete manner than the issue questions of earlier studies. The 1964 and 1968 election studies have been used, however, to validate the findings obtained from the 1970 data. Separate analyses of the earlier surveys demonstrated that the generalizations arising out of the 1970 data are, within limits of the available measures, indeed substantiated.

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[30] Gamson, *Power and Discontent*, p. 178. Gamson provides a general theoretical notion of the process underlying the development of political distrust. The dissatisfaction begins to be generalized when an undesirable outcome is seen as a member of a class of decisions with similar results. Authorities represent the first target of such generalization:

> If such experiences extend over more than one set of authorities, potential partisans may conclude that the institutions themselves may be the source of bias, and "throwing the rascals out" will have little effect if indeed it is even possible. . . . Attacks on political institutions may in turn lead to distrust in the ideology or public philosophy used to justify them. Finally . . . the disaffection may be generalized to the political community itself and a desire for political separation may develop.

Gamson's developmental model does not, however, incorporate a concern with cause and effect or the intricate set of interactions which may occur over time between policy dissatisfaction, distrust and social or historical events. Declining levels of political trust may be the result of increased policy dissatisfaction, or it may be the other way around, or there might be a further independent variable responsible for the decline in both these variables. Cynicism may, for example, be considered in a very generalized form to be a personality attribute that would condition one's perceptions of the world, and one of its effects might be to cause a person to perceive a greater than normal distance between his preferences and what was being done by political authority to satisfy these preferences. Some societal factor may be causing an increase in political cynicism, which in turn acts as an intervening variable to bring about a rise in dissatisfaction with government policies. Alternatively, cynicism and dissatisfaction could both be connected with some changing social condition and therefore be only spuriously related to each other. These are alternative interpretations for trends that show declining trust in government to be related to policy dissatisfaction and they are worthy of further exploration. Unfortunately, they go beyond the scope of this report and would require the analysis of panel data before any conclusive separation of cause and effect could be accomplished.

Policy dissatisfaction has been operationalized here in terms of the distance between the individual's own policy preference and the policy alternative that he identified with a particular party. Underlying this is the assumption that if an individual completely agrees with the policy alternative of a particular party as he perceives it (that is, when a respondent places both himself and the party at the same point on a scale), he is more satisfied with the party's policies than he would be if he identified the party with a policy some distance from his own position on the issue. It should be emphasized that this is either perceived or expected policy and not necessarily an alternative which the parties have already enacted. Similarly, that perception is, of course, unique to the individual respondent.

An indicator of policy dissatisfaction on issue "*j*" can be computed as the absolute difference between the individual's (*i*) issue scale value (i_j) and the scale value associated with the position where the respondent has placed the party (p_j). More formally, this distance measure can be represented as $d_{ip} = |i_j - p_j|$, with a range of scores from 0, minimum policy dissatisfaction, to 6, maximum policy dissatisfaction.

An average measure of policy dissatisfaction, as indicated by a mean distance score for the eight issues, was used in the analysis because this was more parsimonious than using the eight issues separately, and because it captures the effects of dissatisfaction that may be cumulative across the several issues. Using an average measure, however, posed two disadvantages: the average measure correlates at a somewhat lower value with the political cynicism scale than does the multiple correlation of the eight individual measures; and it does not answer the interesting question of the relative strength of the relationship between cynicism and any particular issue. Unfortunately, the scope of this paper does not allow for an explication of the relationship with the full set of issues nor for an unraveling of the cause and effect relationship between cynicism and policy dissatisfaction. Prior cynicism may possibly have an impact on whether or not a policy is perceived in an unfavorable fashion and this may in turn increase cynicism even more. The data used here, however, do not permit a conclusive determination of this causal sequence.

Dissatisfaction with the Republican Administration. Data which test the hypothesis that cynicism is related to dissatisfaction with the perceived policies of the party in power are presented in Table 6.[31] The table displays the mean cynicism score by distance from the Republican party on the eight issues for the total population, white and black Democrats, Republicans, and Independents. The percentage distributions for each group on the policy dissatisfaction measure (distance from Republican party) are also presented.

It is apparent from the means for the total population that a substantial increase in cynicism accompanies increased dissatisfaction with Republican party policies on the issues. The mean level of cynicism for the 13 per cent of the population least dissatisfied with Republican policy was 1.79, whereas the two per cent most dissatisfied had a mean of 4.00. Recalling that the cynicism scale values range from 0, for least cynical, to 5, for most cynical, helps place this difference in perspective.

One of the more interesting results found in Table 6 is that the relationship between policy dissatisfaction and cynicism holds for Republicans, as well as for Democrats. Indeed, the close similarity between the group mean cynicism value for Republicans (2.28) and white Democrats (2.50) demonstrates that distrust of the government is not related to party identification but to policy dissatisfaction. As expected, Republicans were, on the whole, less dissatisfied than Democrats; only about 26 per cent of the Republicans placed the party, on the average, two or more issue points away from their own policy preference, whereas 63 per cent of all Democrats did so. Still, as policy dissatisfaction increases among Republicans, trust in the government declines significantly.

It must be emphasized that these data are for 1970 only. Comparable distance measures did not exist prior to 1970; thus, the hypotheses tested here cannot be compared with data from earlier years. The single snapshot resulting from the 1970 data should not, of course, be unthinkingly generalized to all other years. Some cynicism may, indeed, arise from reaction to the "in" party, and under some circumstances party identification may have a significant, linear relationship with

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[31] Despite the fact that respondents had also been asked to place President Nixon on the scale, the following analysis deals with policies perceived to be associated with the political parties only. That no particular Democratic leader was fully recognized as the counterpart to the President precluded doing analysis with political leaders. It should, however, be noted that there was for the total population an extremely close correspondence between where the Republican party and Nixon were placed on the issue scales and analysis using either one provided virtually identical results.

Table 6. Mean Cynicism by Distance from Republican Party, 1970

Distance from Republican Party	Total Cross-section		White Democrats		Black Democrats		Republicans		Independents	
	Mean[a]	%	Mean	%	Mean	%	Mean	%	Mean	%
0	1.79	(13.1)	1.42	(7.8)	1.33	(6.1)	1.89	(23.8)	2.64	(8.5)
1	2.27	(36.9)	2.14	(32.9)	2.09	(10.6)	2.25	(50.1)	2.43	(35.4)
2	2.66	(30.0)	2.60	(36.0)	2.63	(27.2)	2.57	(20.3)	3.03	(39.7)
3	3.10	(13.4)	3.06	(16.9)	3.44	(27.3)	2.65	(4.3)	3.67	(11.0)
4	3.72	(4.5)	3.25	(4.6)	4.32	(18.7)	4.57	(1.5)	3.88	(5.4)
5	4.00	(2.1)	4.00	(1.8)	4.00	(10.1)	b	—	b	—
6	b	—	b	—	b	—	b	—	b	—
Group Mean and Total Per cent	2.53	100.0	2.50	100.0	3.10	100.0	2.28	100.0	2.90	100.0
(*N*)	(1414)[c]		(646)		(210)[c]		(463)		(164)	
Eta	.28		.28		.44		.22		.27	

[a] Cell entries are mean cynicism values. The cynicism scale ranged from 0 = least cynical to 5 = most cynical.

[b] *N* was less than 5.

[c] To avoid the problems of weighted data, the black supplement was not included in the cross-section analysis. However, the cross-section and supplement blacks form a self-weighting sample of blacks, and supplement blacks were therefore included among black Democrats.

trust in government. That such a relationship is not derivable from the data for any of the election studies prior to 1970 does not eliminate the possibility that such a relationship could develop. In fact, the policy dissatisfaction theory argued in this paper would lead to expectations of a linear relationship between cynicism and party identification under certain conditions. This pattern can be expected, for example, when the policies that are most strongly related to political discontent over time are also strongly aligned with party identification. Thus, if Republicans control the administration and Democrats believe Republican policies to primarily benefit certain types of individuals who may also happen to be Republican, then a correspondence between policy dissatisfaction, party identification and cynicism can be expected. When the policies that are correlated with discontent over time are orthogonal to party identification and partisan cleavages, however, the relationship becomes curvilinear and weaker. As the dynamics of a spiral of discontent come into play, the resulting cynicism may also affect perceived policy performance on more traditional issues, and this in turn may affect party identification itself. Unfortunately, the testing of these dynamic hypotheses cannot be accomplished with the data presented here.

The strongest relationship between dissatisfaction with Republican policy and political cynicism was found for black Democrats. They were more dissatisfied with Republican policy than any of the groups found in Table 6. This dissatisfaction is understandable, given the nature of contemporary issues. Nearly all deal with problems that impinge more directly on the lives of black people who face the constraints in the present system daily and who surely want social change more than do whites.

Furthermore, a comparison of data from 1968 and 1970 for the two seven-point issue scales used in both those studies—urban unrest and Vietnam—reveals that blacks perceived Nixon as somewhat more "hawkish" in 1970 than they did in 1968 and similarly more in favor of using "police force" in the prevention of urban unrest. Thus, even if blacks had no expectations for improvements in their social condition under the Republican administration elected in 1968, these data suggest that cynicism among blacks would still increase because they apparently perceived their situation as deteriorating under the Republicans.

A noteworthy relationship between cynicism and policy dissatisfaction was also found among Independents. As a group, they were somewhat more distrusting of the government than either Democrats or Republicans were; however, they were less cynical than black Democrats. Independents, on the whole, were less dissatisfied with Republican policy alternatives than were Democrats: 56 per cent were two or more issue points from where they placed the Republican party, but only a small number were beyond four units from the party.

Dissatisfaction with Democratic Policies. Are we to conclude from the data of Table 6 that discontent in America would be reduced by replacing the Republican administration and Republican policy alternatives with the Democratic alternative? Not necessarily. Data describing the relationship between dissatisfaction with Democratic party policies and political cynicism are presented in Table 7. The data in Table 7 clearly demonstrate that in 1970 a noteworthy proportion of the population was also dissatisfied with the policy alternatives they perceived the Democratic party as pursuing to solve the problems reflected by contemporary issues. Nearly 60 per cent of the population fell into the two "least dissatisfied" categories (0 and 1) of Table 7, compared to 50 per cent who were equally satisfied with Republican policies. Thus, while there was somewhat less dissatisfaction, in general, with Democratic policies, a sizable fraction of the population (40 per cent) was still somewhat in disagreement with perceived Democratic policy alternatives. All of the dissatisfaction was not found among Republicans either; on the contrary, as a group they were slightly more in agreement with Democratic policies than Democrats were with Republican policies. The distributions of Independents on the measure of dissatisfaction with Democrats and Republicans are almost identical, indicating that Independents, as a group, were equally dissatisfied with both parties. This finding for Independents supports the earlier result which suggested that Independents may be cynical, to a certain extent, because they perceive no difference between the policies of the two parties. Black Democrats, in contrast, exhibited much more policy agreement with the Democratic than the Republican party. Almost 47 per cent of black Democrats thought the Democratic party would pursue policies which they themselves preferred.

Despite this relatively low degree of dissatisfaction with Democratic policies, those blacks who were most dissatisfied were definitely more cynical than those who were in complete agreement with Democratic policies. Indeed, the relationship between cynicism and dissatisfaction with Democratic policy was stronger for black Democrats than for any other group studied, even Republicans. For all the groups, except Republicans, it was somewhat weaker than found in Table 7 for the Republican party. The relationship for Republicans was almost identical in magnitude for both cases: an eta of .22 was obtained in Table 6 and .24 in Table 7.

The data in Tables 6 and 7 demonstrate emphatically that distrust of the government in 1970 was related not only to dissatisfaction with the policies of the party in control of the federal government's administration, but also to dissatisfaction with the policy alternatives identified with the Democratic party. Dissatisfaction with the Republican party was, to a certain extent, correlated with dissatisfaction with the Democratic party, thus suggesting that distrust of the government was related to *dissatisfaction with both parties*.

Data which directly test the hypothesized relationship between dissatisfaction with both parties and cynicism is presented in Table 8. The table displays a 25-celled cross-classification of the total cross-section sample determined by distance from both the Republican and Democratic parties' perceived policies. Categories 4 through 6 of the distance-from-party measures used in Tables 6 and 7 were collapsed for the construction of Table 8 so that the number of cases in the cell entries would be sufficiently large for computing the mean cynicism value for each of the groups in the data array.[32] The data entries are the mean cynicism values for the cases of the given cell. The percentage of the total population falling into the particular cell is indicated in parentheses, and these percentages total to the corner entry.

The results of Table 8 clearly demonstrate the

Table 7. Mean Cynicism by Distance from Democratic Party, 1970

Distance from Democratic Party	Total Cross-section		White Democrats		Black Democrats		Republicans		Independents	
	Mean[a]	%	Mean	%	Mean	%	Mean	%	Mean	%
0	2.16	(21.2)	2.17	(26.4)	2.47	(46.7)	1.82	(9.8)	2.47	(11.7)
1	2.42	(38.6)	2.43	(44.5)	3.50	(39.2)	1.89	(33.3)	2.71	(31.3)
2	2.71	(26.3)	2.71	(22.2)	4.04	(14.1)	2.42	(31.9)	3.02	(37.4)
3	2.88	(9.7)	3.43	(5.5)	[b]	—	2.50	(16.6)	3.00	(12.9)
4	3.15	(3.3)	3.70	(1.4)	[b]	—	3.03	(6.6)	3.20	(6.7)
5	3.85	(0.9)	[b]	—	[b]	—	4.00	(1.8)	[b]	—
6	[b]	—	[b]	—	[b]	—	[b]	—	[b]	—
Group Mean and Total Per cent	2.53	100.0	2.50	100.0	3.10	100.0	2.28	100.0	2.87	100.0
(*N*)	(1412)[c]		(651)		(212)[c]		(457)		(163)	
Eta	.16		.19		.31		.24		.12	

[a] *Cell entries are mean cynicism values. The cynicism scale ranged from 0 = least cynical to 5 = most cynical.*

[b] *N* was less than 5.

[c] To avoid the problems of weighted data, the black supplement was not included in the cross-section analysis. However, the cross-section and supplement blacks form a self-weighting sample of blacks, and supplement blacks were therefore included among black Democrats.

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[32] The basic relationship between distance from both parties and cynicism is left unchanged by collapsing categories 4 through 6 of the original measures. The eta for the correlation with the full set of values was .33 as compared with .32 for the collapsed measures. Further analysis of the type presented in Table 8 but with various subgroups of the population was not possible because of the limited number of cases.

Table 8. Mean Cynicism by Distance From Both Parties, 1970

Distance from Republican Party	Distance from Democratic Party					Total
	0	1	2	3	4	
0	1.69[a] (5.9%)	1.60 (4.1)	2.50 (1.7)	1.50 (0.6)	2.10 (0.7)	1.79 (13.0%)
1	1.84 (6.1)	2.19 (18.2)	2.56 (8.4)	2.33 (3.0)	2.85 (0.9)	2.27 (36.6)
2	2.19 (4.6)	2.71 (9.8)	2.67 (11.4)	3.00 (3.1)	3.24 (1.2)	2.66 (30.1)
3	2.79 (2.4)	3.25 (4.2)	2.88 (3.5)	3.46 (2.5)	3.50 (0.6)	3.10 (13.2)
4	3.46 (2.0)	3.10 (2.1)	4.40 (1.1)	4.37 (0.9)	4.69 (1.0)	3.81 (7.1)
Total	2.16 (21.0%) Eta = .32	2.42 (38.4)	2.71 (26.1)	2.88 (10.1)	3.29 (4.4)	2.53 (100.0%) $N = 1401$

[a] Cell entries are mean cynicism values. The cynicism scale ranged from 0 = least to 5 = most.

degree to which distrust of the government increases as dissatisfaction with the policies of both parties increases. Those individuals who were completely satisfied with the perceived policies of the Democratic and Republican parties (cell 0,0) were the most trusting of the government (mean cynicism of 1.69), whereas those who were most dissatisfied with both parties (cell 4,4) were dramatically more cynical (mean of 4.69). It should be noted that while every column of Table 8 shows a monotonic increase in mean cynicism, every row breaks monotonicity. This substantiates what was previously observed from Tables 6 and 7, namely, that cynicism was, for the total population, somewhat more directly related to distance from the Republican party than distance from the Democratic party. The non-monotonicity of the row entries of Table 8 is greatly reduced, but is slightly apparent, when blacks and Independents are removed from the analysis. Yet for each subset analyzed, the diagonal entries of Table 8 were always strictly monotonic. Also, the difference in mean level of cynicism for those most satisfied with both parties and those most dissatisfied with both parties was always greater than the comparable difference between any of the sets of individuals most and least satisfied with the policies of any one particular party. These data thus provide strong evidence that those who feel that *neither* party offers viable solutions to contemporary social problems are among the most cynical, distrustful, and alienated citizens in the U.S. today.

Policy Dissatisfaction and Cynicism: An Explanation. How is the relationship between policy dissatisfaction with both parties and political cynicism to be explained? And how does this finding correspond to the increase in cynicism between 1964 and 1970? A possible explanation, inspired by the concepts of party identification and relative deprivation, is that Democrats would tend to be dissatisfied with Republican policies, and thereby more distrusting of the government, because they do not believe that Republican policies benefit them. This party-based interpretation would, however, lead to the prediction of no correlation between cynicism and Democratic policies, a prediction obviously not supported by the data. Another interpretation which would seem theoretically reasonable, but similarly unsupported by these data, is that a negligible relationship between cynicism and dissatisfaction with Republican policy would be found for Democrats because their expectations during a Republican administration would be low and policy dissatisfaction would thus not lead to increased cynicism. Perhaps the reason these explanations do not fit the data is that contemporary social issues are not closely related to party identification, and what appears confusing when approached from a party focus becomes more meaningful from another vantage point.

An investigation of policy dissatisfaction for "cynics of the left" and "cynics of the right" produces a partial reduction in the confusion and a clearer explanation. It appears that the Democratic "cynics of the left" ($N = 122$) were dissatisfied with the Democratic and Republican parties because, to a certain extent, they perceived both of them as being too far to the right: 38 per cent placed the Democratic party further to the right than their own policy preference and nearly 96 per cent saw the Republican party as more conservative on the issues than they were themselves. Their average distance from the Democratic party was 1.4, from the Republican party, 3.2, thus illustrating a greater degree of discontent with Republican than Democratic policies. The Democratic "cynics of the right" ($N=138$), however, were dissatisfied with *both* the Republican and Democratic policies equally: 74 per cent perceived both parties as further left than their own policy preference. On the average, the Democratic "cynics of the right" placed the Democratic party 1.9 units away from their own policy position, while their mean distance from the Republican party was 2.4.

That dissatisfaction with Democratic and Republican policies coincides so closely for Democratic "cynics of the right," unlike that for those on the left, may partially be explained by the possibility that they were less well informed, as is suggested by the fact that they were not as well educated as "cynics of the left." The importance of this explanation is somewhat reduced, however, by the finding that Republican "cynics of the left," who were, on the whole, very well educated, were also equally dissatisfied with both parties. Their average distance from the Democratic party was 1.8, compared to 1.6 from the Republican party. The Republican "cynics of the left" perceived Republican party policies as somewhat more conservative than their own preferences but very centrist; on the average, 61 per cent placed the party at point 4 on the issues. Surprisingly, they perceived Democratic policies as somewhat more to the right than Republican policies. Unfortunately, there were only 25 respondents so classified, thus making inferences about Republican "cynics of the left" rather tenuous. In contrast, Republican "cynics of the right" ($N=102$) were more dissatisfied with Democratic party policies (average distance of 2.7) which they perceived as being further left than Republican policies. Nevertheless, 57 per cent placed the Republican party further to the left (average distance of 1.8) than their own policy preference, implying a distrust for the government because of too liberal administration policies.

In short, both Democratic and Republican "cynics of the right" perceived partisan solutions to contemporary social problems as too liberal. It is important to add that individuals preferring centrist policy alternatives, regardless of whether "centrist" is defined as points 3 to 5 or only 4 on the scale, were proportionately more likely to perceive both the parties in the center; hence they were less dissatisfied and correspondingly less alienated. Besides demonstrating which Democratic and Republican subgroups were dissatisfied more with one party than the other, or with both equally, these data support a reformulated explanation of how policy dissatisfaction corresponds to the recent increase in political distrust.

When approached from the left-right cynicism perspective, the data strongly imply that the increased distrust of government in 1970 was for some the result of a combination of unfulfilled expectations, and for others, the perception that their situation, which was already one of deprivation and discrimination, had actually worsened relative to that of others. For example, in 1968 Richard Nixon promised to end the war, but respondents in that year's study who perceived him as tending toward a policy of complete military victory outnumbered, by 2 to 1, those who saw him as favoring withdrawal. By 1970, U.S. involvement in Vietnam had not ended, and Vietnamization—a centrist approach to the war—had been declared the official policy of the Republican administration. Furthermore, a larger proportion (34 per cent) of the "cynics of the right," who on the whole were very "hawkish," perceived Nixon as favoring withdrawal (points 1–3 on the Vietnam issue scale) in 1970 than did in 1968 (23 per cent). In contrast, the most "dovish" of the "doves," the blacks, perceived Nixon, despite his promises to end the war, as more in favor of a complete military victory in 1970 (52 per cent placed him at points 5–7) than they did in 1968 (42 per cent). Thus, the expectations of both "hawks" and "doves," regardless of their party identification, had not been met, resulting in a decline in confidence for both groups.

The failure of the Republican administration to meet the expectations of the Democratic "cynics of the right" may have led them to the view that 'the Republicans are just as bad as the Democrats." Such an interpretation would coincide well with the close correspondence between dissatisfaction with Democratic policy and dissatisfaction with Republican policy that was found earlier for Democratic "cynics of the right." It would also explain why Republican "cynics of the right" were dissatisfied with Republican policies. However, their party identification, which was somewhat stronger than that of the Democratic "cynics of the right," would be expected to keep them from perceiving Republican and Democratic policies as identical; exactly what the analysis demonstrates.

Similarly, the combined effects of party identification, unmet expectations, and the belief that social and economic conditions had deteriorated under the Republican administration can also explain increased cynicism among those on the left. Further, the relationship found between distrust of the government and dissatisfaction with the policies of both parties suggests that discontent in

1970 was related to an earlier dissatisfaction with the policies of the previous (Democratic) administration. Indeed, analyses from 1964 to 1968 support this contention and reveal a relationship between policy dissatisfaction and cynicism for both Democrats and Republicans.

In summary, it has been demonstrated that support of the federal government decreased substantially between 1964 and 1970. Data presented suggested that this increased distrust of the government was partially related to changing attitudes on the issues of racial integration and U.S. involvement in the Vietnam war. A curvilinear relationship was found to exist between policy preference on these issues (as well as on a wide range of other contemporary social issues) and political cynicism, with those favoring centrist policies being less cynical than those preferring noncentrist opposing policy alternatives. This complex relationship between cynicism and policy preference was later explained by dissatisfaction with the policies of both political parties.

The dissatisfied noncentrists formed two highly polarized and very different types of cynics, "cynics of the left" and "cynics of the right." It was established that "cynics of the left" preferred policies bringing social change, while those on the right favored social control policies. The data suggested that increased cynicism could be explained for "cynics of the left" in terms of frustrations arising out of unmet political expectations for social change, an impatient desire for the alteration of existing policies in order to solve social problems more rapidly, and the belief that the policies of the Republican administration, elected in 1968, had brought a deterioration in the social and economic position of certain groups in society. Increased cynicism on the right has similarly been explained in terms of unmet expectations but for greater social control (a hoped-for Vietnam victory, a slowing of racial integration, a more forceful stand on law and order), and fear arising out of what were perceived as the "too liberal" policies of both parties which threatened to negate their values, as well as undermine their social and economic position.

Finally, the data analysis suggests that the individual evaluates the policies of the parties with respect to his own preference, and if he is dissatisfied with those policies it is more likely that he will feel alienated from the political system. There is, however, nothing *inherent* in his own policy preference that would result in his being less trusting of the government. If an individual takes a centrist position on an issue and perceives the policies of the parties to be in disagreement with his desires and expectations, he is as likely to be distrusting of the government as the individual who prefers a more extreme policy position. But, since public opinion on the issues is, in fact, polarized and because the parties are perceived by a sizable proportion of the population to offer centrist policy alternatives as solutions to contemporary problems,[33] it follows that those who prefer noncentrist policies are more dissatisfied and therefore the most alienated.

Conclusion

What conclusions can be drawn from these research results, and what implications for change do they suggest?

This study demonstrates that the widespread discontent prevalent in the U.S. today arises, in part, out of dissatisfaction with the policy alternatives that have been offered as solutions to contemporary problems. The findings strongly suggest, moreover, that policy alternatives more acceptable to the total population will be exceedingly difficult to discover in the future because of the existing degree of issue polarization. This polarization implies that, on the one hand, there is growing discontent among some individuals because of an unfulfilled desire for social change, while, on the other hand, there is an even larger group of alienated individuals who are fearful of change. Those desirous of change, and particularly blacks—since the contemporary problems so directly affect their lives—are those most in need of the use of political influence to obtain that change; however, they perceive the legitimate means of attaining such influence as ineffective. This suggests, for blacks at least, that other means, whether peaceful or violent, legal or illegal, will have to be found to bring about the desired modifications. A move on the part of the administration to make its policies more responsive to the needs of blacks and other minority groups would undoubtedly reduce cynicism among those on the left, but, as the data suggest, this would at the same time increase the size of the cynical group on the right and probably intensify its distrust. The current political dilemma arises out of such issue polarization and also out of the difficulties implicit in maintaining government responsiveness and providing for orderly change without the occurrence of intense political conflict under such conditions. Discontent can be functional for a political system if it acts as a catalyst for orderly change, but when the normal channels are perceived as ineffective, the probability that the conflict may burst forth in the form of extra-legal behavior increases.

One goal of the U.S. political system is to contain protest and rage within the electoral process,

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[33] The proportion of the total population perceiving the Democratic party as taking a centrist position (#'s 3–5) on the eight issues ranged from 51.9 to 66.5 per cent. In comparison, 60.7 to 68.9 per cent placed the Republican party in positions # 3–5 on the eight issues.

and parties generally provide a vehicle for such protest. However, investigation of the data has demonstrated that people lack confidence in the ability of the existing parties to bring about responsive government. The great dissatisfaction with the policies of both parties implies that conditions are highly conducive to party realignment and reformation or a third party movement. What appears necessary to re-establish confidence in the parties is a wave of populism and party reforms which will allow those who normally lack access to legitimate power, but want change, to bring about desired modifications. Whether the current reforms of the primary system and the Democratic party will have a substantial impact on confidence in parties remains to be seen. The results of the study suggest that we are in a critical period of change in the attitude of political trust. There are numerous indications that the distrust of the government has increased beyond the 1970 level. In a system as stable as that in the U.S., however, it is difficult to conceive of the trend in trust continuing to decline at the same rate that it has from 1964 to 1970. A slowing in the rate of decline would certainly have been expected to accompany a settlement of the Vietnam conflict, improvement in the economic condition of the country, the easing of racial tensions, and a reduction in turmoil in schools and on university campuses. Indeed, between 1970 and 1972 the decline in political trust was arrested; some segments of the population were even displaying increased trust in the government by the fall of 1972.[34] The Watergate revelations, seemingly unending political scandals in the Nixon administration, continuing inflation, an energy crisis, and a near-collapse in the president's credibility have, however, rendered short-lived any hopes for renewed trust in the near future.

What happens if the policies of the administration elected in 1972 continue to bring no reduction in dissatisfaction? Likewise, what happens if present policy is maintained in the future? The trends in the data suggest that trust of the government would continue to decline, increasing the difficulty for leaders to make binding political decisions, as well as raising the probability of the occurrence of radical political change. This would again seem to be particularly true for blacks, for whom the very means of legitimate political control and change are now on trial. Further increases in alienation would presumably bring into question the very philosophy and goals of the political system, as well as the viability of the political community itself, and a "desire for political separation may develop."[35]

The importance of political trust is often couched in terms of a discussion of social control or the necessity for influence. It is argued that without enjoying sufficient political trust, political leaders cannot freely make decisions and commit resources to attain collective goals. Likewise, as cynicism increases, the desire for change increases; if confronted with an immobile leadership, this can lead to desire for *radical* change. Both of these concerns have important implications for the political system and merit attention. Such analysis, however, almost smacks of an insensitivity to the questions about the quality of life which must have led to such discontent in the first place. The use of political trust as a simple barometer of satisfaction with the political system is too often ignored or buried beneath a series of esoteric arguments and theoretical concerns. The disenchantment and dissatisfaction of individuals who feel politically inefficacious and cynical about the government is real, and it arises out of a reaction to real conditions of life. The use of political cynicism as an indicator of the quality of American life is, in and of itself, an important justification for its continued study. It is hoped that continued study will bring a better understanding of those conditions which lead to political cynicism and the basic information from which solutions to the problems may be found.

The distribution of policy preferences on contemporary issues, and the curvilinear relationship between preferences and discontent implies that these solutions will be difficult to come by. It is, however, important that these problems be solved, particularly since experts agree that the three major types of contemporary civil violence—urban riots, campus disturbances and crime—all flow from the wellsprings of unsolved social problems. Solutions to these problems necessitate change, but if alterations of the current social system are to be forthcoming, the resistance and fear which often deter change must be dealt with. The findings of this study suggest that a reduction of alienation among those favoring change can be brought about by creative and constructive action in domestic areas that will benefit the majority of people and diminish feelings of frustration and relative deprivation. But the caveat must be repeated that these policies need to be designed to solve existing social problems without further alienating a substantial proportion of the population. The vast opinion leadership potential of the president would have to be fully exploited if this strategy were to be realized in the current climate of opinion polarization. A president initially viewed as a moderate or centrist ought to have a much better chance of mobilizing broad support for reform policies under these circumstances than a president who is viewed as a noncentrist. However, the leadership role cannot rest with the president alone; the decrease of discontent among those preferring social control policies would require that constructive programs of social change be administered by leaders not only sensitive to the fears evoked by change but also capable of dealing with the resulting resistance to such programs. These programs would require educating the public so that it will better understand the need for social change and the benefits which would evolve from such change. Whether the necessary modifications of the social and political system and the style of leadership necessary to bring about these changes with minimal conflict will be forthcoming is, again, yet to be realized. The results of this study, however, clearly imply that current discontent in America will not be reduced with a continuation of centrist policies, or centrist politics, especially if such politics include numerous promises which give rise to expectations that are never fulfilled.

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[34] Between 1970 and 1972 the cynicism PDI value for whites increased from 0 to 7, while trust continued to decline for blacks. The change in cynicism between 1970 and 1972 for blacks was, however, much slower than it had been between 1968 and 1970, going from a 1970 PDI value of −31 to −39 in 1972.

[35] Gamson, *Power and Discontent*, p. 52.

APPENDIX

Issue Questions for 1970

Urban Unrest

[a]1. There is much discussion about the best way to deal with the problem of urban unrest and rioting. Some say it is more important to use all available force to maintain law and order—no matter what results. Others say it is more important to correct the problems of poverty and unemployment that give rise to the disturbances. And, of course, other people have opinions in between. Suppose the people who stress doing more about the problems of poverty and unemployment are at one end of this scale—at point number 1. And suppose the people who stress the use of force are at the other end—at point number 7.

Solve problems of poverty and unemployment — *Use all available force*

1 2 3 4 5 6 7

a. Where would you place yourself on this scale? ________
b. Where would you place the Democratic party? ________
c. Where would you place the Republican party? ________
d. (Where would you place) Richard Nixon? ________
e. (Where would you place) Edmund Muskie? ________
f. (Where would you place) George Wallace? ________

Vietnam

[a]2. There is much talk about "hawks" and "doves" in connection with Vietnam, and considerable disagreement as to what action the United States should take in Vietnam. Some people think we should do everything necessary to win a complete military victory, no matter what results. Some people think we should withdraw completely from Vietnam right now, no matter what results. And, of course, other people have opinions somewhere between these two extreme positions. Suppose the people who support an immediate withdrawal are at one end of this scale at point number 1. And suppose the people who support a complete military victory are at the other end of the scale at point number 7.

Campus Demonstrations

3. Some people are pretty upset about rioting and disturbances on college campuses and in high schools. Some feel sympathetic with the students and faculty who take part in these disturbances. Others think the schools should use police and the national guard to prevent or stop disturbances. And others fall somewhere between these extremes.

Government Aid to Minority Groups

4. Some people feel that the Government in Washington should make every possible effort to improve the social and economic position of Negroes and other minority groups. Others feel that the Government should not make any special effort to help minority peoples but they should be expected to help themselves.

Inflation

5. There is a great deal of talk these days about rising prices and the cost of living in general. Some feel that the problem of inflation is temporary and that no Government action is necessary. Others say the Government must do everything possible to combat the problem of inflation immediately or it will get worse.

Protecting Rights of Accused

6. Some people are primarily concerned with doing everything possible to protect the legal rights of those accused of committing crimes. Others feel that it is more important to stop criminal activity even at the risk of reducing the rights of the accused.

Ecological Pollution

7. There are many sources of air and water pollution; one of them is private industry. Some say the Government should force private industry to stop its polluting. Others believe industries should be left alone to handle these matters in their own way.

Government Health Insurance

8. There is much concern about the rapid rise in medical and hospital costs. Some feel there should be a government health insurance plan which would cover all medical and hospital expenses. Others feel that medical expenses should be paid by individuals and through private insurance like Blue Cross.

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[a] Only these two items were used in the 1968 election study.

Comment: The Political Relevance of Trust in Government*

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"In God We Trust: Everyone Else Pays Cash." America's political leaders should not pretend to godliness; no one will be fooled. According to prestigious biennial national surveys, the government's credit rating has steadily declined as a result of a disastrous foreign investment and growing consumer resistance to its "line" of products. Neither the country's present management nor its most prominent rivals inspire public confidence. How, then, can the political system rebuild its depleted reserves of political trust, the basis of future growth and stability? Will "one good season," better advertising, new blood in the boardroom or product innovation be sufficient? Or is a drastic restructuring of the regime's organization and operating procedures the only alternative to liquidation?

A Political Theory of Political Trust

Arthur Miller's article, "Political Issues and Trust in Government: 1964–70" makes an important contribution to our understanding of the sharp increase in political cynicism among the American public. Miller evokes the language of the corporation balance-sheet and the imagery of *Executive Suite* by suggesting that the cumulative outcome of exchanges between political authorities on the one hand and citizens on the other determines the level of public trust in government. Political elites "produce" policies; in exchange, they receive trust from citizens satisfied with these policies and cynicism from those who are disappointed. Since Miller defines both policy satisfaction and political trust in attitudinal terms, the exchange transactions he records are purely psychological in nature. Operationally, dissatisfied respondents are those whose own policy preferences are discrepant with their perceptions of the positions advocated by the party controlling the presidency. Miller's findings confirm the hypothesis that the greater the perceived discrepancy the less likely one is to express a generalized sense of trust in government. This conclusion, of course, constitutes further evidence for one of social science's most familiar generalizations: We tend to trust and like those who agree with us.

While the reaffirmation of an eternal verity is comforting, the significance of Miller's analysis lies elsewhere. In my view, "Political Issues and Trust in Government: 1964–70" argues that the performance of political officeholders and institutions determines their legitimacy. To be sure, an individual's ideological orientation and policy preferences influence his evaluations of governmental behavior, but such mediating effects are quite consistent with a theoretical emphasis on political events and experiences as the main source of public support for the political system. Thus, Miller interprets the sharp decline in the aggregate level of trust in government as the result of increased discontent with putatively unsuccessful "centrist" policies.

An emphasis on political factors as determinants of attitudes toward the political system represents a departure from earlier theoretical perspectives that stressed the causal influence of social background or personality.[1] But analysis of the well-known election studies conducted by the Survey Research Center and Center for Political Studies of the University of Michigan indicates that social background variables are neither strongly nor consistently correlated with political trust, as measured by the Trust in Government scale.[2] For example, in 1964 blacks were more

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* I would like to thank Scott Brickner, Daniel Hallin and Merrill Shanks for their assistance in the preparation of this article. The data analyzed were made available by the InterUniversity Consortium for Political Research. In addition I would like to thank Merrill Shanks and Richard Brody for allowing me access to selected materials in the 1972 National Election Study.

[1] Other studies that support the conclusion that political factors such as ideological orientations, evaluations of the performance of governmental institutions, and responses to personal contacts with political authorities are important causes of political disaffection are Joel Aberbach and Jack Walker, "Political Trust and Racial Ideology," *American Political Science Review,* 64, (December, 1970), 1199–1219, Edward N. Muller, "The Role of Political Distrust in a Theory of Support and Opposition to the Regime," unpublished paper delivered at the Madison, Wisconsin Conference on Public Support for the Political System, Aug. 13–17, 1973, Jack Citrin, "Political Disaffection in America: 1958–68" (Ph.D. dissertation, University of California, Berkeley, Jan., 1972); and Jack Citrin, Herbert McClosky, J. Merrill Shanks, and Paul M. Sniderman, "Personal and Political Sources of Political Alienation," *British Journal of Political Science,* Vol. 4 (September, 1974).

[2] On this point see Citrin "Political Disaffection in America," chap. 4, and Arthur Miller, Thad Brown,

trusting than whites; people earning less than $5000 a year were more trusting than those earning more than $15,000; and manual workers were more trusting than businessmen. In 1970, and again in 1972, these relationships were reversed. Moreover, the decline in political trust documented by Miller cannot be attributed to changes in the social composition of the American public; virtually every social group became more politically cynical between 1964 and 1972. Turning to the psychological explanations, although measures of personal competence and "trust in people" do correlate positively, albeit rather weakly, with feelings of political trust,[3] we cannot conclude that the recent erosion of public confidence in the political process reflects changes in these psychological dispositions. A comparison of the responses of the 1964 and 1970 election study samples to the questions used to measure ego strength and trust in other people reveals no systematic decrease in the number of "competent" or "trusting" answers. In sum, while social background and personality factors doubtless have some influence on how one evaluates the political order, Miller's identification of the strong and independent impact of political attitudes and experiences is convincing.

This comment accepts Miller's main conclusion that policy-related discontent is a source of political cynicism and proceeds to focus on three issues: (1) The meaning of political trust as measured by the Trust in Government scale; (2) The independent impact, if any, of *attitudes* of political cynicism on political *actions* at the individual level; and (3) A re-examination of the claim that a continuation of "centrist" policies will inhibit the restoration of public confidence in the political process. In pursuing these questions, my principal interest is in the implications of Miller's analysis for the study of political change in contemporary America.

The Meaning of Declining Trust in Government

Miller interprets the persistent decline in the levels of political trust and political efficacy to indicate that "a situation of widespread, basic discontent and political alienation" is the existing condition in the U.S. today (p. 951). He refers to "hostility toward political and social leaders, the institutions of government, and the regime as a whole" and of a "negative orientation toward the political *system*" (p. 951, italics mine). According to Miller, large segments of the American public have unfulfilled needs and blame the government for the unsatisfactory quality of life they are experiencing. Moreover, the high level of political discontent has endured so long (1966–70!) that simply voting out the incumbent leaders will not restore confidence in the political system. The persistence of widespread mistrust of government suggests that "the normal means by which conflict is managed in the political system are not fully operative" (p. 000) and, therefore, that strong potential for radical change exists. In sum, utilitarian as well as expressive motives underlie feelings of attachment to the political system, and the failure of government policy to "fulfill needs" or "meet expectations" has resulted in the erosion of political legitimacy in the United States.

These conclusions rest on the assumption that the Trust in Government scale measures alienation from the political regime rather than mere disapproval of incumbent political leaders. Unfavorable evaluations of the trustworthiness, competence, and responsiveness of "the government," "people running the government," and "public officials," the attitude-objects of the standard political trust and political efficacy items, increased substantially between 1964 and 1970. Allegiance to the political system, however, does not preclude criticism of specific policies, authorities, or institutions; many people readily combine intense patriotic sentiments with cynicism about politicians. Moreover, opinions about incumbents inevitably color evaluations of political roles or institutions, and the tendency of respondents in a survey to perceive "the government in general" in terms of the incumbent president and his administration probably is strongest in an electoral context, when partisan cues are at a maximum. Thus, the political implications of Miller's analysis depends on the discriminant validity of the Trust in Government scale as an indicator of attitudes toward the political regime.[4]

The level of political disaffection observed varies with the focus (or "object") of the questions used to measure it. For example, the 1972 National Election Study indicates that 47 per cent of the general public believed that "you can trust the government in Washington to do what is right only some or none of the time" and that 44.3 per cent felt that "quite a few of the people running the government don't seem to know what they're

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and Alden Raine, "Social Conflict and Political Estrangement," unpublished paper delivered at the 1973 annual meetings of the Midwest Political Science Association.

[3] For example, in 1964, the association between personal trust and political trust was .21 (tau-b); in 1968 it was .20 (tau-b). Aberbach and Walker report a relationship of .16 (gamma) for their Detroit sample.

[4] Throughout this paper I use the terms political authorities, regime, and community in the sense of David Easton, *A Systems Analysis of Political Life* (New York, John Wiley and Sons, 1965). In addition, I use the term political system as a synonym of regime.

Table 1. Political Trust and Attitudes toward the American Form of Government,* 1972 (percentages down)

	Trust in Government			
	Low ($n=419$)	Middle ($n=373$)	High ($n=224$)	Total ($n=1016$)
Pride in Government				
I am proud of many things about our form of gov't.	74.3	91.8	97.9	86.0
I can't find much about our form of gov't to be proud of.	25.8	8.2	2.1	14.0
Change our Form of Government?**				
Keep our form of gov't as is	43.2	63.3	80.8	58.8
Some Change needed	31.7	27.6	14.3	26.4
Big Change needed	25.1	9.1	4.9	14.7

* Access to these materials was provided by Merrill Shanks.

** The exact wording of this question is: "Some people believe that change in our whole form of government is needed to solve the problems facing our country, while others feel no real change is necessary. Do you think . . . ?"

doing." But only 13.5 per cent said "I can't find much in our form of government to be proud of." And 54.9 per cent believed that the existing political system should remain "pretty much as it is" whereas only 14 per cent advocated "a big change in our whole form of government."[5] Thus a diffuse sense of pride in and support for the ongoing "form of government" can coexist with widespread public cynicism about "the government in Washington" and the people "running" it.

The questions about pride in existing institutional arrangements and the desirability of changing them appear to be on their face more valid indicators of a basic attachment to the political regime than most of the items comprising the Trust in Government scale. To be sure, the manifest content of attitude items cannot be the sole criterion of their validity. A process of construct validation that focuses on the relationships among responses to a proposed attitudinal measure and theoretically relevant attitudes and actions constitutes a more definitive test.[6] Nevertheless, the cynical responses to the CPS political trust items are hardly extreme. To believe that the government wastes "a lot" of money, can be trusted to "do what is right only some of the time," and includes "quite a few" people who are "crooked" or "don't know what they're doing" need not bespeak a deep-seated hostility toward the political system at the regime or community levels. As Table 1 shows, in the 1972 election study 74 per cent of those who score "low" on the Trust in Government scale express pride in "our form of government."[7] And 43 per cent of the political cynics would like existing institutional arrangements to remain unchanged compared to 25 per cent who advocate a "big change in our form of government." This strongly suggests that many political cynics focus their dissatisfaction on incumbent authorities rather than systemic values and processes.

Increasing discontent with current government policy undoubtedly has contributed to the growth of political cynicism, but the decline in "trusting" responses to the Trust in Government items may also reflect a higher level of political sophistication and realism among the general public. In addition, the current *zeitgeist*, which legitimizes, even encourages, the expression of anti-political rhetoric, makes it fashionable to denigrate politicians and to criticize established institutions. As a result, the burgeoning ranks of the politically cynical may include many who are verbalizing a casual and ritualistic negativism rather than an enduring sense of estrangement that influences their beliefs and actions. And if the meaning of identical responses to the Trust in Government items change as political and cultural contexts vary, it may be inappropriate to use the same items or cutting-points to measure political disaffection at different points in time, regardless of whether these continue to form a Guttman scale.[8]

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[5] These figures are computed from the marginal distributions provided in the ICPR 1972 Election Study Codebook. "No answer" responses are omitted from the total base. The last two questions are restricted variables made available by J. Merrill Shanks.

[6] See the extended discussion of this point in Citrin et al., "Personal and Political Sources of Political Alienation."

[7] Unless explicitly noted, all the tables reported and all figures in the text derive from my own analysis of the 1964, 1968, 1970, and 1972 election study data made available by the Inter-University Consortium for Political Research. My scoring of the Trust in Government scale employs the same item dichotomies as Miller does, although our handling of missing data varies slightly and I construct scale scores by simply summing responses rather than using a formal Guttman scoring procedure.

Table 1 implies that many "political cynics" do not repudiate the political *regime;* the data reported in Table 2 point directly to a connection between lack of political trust and negative evaluations of the *incumbent* national administration. For example, in 1968 "trusting" scores on the Trust in Government scale were associated with approval of "how President Johnson is handling his job," and in 1972 political trust covaried with approval of President Nixon's performance. With a Democrat in the White House in 1964, strong Republicans formed the most cynical group on the party identification continuum, and strong Democrats were the most likely to express trust in government; during the Nixon presidency the positions of Republican and Democratic partisans were reversed. It is difficult to reconcile this pattern of attitude change among those most committed to the major political parties with the claim that the Trust in Government scale measures alienation from the political system. But if cynical responses record opposition to the "ins" as well as or instead of rejection of the political regime, the shifting relationship between party identification and political trust is to be expected. After all, in the American context partisans of the "out" party usually believe that there are alternatives to the incumbent president whose advent to power would assuage their present sense of disenchantment.

The *overall* relationship between party identification and trust in govenment (tau$-b=-.16$ in 1964, $-.09$ in 1968, and .12 in 1972, with party identification scored in the Republican direction) is relatively weak, and how someone votes for president (Goldwater in 1964, Wallace in 1968, and McGovern in 1972) is a better predictor of political cynicism than his party identification. In an era of strong ideological focus and declining party loyalty, apparently, the personal qualities and policy orientations of the presidential candidates, rather than their party labels, cue cynical (or trusting) responses.

Table 3 provides additional evidence that trust in government and support for the incumbent national administration are correlated, whereas political cynicism reflects a sense of identification with critics of the status quo. Positive evaluations of the incumbent president and vice-president—Johnson and Humphrey in 1968, Nixon and Agnew in 1970 and 1972—were more widespread among the politically trusting than the politically cynical. By contrast, the cynics were more likely to view positively politicians who themselves attacked the underlying assumptions of ongoing public policies, whether from the right or left, George Wallace in 1968 and George McGovern in 1972. These data confirm Miller's analysis of the curvilinear relationship between policy preferences and trust in government: respondents with either "leftist" or "rightist" candidate preferences are disproportionately likely to feel politically cynical.

Quite predictably, "leftist" and "rightist" cynics diverged sharply in their evaluations of presidential candidates and of groups such as civil rights leaders, black militants, student protesters, rock-festival goers, or the police. *But the responses of "leftists" and "rightists" who trusted the government differed in much the same way.* Thus, whether or not the feeling thermometer ratings of the politically cynical and trusting will diverge depends on the relative balance of ideological predispositions within these groups. (It also requires that the group or candidate being rated somehow engages these ideological predispositions.) For example, between 1970 and 1972 the predominance of conservatives among the politically trusting grew, whereas the political cynics, as a group, became more liberal. As a result, neither a negative relationship between political trust and a favorable image of George Wallace nor a positive association between trust in government and "warm" feelings about civil rights leaders appeared in the 1972 sample.

The close connection between disapproval of the incumbent president and cynical scores on the Trust in Government scale does not establish definitively that the "object" of this attitude measure is the current administration rather than the overall political system. After all, someone who is deeply estranged from the underlying values of the political system and rejects its constitutional order is unlikely to view the incumbent authorities favorably. At a minimum, however, the Trust in Government scale fails to discriminate between the politically alienated and those who mistrust particular leaders or politicians as a class without repudiating regime values or institutions. And to the extent that today a cynical score consitutes a surrogate measure of partisanship or anti-Nixon sentiment, a new face in the Oval office might well lead to higher levels of trust in government.

One difficulty with my own argument, of course, is that trust in government has continued to decline despite fluctuations in the level of presidential popularity. In 1972, 57 per cent of those expressing approval of "how President Nixon is handling his job" had cynical scores on the Trust in Government scale. One possible explanation for the coexistence of widespread support

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[8] This conclusion has disturbing implications for analysts of time-series data. Again, the appropriate safeguards lie in conceiving of the construct validation process as a continuous one.

Table 2. Political Trust and Support for Incumbents, 1964–72
(percentages down)

A) Approval of Incumbent President's Performance
1968 Sample:

	Quality of Johnson's Performance					
	Very Good [$n=137$]	Good [$n=372$]	Fair [$n=489$]	Poor [$n=172$]	Very Poor [$n=107$]	Total [$n=1314$]
Trust in Government						
Low	24.1	20.7	32.7	47.7	56.1	32.6
Middle	40.9	47.3	46.8	38.4	35.5	44.1
High	35.0	32.0	20.4	14.0	8.4	23.2

1972 Sample:

	Evaluations of Nixon's Performance*		
	Approve [$n=746$]	Disapprove [$n=291$]	Total [$n=1037$]
Trust in Government			
Low	34.7	67.0	42.8
Middle	39.5	24.1	36.2
High	25.7	8.9	21.0

* Richard Brody provided me access to this material.

B) Political Trust and Party Identification, 1964–72

Trust in Government		Strong Democrat	Weak Democrat	Independent Democrat	Independent	Independent Republican	Weak Republican	Strong Republican	Total
1964:	Low	16.5	19.8	17.3	29.1	31.2	26.6	37.8	22.7
	Middle	33.1	39.4	37.0	39.8	38.7	40.1	37.2	37.2
	High	50.4	40.8	45.7	31.1	30.0	33.3	25.0	40.0
		[$n=393$]	[$n=358$]	[$n=127$]	[$n=103$]	[$n=80$]	[$n=142$]	[$n=156$]	[$n=1409$]
1968:	Low	24.8	33.2	31.2	49.2	32.2	30.9	33.8	32.6
	Middle	42.9	42.6	43.7	36.4	50.4	46.6	53.4	44.1
	High	32.3	24.2	25.0	14.4	17.4	22.5	12.8	23.2
		[$n=266$]	[$n=331$]	[$n=128$]	[$n=132$]	[$n=115$]	[$n=191$]	[$n=133$]	[$n=1296$]
1970:	Low	44.4	43.4	44.1	46.8	38.9	37.9	34.3	42.3
	Middle	35.9	37.2	32.3	34.1	47.3	40.0	39.9	37.4
	High	19.7	19.4	23.6	19.0	13.7	22.1	25.9	20.4
		[$n=304$]	[$n=366$]	[$n=161$]	[$n=205$]	[$n=131$]	[$n=235$]	[$n=143$]	[$n=1545$]
1972:	Low	53.7	44.0	51.5	45.2	33.5	35.1	32.4	42.8
	Middle	31.2	36.8	32.8	35.1	41.2	36.7	40.2	36.2
	High	15.1	19.2	15.8	19.7	25.3	28.2	27.5	21.0
		[$n=324$]	[$n=582$]	[$n=241$]	[$n=279$]	[$n=245$]	[$n=319$]	[$n=244$]	[$n=2234$]

C) Political Trust and Presidential Vote: 1964–1972

	Trust in Government			
	Low	Middle	High	Total
1964 Vote:	[$n=240$]	[$n=428$]	[$n=434$]	[$n=1102$]
Johnson	42.1	64.7	84.3	67.5
Goldwater	57.9	35.3	15.7	32.5
1968 Vote:	[$n=292$]	[$n=444$]	[$n=226$]	[$n=962$]
Nixon	46.6	49.8	43.8	48.0
Humphrey	32.2	41.2	54.4	40.6
Wallace	21.2	9.0	1.8	11.4
1972 Vote:	[$n=646$]	[$n=600$]	[$n=347$]	[$n=1583$]
Nixon	51.9	69.7	76.7	63.9
McGovern	47.2	29.5	23.3	35.4

Table 3. Political Trust and Affect Toward Candidates and Selected Groups: 1968–1972*

Trust in Government		Nixon	Agnew	McGovern	Johnson	Humphrey	Wallace	Policemen	The Military	Civil Rights Leaders
1968:	Low	63	49		50	54	39	71	65	
	Middle	67	51		59	62	30	71	65	
	High	68	51		68	70	25	73	67	
		[*n* =1278]	[*n* =1191]		[*n* =1283]	[*n* =1281]	[*n* =1270]	[*n* =888]	[*n* =925]	
1970:	Low	53	43	45		47	35	77	71	42
	Middle	61	47	46		52	28	80	73	48
	High	65	50	46		53	30	80	74	50
		[*n* =1531]	[*n* =1485]	[*n* =983]		[*n* =1506]	[*n* =1465]	[*n* =1541]	[*n* =1510]	[*n* =1492]
1972:	Low	56	48	53		63	50	73	67	42
	Middle	70	58	45		53	51	76	71	40
	High	76	63	44		54	53	79	72	42
		[*n* =2136]	[*n* =2063]	[*n* =2112]		[*n* =2174]	[*n* =2097]	[*n* =2115]	[*n* =2074]	[*n* =2040]

* Figures given are mean ratings on feeling thermometer; 0 = very cold feeling, 97 = very warm feeling. Blank spaces in the table mean that this "object" was not rated in the year in question.

for the President and a pervasive mistrust of "government in general" is that for many respondents approval of Nixon's performance represented a short-term rationalization of an anti-McGovern vote. One possible explanation for the coexistence of widespread support for the President and a pervasive mistrust of "government in general" is that for many respondents approval of Nixon's performance represented a short-term rationalization of an anti-McGovern vote. The finding that pro-Nixon Democrats were more likely than their Republican counterparts to be politically cynical supports this interpretation. An alternative, but not mutually exclusive, explanation is that many cynical responses are ritualistic rather than genuine. The tendency to demean politics is a well-established cultural tradition in America. Even Lincoln described politicians as "a set of men who have interests aside from the interests of the people and who, to say the most of them, are, taken as a class, at least one long step removed from honesty."[9] In the vocabulary of contemporary American politics, government and politics are "dirty" words that convey contemptuous and derisive feelings. Yet precisely because the political culture sanctions expressions of political cynicism, their consequences may be purely symbolic. According to this point of view, to agree verbally that many people "running the government" are corrupt, incompetent, or untrustworthy is like shouting "Kill the umpire!" at a baseball game. Bloodthirsty rhetoric threatens neither the life expectancy of umpires nor the future of the national pastime. Thus, a diffuse opposition to the "government in general" does not preclude support for its authority in specific instances. For example, the 1972 election study reveals that while 56.1 per cent of the public agreed that "the government in Washington is getting too powerful for the good of the country and the individual person," an overwhelming majority also favored "total government action against inflation" and felt that "government should force private industry to stop its polluting."

To summarize, low scorers on the Trust in Government scale appear to form a heterogeneous group. They include "ritualistic cynics" and partisans of the "outs" as well as respondents who see no viable alternative to the incumbent authorities and reject the ongoing constitutional order. If, as seems likely, ritualistic cynics, partisan cynics, and alienated cynics differ in their backgrounds, attitudes, and modes of political participation, the policy implications of declining political trust will vary according to the relative contribution of each "type" of political cynic to this aggregate shift. "Political Issues and Trust in Government: 1964–70" provides no basis for disaggregating the politically cynical respondents or for identifying the correlates of the relevant "types" of cynic. Thus, the meaning of recent increases in the level of political cynicism remains ambiguous, and to decisively conclude that there exists widespread support for radical political change or pervasive alienation from the political *system* is premature, if not misleading.

Political Cynicism and Political Action

An investigation of the behavioral consequences of political cynicism serves two purposes. First, inferences about the impact of declining aggregate levels of trust in government require knowledge of whether and how the politically cynical and trusting differ in their actions. And methodologically, the validity of the Trust in Government scale as an indicator of alienation from the political regime depends ultimately on the emergence of theoretically predicted differences in the behavior of "high" and "low" scorers. Miller does not examine the empirical relationships between cynical attitudes and political actions, but his remarks point to a conclusion that political cynics, unlike those who trust government, favor large-scale political change. Both the precise objectives of the politically disaffected, however, and the tactics they are likely to adopt remain ambiguous. Are political cynics merely seeking new policies, or do they favor a complete reconstitution of the ongoing political order? Will political cynics seek change in the streets rather than at the ballot box? Does mistrust of government lead people to disobey the law, participate in civil violence, or join revolutionary organizations? And are the politically cynical more likely than those who trust the government to withdraw from involvement in electoral politics?

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[9] Quoted in Richard N. Current, *The Lincoln Nobody Knows* (New York, McGraw Hill, 1958), p. 187.

The standard hypothesis, of course, is that political disaffection (cynicism, alienation) is associated with a rejection of conventional or "conformist" modes of political participation such as voting, lobbying, writing letters to congressmen, and campaigning for political candidates. Thus, the politically cynical should be more likely than those who trust the government *either* to withdraw from political activity altogether *or* to engage in noncustomary, sometimes illegal, activities such as participating in sit-ins or riots, or organizing for revolution. Whether a political cynic adopts an active or an apathetic mode of behavior depends on the interactions among factors such as the level of social support for "alienated" behavior, the availability of an "alienated" response option in the concrete situation, and the individual's social status and personality.[10]

Previous research provides substantial support for these ideas. For example, Muller's Waterloo, Iowa study indicates that a low degree of trust in political authorities is strongly related to a "readiness to engage in acts of unconventional dissent against the state."[11] Schwartz reports that among both university students and urban blacks political alienation leads to the repudiation of "conformist" modes of political participation.[12] Sears and Maconahay show that participation in the 1965 Watts riot was associated with both a generalized sense of political disaffection and mistrust of specific local agencies and officials such as Mayor Yorty and the police.[13] Paige's study of the 1967 riots in Newark indicates that mistrust of local government, when combined with a strong sense of political efficacy, fostered participation in riot activity.[14] Finally, our Berkeley-based study of political alienation in the San Francisco-Oakland Bay Area found a strong relationship between feelings of disaffection from the national political regime and self-reported involvement in unconventional political protests.[15]

While these studies confirm the general proposition that unfavorable evaluations of the political system are associated with support for or participation in "oppositionist" political behavior, they leave several important issues unresolved. For example, there is conflicting evidence about whether political cynicism has a *direct* effect on riot participation.[16] More importantly, there is no consensus about the exact nature of the attitudes that underlie unconventional or illegal protest behavior. Some studies refer to feelings of pride in or belonging to "the country," others to trust in the national government or public officials "in general," and still others to evaluations of specific leaders or local government. But since these attitudes are not always strongly intercorrelated and their behavioral consequences may vary, the political implications of aggregate change on any one attitudinal dimension are unclear and may often be quite limited. Thus, the finding that a sense of estrangement from the political *community*, in Easton's terms, leads people to adopt an "oppositionist" stance and to withdraw from electoral politics does not imply that increased cynicism about the incumbent national administration will reduce turnout at presidential elections.

In this regard one recalls that many of the Watts rioters combined mistrust of local government with strong approval of the federal government, President Johnson, and the Democratic party. Quite plausibly, attitudes and actions are more likely to be linked when they are responses to the same stimulus object.[17] "Oppositionist" activity usually expresses a sense of grievance about some concrete situation and it takes as the targets of its verbal and physical attacks those perceived as responsible for that unsatisfactory situation. Those blamed need not include every, or even any, political institution and authority; in fact, we would expect protestors to express trust rather than cynicism toward government officials and agencies that share their objectives.

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[10] See David Schwartz, *Political Alienation and Political Behavior* (Chicago: Aldine Publishing Co. 1973), ch. 8, for a good summary of the problems in "modeling" the attitude-behavior linkage. The concept of a "behavioral orientation" is similar to Rokeach's concept of "attitude toward the situation." See Milton Rokeach, *Beliefs, Attitudes and Values* (San Francisco: Jossey-Bass Inc., 1970).

[11] Edward N. Muller, "A Partial Test of a Theory of Potential for Political Violence", *American Political Science Review*, 66 (September, 1972), 928–959.

[12] Schwartz, chaps. 9, 10.

[13] David O. Sears and John Maconahay, *The Politics of Violence* (Boston: Houghton Mifflin, 1973).

[14] Jeffery. Paige, "Political Orientation and Riot Participation", *American Sociological Review*, Vol. 36, (1971), 810–820.

[15] Jack Citrin, Herbert McClosky, J. Merrill Shanks, and Paul M. Sniderman, "Sources and Consequences of Political Alienation: A Preliminary Report on Indicator Development," unpublished paper delivered at the Madison, Wisconsin, Conference on Public Support for the Political System, August 13–17, 1973.

[16] See, for example, the conflict between Paige's study of Newark rioters and the report on the Watts riot in H. Edward Ransford, "Isolation, Powerlessness and Violence: A Study of Attitudes and Participation in the Watts Riot", *American Journal of Sociology*, Vol. 73 (1968), 581–591.

The closeness of the relationships among feelings of dissatisfaction about some specific issue, the belief that some circumscribed set of political actors or institutions are to blame, and the decision to act on that issue or against those authorities raises two difficulties for the argument that a diffuse sense of mistrust in government is causally related to "oppositionist" behavior. There is, first of all, the familiar possibility that people rationalize actions based on a narrowly defined grievance or on nonpolitical grounds by characterizing the entire political system as unresponsive and untrustworthy. In other words, behaviors may cause attitudes rather than the reverse. Given the connection between policy dissatisfaction and political cynicism, however, a more fundamental issue arises: Even if mistrust of government precedes acts of dissent against the authorities, does it have an independent effect on behavior. The customary hypothesis that feelings of political mistrust or alienation intervene between a sense of discontent and "oppositionist" actions requires not only that cynical and trusting repondents differ in their behavior, particularly at high levels of policy dissatisfaction, but also that with respondents' orientation toward the political system controlled, the relationship between feelings of dissatisfaction and the relevant behavioral dependent variable is significantly diminished. Few researchers have addressed this issue directly;[18] thus it remains unclear whether or not our capacity to predict the willingness of those who are dissatisfied with current policy to engage in "oppositionist" activity is enhanced by knowing that they mistrust government.

The election studies Miller and I have analyzed provide few opportunities for an analysis of the behavioral consequences of feelings of political cynicism. The 1968, 1970, and 1972 studies did, however, ask respondents whether they approved of a person's showing his disagreement with government policy by: (1) taking part in a legal protest march, (2) refusing to obey an "unjust" law, or (3) attempting, as a last resort, to disrupt the normal activities of government by joining a sit-in or mass demonstration.[19] Admittedly, these forms of protest, while unpopular and in some instances illegal, pose less severe challenges to the political regime than unorganized civil violence or organized attacks on political targets by revolutionary groups; moreover, the survey questions cited above refer to a generalized readiness to approve of certain forms of noncustomary dissent rather than to the respondent's willingness to engage in such behavior himself. Nevertheless, Muller's Waterloo, Iowa study indicates that approval of disruptive sit-ins and of noncompliance with "unjust" laws fall midway on a continuum of support for anti-regime activity that is defined by opposition to all forms of political dissent at one end and approval of armed revolution at the other.[20] Muller also reports that his measures of approval of political violence and the intention to engage in political violence were strongly interrelated (Somers's $d = .53$)[21]. Thus, it appears appropriate and unexceptional to consider the CPS questions about approval of protest marches, civil disobedience, and participation in disruptive sit-ins as indicators of a behavioral orientation that is both "nonconformist" and activist. Accordingly, if the Trust in Government scale measures feelings of alienation from the political regime, political cynics should be more likely than those who trust the government to endorse acts of political dissent.

The data, however, provide only partial support for this hypothesis. As Table 4 shows, the aggregate level of trust in government declined between 1968 and 1972. The extent of unequivocal opposition to protest marches civil disobedience, and disruptive sit-ins also diminished. But at the microlevel, there is neither a strong nor even a consistent association between political cynicism and approval of these forms of political protest. In 1970 and 1972, "low" scorers on the Trust in Government scale were slightly more likely than respondents with a high level of political trust to express at least qualified approval of protesting against government policy through disruptive demonstrations. In 1968, however, political cynicism and approval of sit-ins or mass demonstra-

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[17] See the argument made by Martin Fishbein, "Attitude and the Prediction of Behavior," in *Readings in Attitude Theory and Measurement*, ed. Martin Fishbein (New York, John Wiley and Sons, 1967).

[18] The exception is Muller, who finds that relative deprivation has a weak influence on the potential for political violence and that this influence is entirely dependent on whether such feelings of deprivation are associated with political mistrust. Muller's measure of relative deprivation, however, does not include the element of politicization that is subsumed by the concept of policy dissatisfaction.

[19] The exact working of these questions can be found in the ICPR codebook of the CPS 1970 National Election Study, pp. 83–84.

[20] Muller, "A Partial Test of a Theory . . .", p. 934.

[21] *Ibid.* p. 936.

Table 4. Political Trust and Approval of Political Protest, 1968–1972 (percentages down)

	1968				1970				1972			
	Trust in Government				Trust in Government				Trust in Government			
	Low ($n=388$)	Middle ($n=523$)	High ($n=268$)	Total ($n=1179$)	Low ($n=348$)	Middle ($n=332$)	High ($n=171$)	Total ($n=851$)	Low ($n=951$)	Middle ($n=801$)	High ($n=466$)	Total ($n=2218$)
Legal Protest Marches:												
Approve	15.1	20.1	24.2	19.5	14.0	14.0	14.6	14.2	21.6	18.4	16.8	18.7
Depends	27.1	25.7	28.9	26.9	40.9	43.5	37.4	41.1	39.5	40.1	41.6	40.8
Disapprove	57.8	54.2	46.9	53.5	45.1	42.6	48.0	44.7	38.9	41.5	41.6	40.5
		($p<.05$)				n.s.				n.s.		
Refusal to Obey Unjust Laws:												
Approve	13.1	17.1	12.6	14.7	12.9	10.4	11.0	11.6	20.9	13.5	11.7	16.6
Depends	27.8	23.1	18.8	23.9	40.7	35.9	37.2	38.2	42.2	39.0	36.9	39.3
Disapprove	59.0	59.8	68.6	61.4	46.4	53.7	51.7	50.3	36.9	47.6	51.4	44.0
		($p<.05$)				n.s.				($p<.05$)		
Disruptive Sit-ins, Demonstrations:												
Approve	6.2	8.2	10.1	8.0	9.8	6.6	6.9	7.9	10.0	7.2	5.2	7.9
Depends	15.5	20.1	17.2	17.9	36.5	31.0	24.9	31.8	35.5	32.4	29.8	33.6
Disapprove	78.4	71.7	72.8	74.1	53.7	62.3	68.2	60.3	54.5	60.4	65.0	58.5
		n.s.				($p<.05$)				($p<.05$)		

tions were unrelated. Moreover, in 1968 the politically trusting were *more* likely than political cynics to approve of noncompliance with an "unjust" law or participation in legal protest marches.

The weak and unsystematic relationships between scores on the Trust in Government scale and support for an activist behavioral orientation belie the contention that a diffuse mistrust of political authorities intervenes between political cynicism and approval of disruptive sit-ins or mass demonstrations. The relationships that do appear may be a spurious reflection of the association between mistrust of government and policy dissatisfaction. In both 1970 and 1972, policy dissatisfaction, operationalized as Miller proposes by measuring the distance between an individual's policy preferences and those he identifies with the ruling Republican party, was positively correlated with approval of all three forms of political protest cited above.[22] Controlling for policy dissatisfaction causes the relationships between political cynicism and support for disruptive sit-ins to vanish, whereas the relationships between policy dissatisfaction and support for activist modes of protest remain statistically significant even when we control for the putative intervening variable, trust in government. Thus, it appears that feelings of cynicism about "the government in general" have no independent effect on someone's willingness to approve of or engage in noncustomary and illegal protests. Of course, failure to confirm a strong connection between mistrust of government and support for "oppositionist" activity may reflect inadequacies in our measures of these attitudes and behaviors rather than faulty theorizing.

The 1970 and 1972 election studies indicate that the intensity of a person's discontent with current policy has a direct effect on his orientation toward acts of political dissent. In the 1968 study, however, no such association was found between policy dissatisfaction and approval of legal protest marches, civil disobedience, or sit-ins. Part of the explanation for this discrepancy between the 1968 data and the results of the later studies is that the *direction* of a person's policy preferences as well as the *degree* of his dissatisfaction influence his willingness to support an activist behavioral orientation. In all three data sets analyzed, leftist positions on policy questions are correlated with approval of legal protest marches, noncompliance with unjust laws, or disruptive sit-ins. Given the historical and contemporary associations of these types of protest activity with left wing or liberal causes, this is hardly surprising. But while the relationships between issue orientations and approval of noncustomary protest tactics remained stable, the proportions of "leftists" and "rightists" among those dissatisfied with government policy, and among the political cynics, shifted between 1968 and 1972. And as the discontented and cynical groups came to include relatively more blacks and liberals, they were more likely than respondents expressing satisfaction with current policy and trust in government to endorse unconventional forms of political protest. Thus, whether declining levels of political trust imply a growing potential for political protest and violence depends at least in part on the nature of the relationships among political cynicism, policy dissatisfaction, and ideological orientations.

Granting that political cynicism plays a minor role in producing activists or rebels, does this attitude toward government generate apathy? Are political cynics more likely than people who trust government to withdraw from participation in "conventional" electoral politics? Table 5 conclusively refutes this proposition. Although feelings of political powerlessness and perceptions of governmental institutions as unresponsive to the public's demands are consistently and strongly associated with low levels of political interest and participation,[23] mistrust of government and political apathy do not go together. Even without controlling for education, differences between "low" and "high" scorers on the Trust in Government scale tend to be small whatever the type or measure of political awareness and involvement we examine. In 1964, political cynics, many of whom were well-educated Goldwater supporters, tended to be more active than trusting respondents, whereas during the 1968, 1970, and 1972 elections the levels of interest and participation among cynical and trusting respondents were virtually identical.

Perhaps the most appropriate context for testing the hypothesis that mistrust of government leads to withdrawal from conventional political activity is the 1970 election, a low-stimulus contest without an "anti-mainstream" presidential candidate to mobolize political cynics. In that year, however, cynical and trusting respondents did not differ in their level of prior voting, voting in the 1970 election itself, media exposure during

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[22] The measure of policy dissatisfaction for both 1970 and 1972 refers to the mean distance between a respondent's issue position and the position he attributes to the Republican party on the following eight issues: Vietnam, health insurance, inflation, urban violence, crime control, campus protests, and pollution. See Miller, this issue of the *Review*. The figures concerning the relationships between policy dissatisfaction and approval of political dissent are not included, but in every case these are significant at at least the .01 level.

[23] See Citrin, "Political Disaffection in America," chap. 6, for a detailed report on the connections between diverse orientations toward the political system and political participation.

Table 5. Political Trust and Political Involvement, 1964–1972
(percentages down)

	Trust in Government			
	Low (*n*=324)	Middle (*n*=531)	High (*n*=569)	Total (*n*=1424)
1964:				
Voted	74.8	81.2	77.1	77.7
Did not vote	25.2	18.8	22.9	22.3
Paid a great deal of attention to campaign	43.5	40.1	35.5	38.2
Paid some attention	33.0	37.5	39.4	36.6
Paid little attention	23.5	22.4	25.1	25.1
Pay a great deal of attention to governmental affairs	37.5	32.2	25.3	30.3
Some attention	34.8	40.1	47.4	41.6
Little attention	16.3	16.6	17.5	16.9
Have written to a public official	21.3	18.4	13.3	16.9
Have never written	78.7	81.6	86.7	83.1
Campaign activity index:*				
Performed all five activities	2.2	1.3	0.7	1.2
Performed four activities	2.5	1.9	0.5	1.5
Performed three activities	4.0	2.3	3.1	3.0
Performed two activities	9.0	8.3	10.7	9.3
Performed one activity	26.5	30.1	23.6	26.4
No campaign activities	55.9	56.1	61.4	58.6
	Trust in Government			
	Low (*n*=427)	Middle (*n*=574)	High (*n*=303)	Total (*n*=1304)
1968:				
Voted	70.6	78.6	76.3	78.5
Did not vote	29.4	21.4	23.7	21.5
Paid a great deal of attention to campaign	39.3	40.3	38.8	38.9
Paid some attention	37.2	42.4	45.1	40.4
Paid little attention	23.4	17.4	16.1	20.8
Follow public affairs most of the time	37.1	31.9	28.7	33.0
Some of the time	23.8	34.0	34.0	30.7
Now and then	18.6	18.8	19.5	18.7
Hardly at all	20.5	15.3	17.8	17.6
Have written to a public official	21.5	18.0	19.4	19.8
Have never written	78.5	82.0	80.6	80.2
Campaign activity index:*				
Performed all five activities	0.0	1.6	1.0	0.9
Performed four activities	2.3	1.7	1.0	1.8
Performed three activities	3.5	5.4	2.6	4.1
Performed two activities	7.0	9.6	5.9	7.9
Performed one activity	25.1	27.5	26.2	26.6
No campaign activities	62.1	54.2	63.3	58.7

Table 5 continued on next page

Table 5.—*Continued*

	Trust in Government			
	Low	Middle	High	Total
1970:	($n=647$)	($n=566$)	($n=309$)	($n=1522$)
Voted	56.2	60.1	57.6	57.6
Did not vote	43.7	39.9	42.4	42.4
Paid a great deal of attention to campaign	31.1	36.0	33.0	33.1
Paid some attention	41.8	43.0	47.2	43.2
Paid little attention	27.1	21.0	19.8	23.7
Campaign activity index:*				
Performed all five activities	0.9	1.5	1.3	1.2
Performed four activities	1.7	3.4	2.5	2.5
Performed three activities	2.3	4.5	3.1	3.2
Performed two activities	4.8	5.5	6.9	5.4
Performed one activity	22.3	22.8	18.9	21.6
No campaign activities	68.0	62.3	67.3	66.0

	Trust in Government			
	Low	Middle	High	Total
1972:	($n=931$)	($n=786$)	($n=454$)	($n=2171$)
Voted	69.7	76.0	75.1	72.8
Did not vote	30.3	24.0	24.9	27.2
Paid a great deal of attention to campaign	31.9	34.4	32.8	31.5
Paid some attention	41.1	42.4	44.8	41.1
Paid little attention	27.0	23.2	22.4	27.4
Follow public affairs most of the time	35.7	40.1	33.5	36.6
Some of the time	34.0	36.9	40.1	36.2
Now and then	17.4	14.2	15.9	15.9
Hardly at all	12.9	8.8	10.6	11.4
Have written to a public official	23.4	30.1	30.2	27.1
Have never written	76.6	69.9	69.8	72.9
Campaign activity index:*				
Performed all five activities	0.4	0.9	0.7	0.6
Performed four activities	1.7	0.6	2.2	1.4
Performed three activities	3.1	3.8	3.3	3.4
Performed two activities	7.9	10.9	10.1	9.4
Performed one activity	26.0	24.7	24.7	25.0
No campaign activities	60.8	59.1	59.0	60.2

* This index was created by simply summing the number of these campaign activities performed. The activities were: trying to influence someone's vote, attending a political meeting or rally, belonging to a political club or organization, working for a candidate, and displaying a button or bumper-sticker.

the campaign, concern about the election's outcome, general political interest, political knowledge, or campaign activities such as contributing money, wearing a campaign button, attending political meetings, belonging to a political club or organization, and attempting to influence other people's voting decisions. In other words, the politically cynical were as likely as those expressing trust in government to be eligible for good citizenship awards. *Thus, the evidence that mistrust of government, as operationalized by Miller, produces neither political apathy nor political activism reinforces the argument that many cynical responses merely record opposition to incumbent officeholders or largely ritualistic expressions of fashionable clichés.*

Political Trust and Public Policy: Must "Centrism" Fail?

I do not dispute Miller's finding that disagreement with government policy on important contemporary issues engenders political cynicism.

The data indicate relatively robust and stable correlations between his proposed measure of policy dissatisfaction and the Trust in Government scale—Pearson's $r = .30$ in both 1970 and 1972. Nevertheless, while only 6 per cent of the 1970 respondents with a high level of policy dissatisfaction (mean issue distance scores of 4 or above) expressed a strong sense of trust in government, fully 24.3 per cent of those who agreed completely with the policy preferences of the ruling Republican party expressed political cynicism. Quite plausibly, disagreement with the policies of incumbent authorities is a sufficient but not a necessary condition for judging them untrustworthy and incompetent.

Miller goes beyond this general conclusion to make the more specific assertions that mistrust of government results from dissatisfaction with *both* Republican and Democratic policies and that the "centrist" character of these policies arouses discontent. These claims about the focus and content of policy dissatisfaction are problematic and deserve more detailed consideration.

The argument that political cynicism is related to dissatisfaction with both parties rests on the comparison of mean Trust in Government scores among groups of 1970 respondents cross-classified according to their locations on measures of dissatisfaction with the parties' policies (Miller, Table 8). Miller himself acknowledges that the relationship between cynicism and dissatisfaction with the Democrats, controlling for perceived distance from the incumbent Republicans, is weak and uneven. And the finding that mean levels of trust in government decline monotonically along the diagonal entries in his Table 8 appears to be due largely to the successive increases in perceived distance from the Republican party.

The 1972 election study data indicate unambiguously that dissatisfaction with the perceived policies of the "out" party and its leader are not related to feelings of political cynicism. The correlations between political cynicism and dissatisfaction with the policy stances of the Democrats and George McGovern were an insignificant .04 and −.03 respectively. In addition, Table 6 below, a replication of Miller's Table 8 using the 1972 election study data, confirms that at any given level of dissatisfaction with the policies of the Republicans (or Nixon), increased perceived distance from the policies of the Democrats (or McGovern) was *not* associated with higher levels of political mistrust. (Readers are warned that according to my scoring procedures, unlike Miller's, 0 is the most cynical and 5 is the most trusting score on the Trust in Government scale.) Between 1970 and 1972, the tendency of political trust to signify support for the incumbent national administration grew concomitantly with the increased visibility and salience of ideological cleavages between the parties. One can no longer conclude, therefore, that "those who feel that *neither* party offers viable solutions to contemporary social problems are among the most cynical, distrustful, and alienated citizens in the U.S. today" (Miller, p. 968).

Table 6. Dissatisfaction with Party Policy and Political Cynicism: A Replication of Miller's Table 8 with the 1972 Election Study Data

(Entries are mean scores on trust scale. Trust in Government scores range from 0 = most cynical to 5 = most trusting.)

Distance from Republican Party	Distance from Democratic Party					
	0	1	2	3	4	
0	2.5	2.6	2.4	2.6	2.2	($n = 329$)
1	2.4	2.1	2.4	2.2	2.0	($n = 513$)
2	1.3	1.5	1.9	2.1	2.2	($n = 244$)
3	1.1	1.4	0.9	0.5	1.4	($n = 110$)
4	1.1	0.8	0.8	0.5	2.2	($n = 82$)
	($n = 300$)	($n = 511$)	($n = 268$)	($n = 92$)	($n = 30$)	

Distance from Nixon	Distance from McGovern					
	0	1	2	3	4	
0	2.5	2.6	2.3	2.3	2.2	($n = 455$)
1	2.2	2.3	2.3	2.3	2.0	($n = 514$)
2	1.4	1.7	1.9	1.7	1.9	($n = 275$)
3	1.2	1.11	1.4	1.0	2.5	($n = 141$)
4	0.9	1.07	0.5	1.6	1.7	($n = 99$)
	($n = 213$)	($n = 443$)	($n = 313$)	($n = 179$)	($n = 75$)	

Miller bases the proposition that "centrist" policies are the source of public discontent and cynicism toward government on the finding that people who placed themselves at the extremes (scale values 1 and 7) on a number of policy questions were less likely to express trust in government than those who located themselves at the center (scale value 4). Given the institutional and cultural pressures toward compromise, bargaining, and coalition-building in American politics, this conclusion should surprise no one. Indeed, the slight relationship between political cynicism and dissatisfaction with both parties in the 1970 sample reflects the tendency of those at the ideological extremes to reject moderate parties whose primary goal is electoral victory. It is noteworthy, therefore, that Miller's data reveal several deviations from the reported tendency of both the "leftist" and "rightist" camps to contain relatively more political cynics than the group at the center of the political spectrum. For example, Table 5 in his paper indicates that respondents at scale position 4 on the national health insurance issue were more likely to be politically cynical than those at the extreme right of this issue continuum. Similarly, on the question of what should be the rights of accused criminals, the most trusting group of respondents were those with a self-assigned scale value of 2, near the liberal end of the continuum. Only on the urban riot and Vietnam policy questions were there consistently monotonic decreases in the level of political trust as one moved from the center of the policy continuum toward its extremes.

Even if one makes the somewhat unrealistic assumption that people regularly monitor the extent of their disagreement with government policies and then re-evaluate how much they trust political authorities, the impact of a move from the center on the aggregate level of political cynicism depends on the size and cohesion of the "centrist" bloc. Miller, however, underestimates the number of "centrists" in the public by applying this term only to those who assign themselves a scale value of 4 on the seven-point issue continua. That more than half of the 1970 respondents whose Vietnam policy scale values were 3 and 5 respectively agreed that U.S. policy should be "to keep our soldiers in Vietnam but try to end the fighting" suggests that the "centrist" bloc includes a larger number than he allows for. But when the "center" is redefined to include scale values 3, 4, and 5 it becomes the modal, although not the majority, position of the 1970 sample on the issues of urban unrest, Vietnam, rights for the accused, and government aid for minority groups. And when the 1972 study gave respondents the explicit choice of saying they had not thought about an issue, the proportion of respondents with "leftist" (scale values 1 and 2) or "rightist" (scale values 6 and 7) preferences declined even further.

Determining the precise meanings of "leftist," "centrist," or "rightist" policy preferences presents another methodological problem. Miller tends to rely too much on the numerical symbols that denote opinions on a given issue and to overlook the ambiguity of their concrete referents. In some cases it is difficult to imagine what these specific policy positions might be. For example, what is a "centrist" policy on inflation? Does someone who assigns himself a scale position of 5 on this issue reject a policy of wage and price controls? Does someone who calls for "total" government action against inflation advocate a planned economy, a tight money policy, or both? What does it mean to give oneself a scale value of 6 on the issue of urban unrest? Does this indicate support for the use of only two-thirds of the available force to quell riots?

On some issues, of course, elites have defined policy alternatives in ways that make it easy for the public to identify meaningful left, right, and center positions. In Donald Stokes's terms these are "position" issues; that is, on such questions there are rival bodies of opinion about the goals of public policy and how to achieve them.[24] American policy toward the war in Vietnam became a "position" issue on which the policy of "fight and negotiate" emerged as a distinctively "centrist" option. But on most of the other policies Miller discusses, the public does not encounter a well-articulated set of choices that can be ranked along the left-right continuum. And if one cannot tell where along the seven-point continuum respondents would locate a particular policy, the effect of its adoption on the aggregate level of political trust is indeterminate. The self-anchoring scale is an easily administered and hence convenient methodological device, but it is hardly the most appropriate technique for identifying concrete policy preferences. Similarly, when the referents of perceived policy stands are vague or vacuous, we should be cautious about using issue distance measures as valid indicators of policy dissatisfaction. Clearly, the diffuse character of the perceptions that define a respondent's distance from a party or candidate enhances the possibility that "high" policy dissatisfaction scores result from a tendency to attribute discrepant opinions to people one already dislikes.

Thus, the relationships between political cynicism and "extremist" policy orientations do not imply that abandoning "centrist" policies would rebuild trust in government. Miller argues that when government moves to the left it often increases the number of "cynics of the right," and vice versa. By the logic of his own argument, such shifts in policy would also produce "cynics of the center." As both George McGovern and Barry Goldwater have discovered, Anthony Downs remains relevant reading material.

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[24] For the distinction between "position" and "valence" issues, see Donald E. Stokes, "Some Dynamic Elements of Contests for the Presidency," *American Political Science Review*, 60 (March, 1966), pp. 19–38.

Miller is convinced that only policies of social change—"leftist" or liberal policies, that is—can solve existing social problems, improve the quality of life, and thereby stem the tide of public cynicism and dissatisfaction. He advocates "creative and constructive" action that will benefit the majority of people and diminish feelings of frustration. It is difficult to oppose such a recommendation. Of course, government will have to counter the inevitable resistance to creative radical change by "educating the public so that it will better understand the need for social change and the benefits which would evolve from such change" (Miller, p. 972). In other words, the restoration of public trust in government requires a "centrist" president to sell "leftist" policies to a sizeable group of "rightists." According to Miller, failure to achieve these changes in policies and leadership will cause even more people to repudiate the underlying goals of the ongoing political system and raise the probability of extra-legal protests leading to major institutional transformations.

It seems to me that this apocalyptic prognosis rests on a dubious interpretation of the meanings of political cynicism and policy dissatisfaction, as operationally defined. In addition, Miller's pessimism about the consequences of the government's continuing to pursue "centrist" policies, or, more properly, of its seeming to do so, assumes that the most salient political questions are "position" issues on which there is a sharp polarization of public opinion. Today, however, "valence" issues such as inflation, economic prosperity, the energy crisis, and honesty in government are uppermost in the public mind. On these issues, everyone agrees about the goals of public policy; everyone, including political cynics, is against inflation and corruption and for prosperity, full employment, and cheap gasoline. Because public opinion on "valence" issues does not constrain the government's choice of specific policies for achieving consensual goals, *results*, such as an improving economy, will do more to rebuild trust in government than the adoption of some particular program or ideological orientation.

The current decline in trust in government and the public's seeming loss of confidence in a wide range of social institutions and elites are expressions of a pervasive sense of malaise. Many Americans feel unhappy about the state of the nation and, quite naturally, blame incumbent authorities for this situation. But many political cynics are probably expressing the conviction that "the times are bad" rather than repudiating the political regime on ideological grounds. To use another baseball analogy, political systems, like baseball teams, have slumps and winning streaks. Having recently endured a succession of losing seasons, Americans boo the home team when it takes the field. But fans are often fickle; victories quickly elicit cheers. And to most fans what matters is whether the home team wins or loses, not how it plays the game. According to this analysis, a modest "winning streak" and, perhaps, some new names in the lineup may be sufficient to raise the level of trust in government.

Unfortunately, winning baseball games comes more easily than progress in solving the complex and intractable problems that face American society. Overcoming public resistance to social change becomes a pressing issue only after government discovers the "creative and constructive" programs that, if implemented, will benefit the majority of the people. In the meantime, politicians should follow Miller's advice to promise less and deliver more. For as long as "bad times" persist and current linguistic habits do not change, political authorities, *whatever their ideological predilections*, will remain the object of considerable public scorn and mistrust.

Conclusion

Since the previous sections have necessarily accentuated my disagreements with Miller's analysis, I would like to re-emphasize the significance of his demonstration that political events, attitudes, and expectations have been a primary source of declining trust in government. Future research should develop superior indicators of subjective orientations toward the political system and specify more precisely the cognitive processes that link policy dissatisfaction to political cynicism. In this regard, we need to distinguish, operationally, between the following attitudes: dissatisfaction with current government policy positions, dissatisfaction with the *outcomes* of ongoing events and policies, mistrust of incumbent officeholders, and rejection of the entire political system.

We also need greater knowledge of the consequences of cynical attitudes at both the micro and macro levels. In particular, we require an empirical assessment of the familiar contention that low levels of political trust inhibit the capacity of political elites to make necessary decisions and commit public resources to collective ends. Stated so abstractly this proposition appears plausible, but its confirmation in a concrete case surely depends on such factors as the number of nonritualistic cynics, the degree of consensus among them, their location in the social, economic, and political stratification systems, and on the degree to which the government's policy requires the voluntary cooperation of the mass public in unpopular tasks. For instance, high levels of political cynicism did not prevent President Nixon from renewing the bombing of Hanoi in December, 1972, or from obtaining broad powers to deal with the energy crisis in late 1973.

To conceive of public trust in government as a "resource" for authorities to exploit fosters the belief that high levels of cynicism are dysfunctional and undesirable. Cynics may also provide a service, however, by insisting that policy departures be founded on public support, and, as a result, restricting the flexibility of elites. Recent events remind us that allowing authorities the freedom to act provides no guarantee that they will act wisely. It is worth recalling John Stuart Mill's belief that a democratic political culture is characterized by a vigilant skepticism (or realistic cynicism) rather than an unquestioning faith in the motives and abilities of political authority.

Rejoinder to "Comment" by Jack Citrin: Political Discontent or Ritualism?

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In 1958 only 22 per cent of the total population felt that they could not "trust the government in Washington to do what is right" all or most of the time. By the fall of 1972 that figure had climbed to 45 per cent. Furthermore; the percentage of eligible voters participating in the 1972 presidential election was the lowest it has been since 1948; crises and scandals have continually plagued the government since the Watergate revelations; and the economic conditions of the country have provoked widespread uncertainty and anxiety among the populace. There is good reason, then, for the intense current interest in attitudes of political disaffection and alienation.

Present U.S. conditions demonstrate that political alienation is a phenomenon of fundamental significance in political processes. Feelings of political cynicism and alienation may substantially diminish the willingness of citizens to participate in politics or to support programs directed at resolving the social problems that stimulate discontent. Attitudes of political alienation have likewise been related to public demands for radical political reforms during trying periods of social or economic discontent. Alienation and non-participation, however, go beyond just questions of voluntary compliance with policies or the possibilities for radical change; they strike at a very basic democratic norm. Democratic theory emphasizes voluntary consent as the basis of political obligation and legitimacy. Democratic government assumes—indeed, requires—widespread participation, political equality, the accountability of leaders and protection of the individual citizen's constitutional guarantees. The full attainment of these values is only possible when the relationship between the leaders and the public is based on mutual understanding and reciprocal trust rather than on the use of coercive and arbitrary authority.

Political distrust has varying significance for different political systems. Distrust obviously has other implications in a two-hundred-year-old democracy than it would in a dictatorship that has been in existence for only a brief period. Because of these differences a concern with questions of legitimacy in the United States seems more reasonable when focused on satisfaction or dissatisfaction with the implementation of values rather than on the acceptance or rejection of those values. Distrust of government in the U.S. may, for some, be associated with the partisan hopes of "voting the rascals out;" for others, it may indicate a sense of enduring inequities in government decisions and outputs. It does not, therefore, necessarily imply a desire for a completely different form of government—although for some it may mean exactly this. These varying "foci" of distrust may be conceptualized as a continuum incorporating distrust of the leaders, various institutions, political processes, and democratic values. The behavioral consequences of political disaffection may be expected to divaricate from apathy to participation in reform or protest behaviors, depending on the efficacy and social conditions of the population subgroups under investigation. Likewise, we can expect the several causes or correlates of a complex attitude such as trust in government to be affected by sociological and historical circumstances.

Because of these complexities it is understandable that Professor Citrin should raise, in his "Comment," questions about the meaning, behavioral consequences, and political correlates of trust in government. A number of conclusions that his discussion leads to are, however, inconsistent with the data, and more importantly, hold disturbing implications for the relevance of empirical political research as social commentary. These inconsistencies, problems of interpretations and the questions raised in the "Comment" can best be explicated by a further discussion of the three major points of the "Comment."

The Meaning of Declining Trust in Government

Political trust is the belief that the government is operating according to one's normative expectations of how government should function. The concept is closely related to the notion of legitimacy, a statement that government institutions and authorities are morally and legally valid and widely accepted. Presumably, the behavior and decisions of trusted authorities are more likely to be accepted as legitimate and worthy of support than are those of distrusted leaders. At an abstract, conceptual level, trust in government—through the notion of legitimacy—thus becomes associated with questions of identification with, or estrangement from, political institutions, symbols and values. On a more practical,

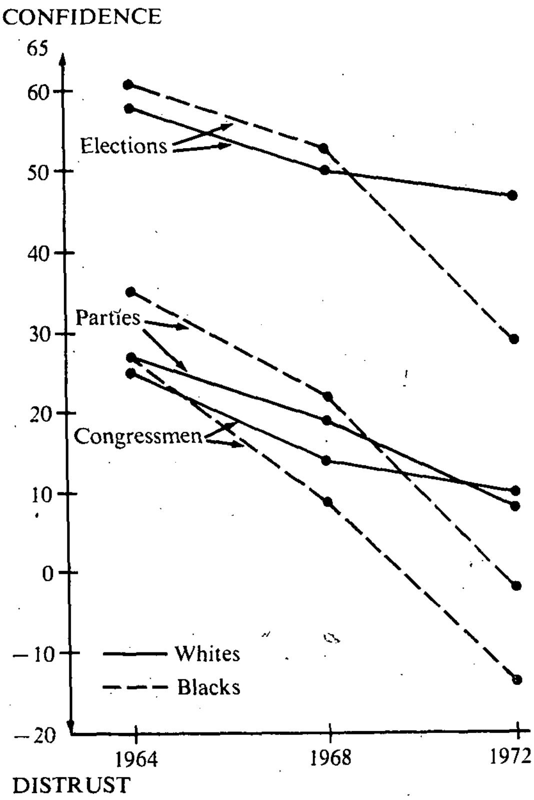

Figure 1: Confidence in Leaders, Parties, and Elections, 1964–1972

operational level, political trust may be treated as an affective orientation toward the "government in Washington"—the most salient level of government in the United States.[1]

The validity of the trust in government scale as a measure of political disaffection cannot be ascertained simply from the manifest content of the five items used to construct the scale. Nor can the specific target of the distrust be determined from the scale items alone. Questions of validity and focus can only be answered through an examination of the relationship between the trust scale and other political indicators.

As noted in "Political Issues and Trust in Government," political alienation has been predominantly conceptualized in terms of negative affect for the existing political process, and it has been viewed as composed of two distinct attitudinal components—political efficacy and political trust.[2] Political efficacy, besides showing declining trends similar to that of political trust,[3] has become increasingly intercorrelated with trust, going from a Pearson *r* of .17 in 1964 to .35 in 1972. Furthermore, political trust has also been correlated with attitudes of government responsiveness (.35 in 1964 and .39 in 1972).

Prior to 1972 CPS surveys were not well suited to an examination of the extent to which distrust has been generalized across various governmental institutions. The trend lines for three government responsiveness items dealing with confidence in congressmen, parties and the institution of elections are, however, somewhat informative about the focus of political discontent; they are thus presented in Figure 1.[4]

It is clear from Figure 1 that by 1972, discontent had been generalized to several different aspects of the political system. As with the political trust scale, the trend is more severe among blacks, who changed from the more trusting to the less

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[1] Support for the notion that the federal government is the most salient level of the political system can be found in M. Kent Jennings and Harmon L. Zeigler, "The Salience of American State Politics," *American Political Science Review*, 64 (June, 1970), 523–535.

[2] A more complete conceptual and theoretical statement of the different dimensions of alienation is presented in Ada W. Finifter, "Dimensions of Political Alienation," *American Political Science Review*, 64 (June, 1970), 389–410, and William A. Gamson, *Power and Discontent* (Homewood, Illinois: Dorsey Press, 1968).

[3] For a discussion of the efficacy trend, see Philip E. Converse, "Change in the American Electorate," in Angus Campbell and Philip E. Converse, eds., *The Human Meaning of Social Change* (New York: Russell Sage Foundation, 1972).

[4] The three government responsiveness questions were:

1. How much do you feel that political parties help to make the government pay attention to what the people think: a good deal, some, or not much?
2. And how much do you feel that having elections makes the government pay attention to what people think: a good deal, some, or not much?
3. How much attention do you think most congressmen pay to the people who elect them when they decide what to do in Congress: a good deal, some, or not much?

A percentage difference index was computed for each of the three questions by subtracting the percentage responding "not much" from the percentage answering "a good deal." Positive values of the PDI therefore specify the degree of confidence, whereas negative values denote a lack of confidence. These PDI values are plotted in Figure 1 for whites and blacks.

It should be noted that these items indicate the respondent's confidence in elections, parties and congressmen to make government responsive and not the respondent's confidence, *per se*, in the institutions and leaders referred to by the questions. The interpretation of the items as indicators of "diffuse public support" was suggested by Jack Dennis in "Support for the Institution of Elections by the Mass Public," *American Political Science Review*, 64 (September, 1970), 819–835.

Table 1. Relationship Between Confidence in the Responsiveness of Elections and Parties and Political Trust, 1972

Elections make government responsive	Trusting	Cynical	Parties make government responsive	Trusting	Cynical
A Good Deal	72%	41%	A Good Deal	37%	18%
Some	25	45	Some	53	53
Not Much	3	14	Not Much	10	29
	100%	100%		100%	100%
(*N*)	(825)	(776)	(*N*)	(803)	(763)
	Gamma = .40			Gamma = .35	

trusting group between 1964 and 1972. The data for blacks suggest that, in general, discontent is directed *first* at political leaders and parties and later becomes generalized to such institutions as elections. The persistently high degree of confidence in elections exhibited by whites is consistent with such a pattern. Their discontent with leaders and parties may not yet have reached the critical level at which it becomes broadly generalized to the most fundamental institution of all, that of elections.

The data of Figure 1 confirm what other scholars have suggested about political discontent, namely, that it is not necessarily an undifferentiated malaise directed with equal intensity at the leadership and at all political institutions. Dennis, for example, has reported that public evaluations not only vary across different institutions, but that the major governmental institutions are differentially rated with respect to trustworthiness, power and competence.[5] Gamson also hypothesized that discontent might first be directed at the authorities and then later become generalized to distrust of institutions if a change in leadership brought no reduction in discontent.[6]

One point which apparently needs emphasis is that while elections enjoy a greater degree of confidence than parties as mechanisms for making government responsive, evaluations of both these institutions are related to the general attitude of trust in government.[7] The strength of this relationship can be ascertained from Table 1 which presents the crosstabulation of the trust scale and the two responsiveness items. The data demonstrate that trusting respondents were substantially more likely to express a "good deal" of confidence

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[5] Jack Dennis, "Public Support for American National Political Institutions," (paper presented at the Conference on Public Support for the Political System, Madison, Wisconsin, August 13–17, 1973).

[6] Gamson, *Power and Discontent*, p. 51.

[7] This statement is very straightforward; it is emphasized here because Citrin has ignored the strong correlation between the "change in form of government question" and political trust, choosing instead to call attention to only a few cells in the crosstabulation of the two variables.

The reader should also be aware of an important difference between the trust in government *index* that Citrin uses and the trust in government *scale* used in "Political Issues and Trust in Government" and in this "Rejoinder." The scale scores reflect the individual response pattern for the five trust in government items ordered according to the proportion of cynical responses. Individuals with no more than two missing data responses or scale errors were assigned the score that is the median value of the possible scale scores for that particular response pattern. Respondents with more than two errors or two missing data responses were excluded from the analysis. Citrin, however, simply formed an index by adding up responses to the five trust items, thereby ignoring the cumulative nature of the items. This method difference affects the overall distribution of the trust measure as indicated below for 1972:

	Trust Index[a]	*Trust Scale*
Low	42.8%	36.8%
Medium	36.2	24.0
High	21.0	39.2
	100.0%	100.0%

[a] Figures taken from Citrin's Table 2, this issue of the *Review*.

The index and scale scores are no doubt highly correlated but they are not identical. Citrin's analysis, therefore, cannot be viewed as a replication of "Political Issues and Trust in Government," nor is it strictly comparable to the analysis presented in this "Rejoinder."

The conceptual distinction between an additive index and a scale is well established in the empirical literature; for example, see Helen Peak, "Problems of Objective Observation," in Leon Festinger and Daniel Katz (eds.), *Research Methods in the Behavioral Sciences* (New York: Holt, Rinehart and Winston, 1953), chapter 6. For a more recent statement of the response models underlying unidimensional scales, see Herbert F. Weisberg, "Dimensional Analysis of Legislative Roll Calls," (Ph.D. dissertation, The University of Michigan, 1968) or Herbert F. Weisberg, "Dimensionland: Excursions into Spaces," *American Journal of Political Science* (forthcoming, November, 1974).

in both elections and parties as a means of producing responsive government than were cynical respondents. Despite the differences in the distribution of responses to the elections and parties questions, both items were strongly correlated with the trust in government scale.

It is not surprising to find relatively more public support for the institution of elections than for congressmen. Similarly, it is not surprising, nor inconsistent, to find that 45 per cent of the 1972 respondents felt that they could "trust the government in Washington to do what is right only some of the time," while at the same time 41 per cent thought that a change in "our whole form of government was needed to solve the problems facing our country," and only 14 per cent said "I can't find much in our form of government to be proud of."[8] The importance of differences in marginal distributions for more or less extremely worded survey questions should not be overstated. What is critical here is the relationship between these relatively more extreme items and the trust in government scale, as well as the determinance of what respondents mean, substantively, when they say that a change is needed in our form of government.

Both the "pride in our form of government" and "change in our form of government" questions are somewhat biased as indicators of political discontent because, unlike the trust and responsiveness scale items, they are more sensitive to discontent among those with a social change orientation than among those expressing a social control ideology.[9] While the trust and responsiveness items measure disaffection equally as well among both ideological groups, the "pride" and "change" questions can be expected to exhibit a larger proportion of discontented responses for individuals with a leftist rather than a rightist orientation. For example, 51 per cent of those with a social change orientation say that at least "some change" in our form of government is needed, whereas only 36 per cent of those on the right say "some change" is needed. The correlation between the "change" item and the trust scale is so strong, however, that despite this question bias, the two measures are substantially related even after controlling for ideology (see Table 2). In fact, the "pride" and "change" items have loadings of .77 and .67, respectively, on the single factor (which explains 61 per cent of the item variance) obtained from a factor analysis of these and the five individual trust in government items. Thus, the "pride" and "change" items can be viewed as measuring the same underlying attitudinal dimension that is tapped by the trust in government scale.

Further support for this conclusion comes from the open-ended responses which give a good indication of what respondents had in mind when they replied that a change was needed in our form of government. These responses hardly intimate a strong desire among the populace for change from democracy to some other form of government: Less than 1 per cent of the respondents proposed a change toward a socialistic government. What *was* expressed by the open-ended statements was a discontent and dissatisfaction with the performance of the system and the need for reform to make it more responsive. A majority of the respondents commented: that the government pays more attention to some people than to others; that parties and the government bureaucracy are not responsive; that politicians are out of touch, unresponsive and untrustworthy; and that the electoral system is antiquated and in need of reform. The recommended changes included an increase in the representativeness of government, an increase in popular participation and control, a decrease in the power of the rich and an increase in the power of the poor, a more efficient government, and more governmental efforts directed at the problems of social welfare, crime control, unemployment, inflation and taxes. About one quarter of the responses suggested some type of institutional reform, such as changing the power relations among the President, Supreme Court and Congress, or reforming the parties, electoral system and government bureaucracy —but all with the intention of increasing popular control and government responsiveness.

To summarize, political cynicism is related to feelings of political inefficacy, to the belief that government is unresponsive, and to an apparent desire for structural and institutional reform. The trend toward increased distrust, therefore, reflects a growing dissatisfaction and discontent with the performance of government in the United States. In general, the trend indicates an increasing sense that the government is not functioning in a manner consistent with the normative expectations of how a democratic government ought to function. We should *not*, however, conclude that this trend simply reflects an increase in superficial, "ritualistic" responses just because distrust is not synonymous with the desire to replace democracy with another form of government. On the contrary, the trend is a serious statement that government in the U.S. is perceived as falling far short of democratic goals. We might expect, therefore, that the radicalism of desired change and reforms, as well as the methods used to effect that change,

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[8] This is the same item referred to by Citrin. The question was contributed to the 1972 CPS election study by J. Merrill Shanks.

[9] The operational definition of the ideological orientations used here is the same as that used in "Political Issues and Trust in Government."

Table 2. Relationship Between Desired Change in Our Form of Government and Political Trust by Ideological Orientation, 1972

	Social Change Ideology			Social Control Ideology		
	Trusting	Cynical	Total	Trusting	Cynical	Total
Desired Change						
Big Change	9%	35%	22%	4%	22%	11%
Some Change	24	33	29	17	29	25
Keep As Is	67	32	49	79	49	64
	100%	100%	100%	100%	100%	100%
(*N*)	(91)	(103)	(255)	(185)	(142)	(441)
	Gamma = − .47			Gamma = − .46		

would depend on the profundity and duration of political discontent.

Some Behavioral Consequences of Political Disaffection

It is suggested in the "Comment" that attitudes of political distrust and disaffection have no independent correlation with political participation, direction of vote, candidate evaluation, or support for protest behaviors; according to Citrin, this therefore implies that political cynicism is largely a ritualistic expression of fashionable clichés. Space limitations did not allow the inclusion of an investigation of the behavioral consequences of political disaffection in "Political Issues and Trust in Government." Moreover, a few pages in a "Rejoinder" would hardly suffice as an introduction to the subject since one would expect this to be a complex set of relationships affected by social conditions and psychological motivations. The discussion in this section will therefore simply elaborate on some of the relationships briefly referred to in the "Comment."

Previous studies linking alienation and behavior have been limited in several respects: in their focus on a narrow set of behaviors; in their contradictory findings; in their failure to investigate racial differences—a very important factor in both alienation and behavior. In addition, they generally provide little insight into the conditions under which alienated individuals will or will not display certain types of behavior. Much of the confusion appears to arise from the complexity of these relationships and the failure to specify the conditions that affect them.

One would expect, for example, that the relationship between efficacy and political trust would be a critical determinant of whether one acts politically and of the direction that action takes. Indeed, in the 1972 presidential election, there was an 89 per cent turnout among the efficacious and trusting, while only 62 per cent of those who felt inefficacious and distrusting voted.[10] Furthermore, whereas only 21 per cent of the efficacious and trusting respondents cast a Democratic vote for president, 49 per cent of the inefficacious and distrusting voted Democratic.[11] Both the rate of turnout and percentage of Democratic vote figures were different than would have been expected when related with efficacy or trust singly.

The 1972 election study also demonstrated that the relationship between distrust of government and the vote is not a simple linear relationship but one heavily affected by political ideology. McGovern obtained 82 per cent of the vote among cynical respondents with a "leftist" ideology—15 per cent more than would normally be expected from this group.[12] Cynical respondents with a "rightist" ideology, however, deviated further *below* their normal Democratic proportion of the two-party vote than did trusting respondents with a "rightist" ideology. Political cynicism thus had a substantial independent effect on the vote among social change advocates but actually worked against McGovern among social control advocates. This result not only demonstrates how ideology conditions the effect of political

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[10] For the complete relationship among the four variables (political efficacy, political trust, turnout and two-party vote), see Table 4 of Arthur H. Miller, Warren E. Miller, Alden S. Raine and Thad A. Brown, "A Majority Party in Disarray: Policy Polarization in the 1972 Election," (paper presented at the 1973 Annual Meeting of the American Political Science Association, New Orleans, Louisiana, September 4–8).

[11] Miller, Miller, Raine and Brown, Table 4.

[12] For a detailed discussion of these data, see Miller, Miller, Raine and Brown, pp. 47–53. The concept of the normal vote is discussed in Philip E. Converse, "The Concept of a Normal Vote" in Angus Campbell, Philip E. Converse, Warren E. Miller and Donald E. Stokes, *Elections and the Political Order* (New York: John Wiley and Sons, 1966), pp. 9–39.

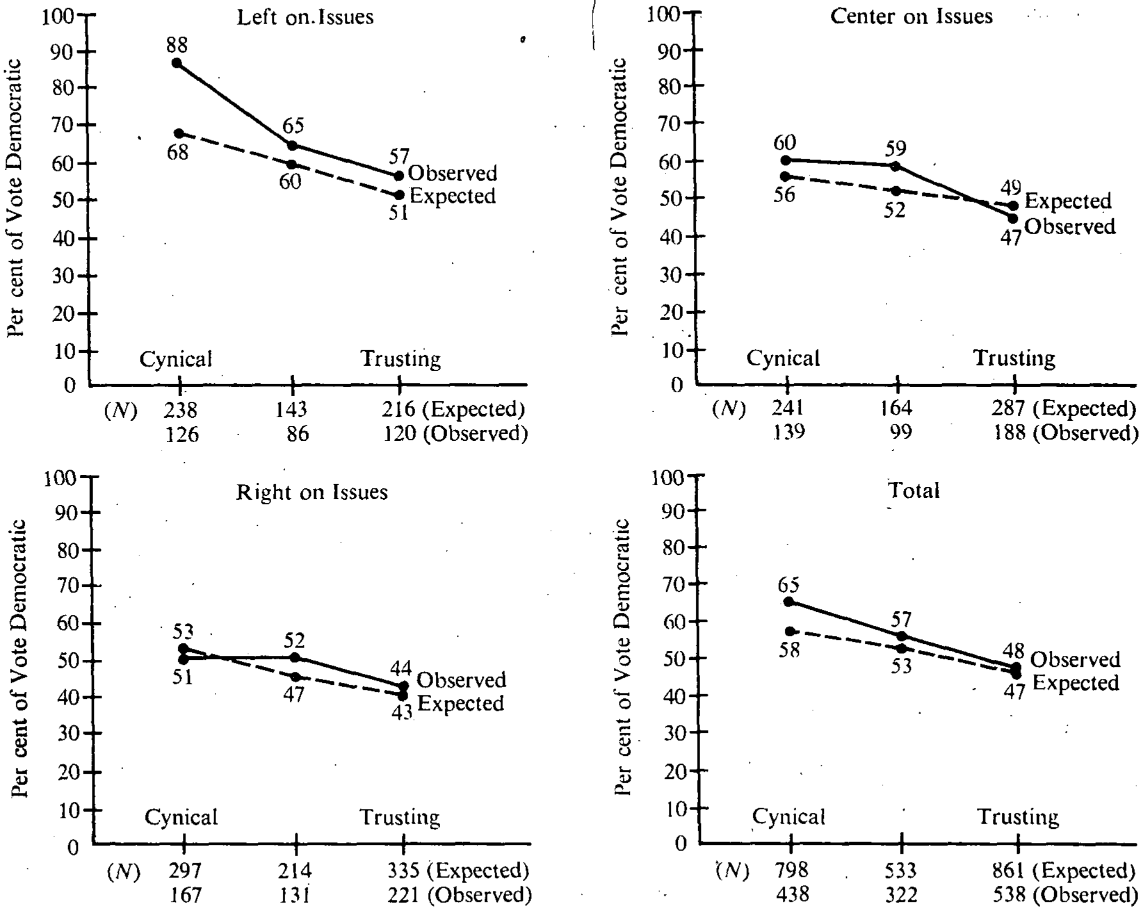

Figure 2. Expected Vote and 1972 Congressional Vote by Political Trust and Ideology

disaffection, but it also refutes the notion that distrust is simply directed at incumbent authorities.

It might, of course, be argued that this independent effect of political distrust on the vote among liberals is merely a function of the presidential race and the candidate personalities. To test this hypothesis, the above analysis was replicated with the 1972 congressional vote. The results (see Figure 2) demonstrate that for the total population cynics voted only slightly more Democratic than would normally be expected. Dividing the total population into ideological subgroups, however, results in a substantial augmentation of the independent effect of political distrust on the vote. Again, this effect appears primarily among liberals, thereby substantiating the hypothesis that ideology conditions the behavioral consequences of political disaffection.

Political distrust was also found to have had a direct impact on the voter's image of the candidates in the 1972 election.[13] Yet even this relationship may be conditioned by ideological and racial effects. Displayed in Table 3 are some data that contradict the hypothesis that " . . . political cynicism reflects a sense of identification with critics of the status quo."[14] George Wallace is surely a critic of the status quo, and yet it is quite obvious that among whites his affective ratings depended upon ideological orientations, not political trust. Among blacks, the Wallace ratings did differ by degree of political trust, but the differences are exactly opposite to those predicted by the hypothesis. In addition, black ratings of McGovern were uniformly high regardless of ideology and degree of political trust (his increased rating by black cynics of the left was not significantly higher). The relationship between political trust and evaluations of political leaders is clearly influenced by racial differences. Furthermore, the effect that ideology has on this relationship is suggested by the somewhat larger differences in McGovern and Nixon ratings by level of trust for

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[13] This relationship is discussed in Miller, Miller, Raine and Brown, p. 64.

[14] Jack Citrin, "Comment," this issue of the *Review*, p. 976.

Table 3. Mean Ratings of Political Leaders by Race, Political Ideology and Trust in Government, 1972

	WHITES			
	Social Change Ideology		Social Control Ideology	
	Trusting (*N*=194)	Cynical (*N*=163)	Trusting (*N*=330)	Cynical (*N*=295)
Political Leaders				
George Wallace	37	36	62	64
George McGovern	56	68	35	42
Richard Nixon	62	43	78	65
	BLACKS			
	Social Change Ideology		Social Control Ideology	
	Trusting (*N*=34)	Cynical (*N*=88)	Trusting (*N*=10)	Cynical (*N*=23)
Political Leaders				
George Wallace	19	11	35	24
George McGovern	75	80	77	76
Richard Nixon	59	36	64	55

those whites with a social change ideology compared to those with a social control ideology.

Likewise, ideology can be expected to condition the predisposition to participate in protest behaviors. The CPS questions[15] that deal with these behaviors refer principally to protest activities acceptable to individuals with a social change orientation. We thus find large differences in responses to these items by ideological orientation (see Table 4). Given that these types of protest behaviors are more acceptable to social change advocates, one would expect stronger relationships between the protest items and political cynicism for that ideological group than for those with a social control orientation. This is clearly confirmed by the data of Table 4.

The relationship between political cynicism and support for protest behaviors that is found for the social change group may, of course, possibly be a "spurious reflection of the association between mistrust of government and policy dissatisfaction."[16] That is, policy dissatisfaction may be a direct cause of both political cynicism and support for protest behavior. Thus, controlling for policy dissatisfaction would cause the relationship between cynicism and attitudes toward protest behavior to vanish. Indeed, this is what Citrin found when ideological orientations were ignored and the total sample was analyzed. Contrary to this, controlling for policy dissatisfaction *among social change advocates* did not reduce the strength of the relationship between trust and the predisposition toward protest behavior. Furthermore, the additional control actually clarified the relationship between trust and protest attitudes for social control advocates (see Table 5). Although social control advocates who were dissatisfied with Republican policies were, as a group, only slightly less negative toward protest behaviors than those with a control ideology who were satisfied with the policies, cynicism and support for protest were more strongly related among the former group than the latter.

Tables 4 and 5 demonstrate that the relationship between political trust and support for protest actions depends on a number of conditions and interactions. For individuals with a social

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[15] The political protest questions were worded as follows:

> There are many possible ways for people to show their disapproval or disagreement with governmental policies and actions. I am going to describe three such ways. We would like to know which ones you approve of as ways of showing dissatisfaction with the government, and which ones you disapprove of.
>
> 1. How about taking part in protest meetings or marches that are permitted by the local authorities? Would you approve of taking part, disapprove, or would it depend on the circumstances?
> 2. How about refusing to obey a law which one thinks is unjust, if the person feels so strongly about it that he is willing to go to jail rather than obey the law? Would you approve of a person doing that, disapprove, or would it depend on the circumstances?
> 3. Suppose all other methods have failed and the person decides to try to stop the government from going about its usual activities with sit-ins, mass meetings, demonstrations, and things like that? Would you approve of that, disapprove, or would it depend on the circumstances?

[16] Citrin, p. 982.

Table 4. The Relationship Between Political Protest and Political Trust by Ideological Orientation, 1972

	Social Change Ideology			Social Control Ideology		
	Trusting	Cynical	Total[a]	Trusting	Cynical	Total[a]
Legal Protest Marches:						
Approve	31%	44%	38%	12%	13%	12%
Depends	44	40	42	33	37	35
Disapprove	25	16	20	55	50	53
	100%	100%	100%	100%	100%	100%
(*N*)	(178)	(233)	(536)	(390)	(330)	(960)
Refusal to Obey Unjust Laws:						
Approve	22%	35%	30%	10%	13%	11%
Depends	42	44	43	33	36	35
Disapprove	36	21	27	57	51	54
	100%	100%	100%	100%	100%	100%
Disruptive Sit-ins, Demonstrations:						
Approve	9%	23%	16%	4%	6%	5%
Depends	46	46	46	20	26	22
Disapprove	45	31	38	76	68	73
	100%	100%	100%	100%	100%	100%

[a] Total distribution is for all cases with the specified ideological orientation including those in the middle category of trust. The center category on the trust scale was excluded from the table because the relationships are linear.

change ideology, the interaction of the ideological orientation and distrust of the government was sufficient to result in positive predispositions toward protest behavior. For those with a social control ideology, however, a moderate degree of policy dissatisfaction was also needed before distrust of the government was translated into a more positive attitude toward protest actions. Most assuredly, dissatisfied "cynics of the right" were not as favorably disposed toward protest participation as dissatisfied "cynics of the left," but they were more supportive of illegal protest behaviors than their ideological counterparts who were satisfied with Republican policies.

There are numerous other social and psychological conditions that can be expected to affect the trust/behavior relationship. The preliminary analyses presented here demonstrate that a superficial investigation of these complex relations would only serve to confuse the issue. Perhaps the brief analysis of this "Rejoinder" will stimulate greater concern for those conditions that affect the translation of political disaffection into political behaviors.

Political Trust and Public Policy

Political disaffection is a complex phenomenon that almost certainly depends on the convergence of different explanations rather than on one single cause. Thus, while policy dissatisfaction has been isolated as a strong correlate of political cynicism, no one would claim that it is the only explanation of political distrust. Yet, in an age when government policy impinges directly on the citizen's life in so many ways, it seems reasonable that public policy itself would weigh heavily as a determinant of discontent. Furthermore, in a period of increased issue politics and issue ideology it is not surprising that individuals would evaluate government performance with respect to a broad range of public policies. This relationship between judgments of governmental policy performance and distrust is affected by a number of social and political conditions which were discussed throughout "Political Issues and Trust in Government." It seems appropriate, however, to state some of these conditions more explicitly, especially as they apply to dissatisfaction with the policies of both parties.

Policy dissatisfaction may be conceptualized as a function of the discrepancy between one's own policy preference and the policy alternatives offered by political authorities on salient issues. Under this definition, an individual may prefer centrist policies and yet be dissatisfied if the policy alternatives he identifies with the authorities are either more liberal or more conservative than he prefers. Policy dissatisfaction, in other words, is not simply dependent on the individual's own policy position. In a polarized society where the political authorities are perceived as offering centrist policies we should find, however, that both "liberals" and "conservatives" are more dissatisfied than "moderates." If there is a correlation between policy dissatisfaction and political cynicism, we should also expect these groups to display similar relative degrees of distrust. These relative degrees of aggregate dissatisfaction and distrust will, therefore, be a function of the extent to which the various sets of authorities offer viable policy alternatives. For example, if Democrats and Republicans offer nearly identical policy alternatives, dissatisfaction and discontent should be directed equally at both of them.

Generally, the relationship between dissatisfaction with the policies of both parties and distrust of government will be affected by several factors: the saliency and relative importance of particular issues, the degree of polarization on each issue, and the relative acceptability of the policy alternatives offered by each party. The hypothesis proffered by Citrin that policy satisfaction and political trust are synonymous with support for the incumbent president and that changes in the partisan focus of distrust reflect "out-of-power" disgruntlement can also be considered an explanation of the relationship.

The attempted replication of Table 8 from "Political Issues and Trust in Government" in the "Comment" demonstrates that in 1972 political cynicism was more strongly related to dissatisfaction with Republican than Democratic policies. In 1970 dissatisfaction with Democratic policy alternatives had been almost as strongly correlated with cynicism as was dissatisfaction with Republican policies. The change in this relationship can be partially understood by disaggregating the sample into the racial and partisan subgroups analyzed in Tables 6 and 7 of "Political Issues and Trust in Government." Mean cynicism by dissatisfaction with the policy alternatives identified with the Democratic and Republican parties is presented for these groups in Table 6.

The data of Table 6 demonstrate that the reduction in the relationship between dissatisfaction with Democratic policies and cynicism occurred primarily for Republicans and Independents. The 1972 relationship for white Democrats, however, was only slightly weaker than that found in 1970. Similarly, while there was a reduction from 1970 to 1972 in the relationship for black Democrats, the correlation remained moderately strong and almost equal to that found for dissatisfaction with Republican policies.

Apparently, the policies of the Democratic party had become less viable between 1970 and

Table 5. The Relationship Between Political Protest and Political Trust by Policy Dissatisfaction for Social Control Ideologues Only, 1972

	Satisfied with Policies			Dissatisfied with Policies		
	Trusting	Cynical	Total[a]	Trusting	Cynical	Total[a]
Legal Protest Marches:						
Approve	14%	14%	14%	9%	15%	12%
Depends	36	37	37	30	38	36
Disapprove	50	49	49	61	47	52
	100%	100%	100%	100%	100%	100%
(*N*)	(195)	(102)	(409)	(94)	(126)	(295)
Refusal to Obey Unjust Laws:						
Approve	8%	10%	9%	5%	16%	10%
Depends	32	33	33	34	39	38
Disapprove	60	57	58	61	45	52
	100%	100%	100%	100%	100%	100%
Disruptive Sit-ins, Demonstrations:						
Approve	3%	8%	5%	4%	9%	6%
Depends	16	15	16	20	22	22
Disapprove	81	77	79	76	69	72
	100%	100%	100%	100%	100%	100%

[a] The total is for all cases with the specified ideological orientation and level of policy satisfaction.

Table 6. Mean Cynicism by Dissatisfaction with Democratic and Republican Policies

	White Democrats		Black Democrats		Republicans		Independents	
	Mean[a]	%	Mean	%	Mean	%	Mean	%
Distance from Democratic Party								
0	1.98	(11.4)	4.00	(16.1)	2.27	(6.0)	2.36	(7.9)
1	2.45	(44.0)	3.19	(46.2)	1.89	(29.1)	2.46	(37.2)
2	2.40	(27.8)	3.60	(31.3)	1.97	(34.8)	2.35	(31.4)
3	2.78	(11.0)	4.35	(6.4)	2.17	(17.5)	2.42	(16.3)
4–6	2.66	(5.8)	[b]	—	2.40	(12.6)	2.63	(7.2)
Group Mean and Total Percent	2.51	100.0	3.59	100.0	2.10	100.0	2.45	100.0
(*N*)	(909)		(180)		(612)		(874)	
Eta	.15		.24		.09		.04	
Distance from Republican Party								
0	2.00	(13.0)	[b]	(0.8)	1.76	(26.5)	1.44	(12.6)
1	2.04	(31.7)	2.67	(11.5)	1.92	(49.0)	2.14	(39.8)
2	2.54	(31.0)	3.25	(23.6)	2.73	(17.6)	2.51	(26.8)
3	2.89	(13.6)	3.59	(21.1)	2.11	(5.9)	3.06	(11.9)
4–6	3.71	(10.7)	3.86	(43.0)	3.25	(1.0)	3.66	(8.9)
Group Mean and Total Percent	2.51	100.0	3.59	100.0	2.10	100.0	2.45	100.0
(*N*)	(909)		(180)		(612)		(874)	
Eta	.29		.32		.20		.35	

[a] The cynicism scale ranged from 0 = least cynical to 5 = most cynical.
[b] *N* was less than 5.

1972 as alternatives to Republican policies for Republicans and Independents. These groups were slightly less satisfied with Democratic policy alternatives in 1972 than they had been in 1970 and slightly more satisfied with Republican alternatives, resulting in higher trust, on the average, for Republicans and Independents. Both groups perceived the Democratic party as further *left* on the issues in 1972 than either group had perceived them in 1970.[17] With the leftward shift of Democratic policy alternatives, both parties were thus no longer equally centrist and Democratic alternatives therefore became less meaningful than Republican alternatives as a reference for policy judgments among Republicans and Independents. In this regard it is most important to note that from 1970 to 1972 the association between dissatisfaction with Republican—or Nixon's—policies and political cynicism remained basically unchanged for Republicans, not surprisingly since that party is the focus of their partisan identification. The correlation increased, however, for Independents—as would have been predicted by the shifting reference hypothesis. These data support the "viability of alternatives" argument and illustrate the weakness of the "out-party" disgruntlement hypothesis, namely, that it does not explain distrust among Republicans.

Further evidence of change in the conditions that would affect the relationship between policy dissatisfaction and cynicism comes from attitudinal shifts occurring among Democrats. Between 1970 and 1972 Democratic attitudes became somewhat more liberal on a number of important issues, especially the Vietnam war. These shifts did not, however, parallel the change in the perception of the Democratic policy alternatives, nor did they eliminate the profound issue polarization that had existed in Democratic ranks since 1968. In fact, the net result of these aggregate shifts was, for Democrats, an increased dissatisfaction with Democratic party policy alternatives. Black Democrats, in particular, exhibited a substantially higher degree of dissatisfaction with Democratic policies in 1972 than they had in 1970. They also registered a large increase in dissatisfaction with Republican policies and an aggregate *increase* in political distrust. Among white Democrats, on the other hand, there was a decrease in satisfaction with Democratic policies, a slight enlargement in the proportion most satisfied with Republican policies, and no change at all in the aggregate level of distrust. In general, then, aggregate shifts in mean cynicism coincided closely with aggregate changes in policy dissatisfaction.

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[17] The average location of the Democratic party on the five issues of Vietnam, Urban Unrest, Campus Unrest, Protecting the Rights of the Accused, and Government Aid to Minorities was 3.49 in 1970 and 3.25 in 1972 for the total population. In 1970, Independents had placed the Democratic party, on the average, at 3.58 and in 1972 they located it at 3.22. Republicans placed the Democratic party at 3.20 in 1970 and 3.02 in 1972. For a further discussion of these shifts, see Miller, Miller, Raine and Brown, pp. 13–16.

The strength of the 1972 relationship between dissatisfaction with Democratic policies and cynicism was only slightly weaker for white Democrats than it had been in 1970, and there was no noticeable change at all in the relationship with the Republican policy dissatisfaction measure. Similarly, for black Democrats it was not surprising to find that cynicism was still related to dissatisfaction with Democratic *and* Republican policies since for them the policies of both parties had declined in acceptablity. The strength of the relationships had, however, decreased from 1970.[18] Despite these slight fluctuations, the relationships remained significant demonstrating the importance of dissatisfaction with Democratic policy alternatives as a source of political discontent for certain subpopulations.

Fluctuations in the strength and partisan focus of these relationships may also reflect the increased importance of other issues or factors having an independent effect on political trust. The relative saliency and importance of various political issues may influence the relationship between policy dissatisfaction and political trust. For example, by 1972 the Vietnam war had, at least for Democrats, become "Mr. Nixon's" war—a change that could be expected to alter the partisan focus of policy dissatisfaction. Similarly, if trust in government was primarily a statement of support for the incumbent authorities—in this case President Nixon—we might expect an increased partisan effect during presidential campaigns. Some brief but very persuasive data relevant to these points are presented in Table 7.

Table 7. Regression Analysis Predicting Political Cynicism, 1972

Independent Variables	Beta
Dissatisfaction with administration policies regarding:	
Vietnam	.11
Pollution	.01
Urban Unrest	.07
Campus Unrest	.07
Rights of Accused	.07
Assistance to Minorities	.02
The Economy[a]	.26
Nixon thermometer rating	.10
Party identification	.02
	$R^2 = .24$

[a] This is a restricted variable in the 1972 election study. The wording of the question was "How do you feel about what our government is doing about the economy—jobs, prices, profits?" The responses ranged from 1 = Delighted to 7 = Terrible.

The standardized regression coefficients presented in Table 7 clearly demonstrate that although they are intercorrelated, both support for the President and policy evaluations are independent predictors of political trust.[19] Obviously, policy dissatisfaction is not synonymous with presidential support, nor is it simply a statement of partisan identification. Likewise, trust in government is not just a function of presidental approval or party identification; it is, rather, primarily associated with policy evaluations. The policy performance and dissatisfaction measures alone can account for 20 per cent of the variance in political trust. At the zero-order level Nixon's rating, however, explains less than ten per cent of the variance in trust ($r=.31$), and it adds less than four per cent of explained variance to the multiple regression.

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[18] Other factors, such as increased group identification among blacks, were substantially affecting black cynicism in 1972. Blacks who were not necessarily dissatisfied with the particular policy items analyzed were becoming distrusting for other reasons, thereby reducing the clarity and linearity of the policy dissatisfaction/cynicism relationship. The curvilinearity of the 1972 relationship between dissatisfaction with Democratic policy alternatives and trust partially reflects strength of party identification and the associated desire to replace Republican policies with Democratic policies. The curvilinearity also results, however, from perceived discrimination and patterns of social integration among blacks. For a discussion of these sources of black political cynicism, see Arthur H. Miller, Alden S. Raine and Thad A. Brown, "Racial Trends in Political Estrangement, 1958–1972," (unpublished paper, The University of Michigan, Center for Political Studies, 1974).

[19] The Nixon thermometer rating, rather than the performance evaluation item Citrin used in his Table 2, was used for the regression analysis because the performance question was only asked of half the 1972 sample and thus could not be correlated with all of the issue measures included in the regression analysis. It should be noted that the Nixon thermometer and the performance evaluation questions were strongly correlated ($r = .68$) for that half-sample that presented both items; and the zero-order correlations of each item with trust were nearly identical (.31 and .32, respectively). It is therefore unlikely that the regression results would have differed had the performance question been used in place of the thermometer.

Clearly, support for Nixon in 1972 was only partially dependent on the policy performance of his administration, whereas political trust was strongly affected by policy evaluations, thus making possible "the coexistence of widespread support for the President and a pervasive mistrust of 'government.'"[20] This "coexistence" of presidential support and mistrust of government cannot be explained by what Citrin contends is a "short-term rationalization of an anti-McGovern vote."[21] because the same phenomenon occurred in 1970, long before McGovern was even a candidate. Since the political focus of discontent was obviously not just the incumbent authorities, there is no inconsistency in finding the coexistence of a popular president, as Nixon was in 1972, and pervasive mistrust of government. Much more than a friendly face in the Oval office is necessary for government to be judged responsive and trustworthy.

"Out-party disgruntlement" and partisan identification appear to be weak explanations for shifts in the partisan focus of political discontent. The regression analysis of Table 7 suggests, however, that a partial explanation may be found in the relative importance of various issues. In 1970, the social issues were the primary focus of policy discontent. In 1972, dissatisfaction with the policy alternatives offered as solutions to social problems (which included Vietnam) remained an important source of distrust. At the same time, although the 1972 survey predated the energy crisis, economic problems had also become a substantial source of discontent. A regression analysis including only the 1972 social issues explained 15 per cent of the variance in trust; an additional five per cent was explained by incorporating evaluations of how well the government was handling economic problems.

We would expect economic discontent that arises during the term of a particular administration to be principally directed at the performance rating of that administration. Only if the economic dissatisfaction persisted across different administrations or became part of a generalized policy dissatisfaction would we expect economic discontent to be directed at both parties equally. An increase in the relative importance of economic dissatisfaction between 1970 and 1972 may thus be a partial explanation for the shift in the partisan focus of political discontent that occurred during that period.

In summary, the parties were perceived as having shifted in their issue positions so that they were no longer seen as equally centrist; there had been a slight reduction in the polarization of the *total* population on some issues; and the relative importance of various issues had apparently changed. These factors ostensibly affected the partisan focus of policy dissatisfaction and, in turn, the relationship between policy judgments and political trust. A decline in the relationship between dissatisfaction with Democratic policy alternatives and political cynicism was, therefore, not unexpected. Furthermore, some groups still felt that the policies of both parties were somewhat unacceptable, and for them dissatisfaction with the policies of *both* parties was still related to distrust. This further analysis, therefore, confirms the conditions previously hypothesized in "Political Issues and Trust in Government" as affecting the relationship between policy dissatisfaction and political cynicism. More importantly, the analysis reconfirms policy dissatisfaction as a major source of political discontent.

Conclusion

Professor Citrin has suggested that declining trust is just a reflection of dissatisfaction with the incumbent; that political trust should correlate with political action but that the trust index does not; and that centrist policies are not a cause of political distrust. The analysis presented in this "Rejoinder" demonstrates, however, that while distrust of the incumbent authorities exists, other forms of discontent predominate and a lack of citizen fervor to replace the regime does not mean that distrust is ritualistic. Furthermore, the trust scale does correlate with political actions and support for illegal protest behaviors under appropriate conditions, but care must be taken to apply the relevant controls. Simple relationships may not be present, but relationships emerge when properly examined. Finally, the change between 1970 and 1972 in the relationship between policy dissatisfaction and trust is due to the Democrats being *perceived* as less centrist, which explains why trust no longer correlates with dissatisfaction with their policy alternatives. Again, careful indication of specific control conditions are essential to clarify the relationship. In sum, the analysis results demonstrated the validity of the trust in government scale as a meaningful measure of political discontent. Moreover, they also confirmed the political relevance, importance and validity of the ideological orientations specified in "Political Issues and Trust in Government." Ignoring these orientations not only detracts from our recognition of the serious social and political implications of distrust, but it also limits our comprehension of the behavioral implications of political discontent.

A little common sense and a great deal of data suggests that there is much discontent with numerous facets of government in the U.S. today. To conclude, therefore, that a ten-year trend of declining confidence in government indicates little more than a shift in superficial or "ritualistic" verbal responses is not to just call into question particular survey methods. To do so is, rather, to ignore the political turmoil that has polarized this country since the mid-sixties, and, more importantly, it suggests that the turbulence and distrust spring from superficial rather than real social problems. The civil rights movement, the peace movement, and the women's movement express a discontent that grows out of repressive and discriminatory practices which require institutional and structural changes if permanent rectification of the situation is to be attained. Both positive attitudes towards these movements and negative reactions to them are related to distrust of government; to deem these attitudes and reactions "ritualistic" is tantamount to treating the events, experiences and evaluations of government performance that cause the distrust as fictions and abstractions.

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[20] Citrin, "Comment," pp. 976–978.

[21] Citrin, "Comment," p. 978.

More extensive work is needed if we are to fully explain how individual experience is translated into societal and political discontent. This is a timely and relevant problem that deserves serious consideration. A more complete understanding of political discontent can best be attained through careful analysis of trend data. Analogies between baseball and politics are weak and rarely help us understand either, although I would remind Professor Citrin that the endless changes of baseball managers and players in which many teams engage have not brought back the fans who became disinterested in the game because of its inherent structural flaws—for example, its lack of speed. Furthermore, the visible actors in baseball have little if any impact on the rules of the game; in politics, they make the rules. A change in the political leadership, therefore, holds the potential for profound systemic political change which may in turn increase confidence in government. On the other hand, a replacement of political leaders with no subsequent improvement in the performance of the government may generate a new spiral of political distrust. As noted at the outset of this rejoinder, democracy rests upon the trust that citizens extend to their government; when that trust is undermined, the whole system of government is threatened.

Theory and Voting Research: Some Reflections on Converse's "Change in the American Electorate"

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Background to a Controversy

In his brilliant and justly famous monograph, *The Structure of Scientific Revolutions,* Thomas S. Kuhn has illuminated the sociology of scientific discovery.[1] Developing an antinomy between "normal science" and "scientific revolutions," he places great stress upon the crucial importance of the discovery of anomalous data—data which do not "fit" and cannot be made to fit in preparing the ground for the overthrow or supersession of currently dominant scientific paradigms.[2] This dialectical pattern—paradigm, anomaly, resistance, revolution—appears to dominate the history of science.

No one pretends, or should pretend, that most areas of political science, including voting-behavior research, have advanced to the point that true paradigms have been constructed, or that professional controversies over the nature of political reality have more than a closely analogous relationship to the kinds of natural-science dynamics which Kuhn has brought to our attention. Ours is a more modest enterprise than this. But the analogies seem close enough to provide a starting-point for this explicitly controversial (or controversy-oriented) paper.

In 1965 the *Review* published a paper of mine, "The Changing Shape of the American Political Universe."[3] This paper presented a theory of American electoral change, which stands in some contrast to the then-current professional emphasis on political system. Moreover, it presented data which were anomalous in light of the then-received theories of American voting behavior—many if not most derived from survey research on the contemporary electorate. Such anomalies included the steep decay in post-1900 voting behavior by nineteenth-century American or contemporary European standards, sharp increases in partisan volatility, and other indicators of growing "marginality" preceding chronologically *pari passu* with the shrinkage of the "voting universe." From this it was argued that the establishment of industrial-capitalist political hegemony after the realignment of 1896 was closely and causally associated with these remarkable movements, movements which were markedly retrograde to electoral development elsewhere in the Western world. The inference was drawn that this erosion of the mass base of American politics was a consequence of the tension between capitalism and democracy, and of the ascendancy of capitalism.

This presentation brought forth other anomalous findings in terms of contemporary survey-research theory. (1) "Landslides" in the nineteenth century tended, as did much else in the period, to be inverses of the contemporary behavioral phenomenon—they were not the product of "surges" as much as of one-sided abstentions by a normally involved partisan electorate. (2) Rural voting behavior in the contemporary period tends to reveal eccentricities in turnout and in partisan alignment. In the last century, by contrast, agrarian turnout tended to be more invariant and normally a good deal higher than in cities; rural partisan shifts were correspondingly very small except during realignment sequences. (3) The oscillatory or "sawtooth" pattern in turnout between presidential and off-year elections tended to be much smaller in relative amplitude before 1900 than is the case today. This is another way of saying that the relative net proportion of marginal or "in-and-out" voters was much smaller then than it is now, despite the much higher turnout rates in presidential elections. (4) The educational level of the American population has steadily increased over the past century. Yet despite this increase, voting participation went into a steep decline early in this century from which it has only marginally recovered. As is well known, educational level is a strong predictor of voter turnout in the contemporary era. An anomaly exists here which is disturbing to the SRC's paradigm.

This list could be extended, but it is at least indicative of the analytical problem. Perhaps the most disturbing empirical finding of this piece was that the indicators of nineteenth-century voting behavior, taken as an integrated set of characteristics, point to an electorate much more involved in democratic politics than its present counterpart, and vastly more so than that of the 1920s.

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[1] Thomas S. Kuhn, *The Structure of Scientific Revolutions,* 2nd ed. (Chicago: University of Chicago Press, 1970).

[2] *Ibid.,* pp. 52–65.

[3] *American Political Science Review,* 59 (March, 1965), 7–28.

At the very base of these anomalies and the conclusions drawn from them is the author's belief that sociological and historical contexts profoundly shape both political consciousness and political behavior. This suggests a plasticity, as well as a sense of potential cognition of political objects within a mass electorate, which is most uncongenial to the semi-paradigms of contemporary survey research.[4] In particular, such a suggestion casts serious doubt upon the generalizability of many—though far from all—survey findings based on contemporary American (and other) voters. Resistance to incorporation of these anomalies and their implications has been correspondingly strong. It has, moreover, been greatly facilitated by a crucial deficiency in the original (1965) presentation: that work failed specifically to incorporate changes in electoral mechanics after 1890, which are of obvious significance in shaping subsequent voting behavior and nonbehavior.

Now, when one is confronted with anomalies, it is possible to assimilate them into an existing paradigm in several ways. For example, those anomalies which have to do with change toward a present condition can achieve explanation through incorporation of straightforward and "undramatic" structural change unaccounted for in the original presentation of the anomaly. Similarly, those anomalies which have to do with the nature of reality before the onset of system change, in cases where their empirical reality cannot be disputed, may be explained away through hypotheses positing that they are artifacts or "optical illusions." Such efforts will, of course, be resisted by those who discover anomalies. The conflict which necessarily arises is most certainly not a matter either of ill will or of differences in intelligence: it is a matter of differences in reality perception. The conflict between anomaly and existing paradigm is a key vehicle for the development of fruitful scientific controversy, and such controversy in turn is a most valuable, indeed necessary, step in the development of better approximations to reality than any which currently exist.

Philip E. Converse has recently written an essay, "Change in the American Electorate," which has the perceptiveness and elegance of style and insight which we have come to expect from him.[5] The first half of this absorbing study consists of a detailed exposition and critique of my 1965 "Changing Shape" article and some more recent reformulations of its thesis. The differences between us, while most cordial, are profound and substantive enough to merit an examination by me of this counter-hypothesis.

Converse's hypotheses fall into two fairly discrete sets: those dealing with intervening legal or rule variables which may explain most of the observable change, and those which raise questions about the politicized character of the nineteenth-century voting universe.

The first set of hypotheses includes the intervening variables of (1) women's suffrage (1914–1920); (2) the introduction of the Australian ballot (ca. 1889–1896); and (3) the introduction of personal-registration requirements for voting (ca. 1900–1920). Since the first variable, that of women's suffrage, was mentioned and in a sense "controlled for" in the 1965 article, the only point discussed here has to do with its possible contribution to the notorious electoral volatility of the 1920s. The second has partially been discussed elsewhere in light of Jerrold Rusk's extensive review article on the subject in *The American Political Science Review* and will receive limited treatment here.[6] The third—the personal-registration requirement—will receive much closer analysis in this paper.

The second set of Converse's hypotheses, if I have read him correctly, includes: (1) the primitive level of social bookkeeping in the nineteenth century, which casts some doubt upon turnout figures themselves; (2) the widespread existence of corruption at the polls, especially that involving fraudulent surplusage of votes, a corruption which is suggested to have been very widespread in rural as well as urban America before the turn of this century; (3) discussion of the causes of higher rural than urban turnout in contemporary underdeveloped countries (the Philippines and Turkey), based on survey work there, is presented as though somehow relevant to the American nineteenth-century case.[7] Let us evaluate each of these in turn, with such evidence as is currently available.

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[4] It is becoming increasingly apparent that the survey-research profile of individual voters laid down by the Michigan team in *The American Voter* (New York: Wiley, 1960), and in their *Elections and the Political Order* (New York: Wiley, 1966), must be significantly modified for the post-1964 period—and on the basis of survey analysis itself. See, for example, Gerald Pomper's two articles, "Toward a More Responsible Two-Party System? What, Again?", *Journal of Politics,* 33 (November, 1971), 916–940, and "From Confusion to Clarity: Issues and American Voters, 1956–1968," *American Political Science Review,* 66 (June 1972), 415–428; also, Norman Nie, "Mass Belief Systems Revisited: Political Change and Attitude Structure" (mimeo draft, August 1972); and especially Arthur H. Miller, Warren E. Miller, Alden S. Raine and Thad A. Brown, "A Majority Party in Disarray: Social and Political Conflict in the 1972 Election" (paper presented at the 1973 meeting of the American Political Science Association). In view of these findings, which are of course congenial to the arguments made in this paper, it seems about time that we had a complete retesting or replication of *The American Voter,* and one which is relevant to the contemporary electoral scene.

[5] Philip E. Converse, "Change in the American Electorate," in *The Human Meaning of Social Change,* ed. Angus Campbell and Philip E. Converse (New York: Russell Sage Foundation, 1972), chapter 8 (pp. 263–337). The controversy at issue here is centered in the first half of Converse's essay, pp. 263–301.

[6] Jerrold G. Rusk, "The Effect of the Australian Ballot Reform on Split-Ticket Voting: 1876–1908," *American Political Science Review,* 64 (December, 1970), 1220–1238. Also see the exchange of views between Rusk and Burnham in this *Review,* 65 (December, 1971), 1149–1157.

The Way Things Were? An Exploration of Three Structural-Change Hypotheses

The Influence of Personal-Registration Statutes: An Examination of Turnout Decline in Four States, 1900–1916. There is an extraordinary messiness in the analysis of many variables affecting historical American voting behavior. In the first place, since social bookkeeping in this country is in many respects primitive compared with Europe or even Canada, we are left in ignorance as to the precise number of potential voters, or even of eligibles, in any election.[8] If we are to make any sense of the data at all, it is necessary to construct after-the-fact estimates of the potential electorate at given times, controlling as best we can for large potential inflators, such as noncitizen males in states where only citizens had the right to vote. These estimates must in most cases be based on linear interpolations between decennial federal census years. Thus, in addition to the serious problem of accuracy in enumeration (most serious, apparently, in the censuses of 1870 and 1920), it seems reasonably certain that such estimates of the potential electorate are most likely to be relatively more accurate the nearer in a decade one is to the preceding or succeeding census, and least likely to be so at midpoints. Moreover, as any analyst of historical data can attest, it is not easy to identify the precise legal status of election regulations in the past. Still more problematic, once one has identified at least the approximate date and coverage, say, of personal-registration statutes, is the issue of the local effectiveness of such requirements. These aspects of American bookkeeping present such difficulty that one could well be pardoned for abandoning any effort to define change in electoral participation and its components in any way which could generate broad acceptance of the accuracy of the results. Still, best approximations are considerably better than nothing, even if they are relatively crude; and one may hope that whatever errors exist tend randomly to cancel themselves out.

With all of these limitations, the initial structure of the personal-registration legislation adopted at the turn of the century permits a straightforward statistical testing of the hypothesis that it accounted for the lion's share of the post-1900 decline in American voting participation. Such laws, of course, must themselves be integrated into a conceptual pattern of system change in response to stress: they, too, represent change produced by concrete men acting out their ideological biases in concrete legislative form. This is nowhere more evident than in the fact that, in a great many states, the first such laws were applied initially against the populations of cities while nonurban areas were left without such coverage.[9] But this pinpointed registration coverage permits us to decompose changes in the turnout rate by county and thus to establish some relatively satisfactory measures of differential change. Indeed, it permits us to make a first, if crude, approximation to the probable relative contribution of personal-registration requirements to post-1900

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[7] Converse, "Change in the American Electorate," pp. 287–288.

[8] To take but two *contemporary* examples of this problem: As of the 1970 election, there were no personal-registration requirements at all in 37 (or 42.0 per cent) of Ohio's 88 counties, partial registration for small cities in another ten (11.4 per cent), and countywide registration in the remaining 41 (46.6 per cent). In such a case, of course, no uniform standard exists even at the level of enumeration of electors provided by registration lists. Moreover, while Ohio gives the total number of ballots cast for its cities, it provides no such information for its county returns. In Massachusetts, on the other hand, complete information concerning the numbers of registered voters is published for each town and precinct; and the total number of votes cast, including blanks, is reported by town along with the vote for each candidate. Even in this case, of course, there is no indication of the number of potential voters—those who met legal requirements for the suffrage, but who did not register in the first place. In most European elections and in Canadian elections as well, it is possible to identify: (a) the total eligible electorate; (b) the total vote cast; (c) the number of invalid ballots cast; (d) the total valid vote cast; (e) the partisan distribution of that vote. For a more detailed discussion of heterogeneity and estimation problems in American Electoral Reporting, see the author's "Estimates of Potential Electorate," Introduction for Section Y, 1974 edition of U.S. Census Bureau, *Historical Statistics of the United States* (forthcoming).

[9] This urban/nonurban stratification has remained quite widespread down to the present day, particularly in Midwestern states with many low-density rural counties. In addition to the Ohio case mentioned in Footnote 7, it is worth noting that as late as 1960, 87 of Missouri's 115 counties had no personal-registration requirements. As a group, these counties had turnout rates about 12 per cent higher than those of the state's two major metropolitan centers. In New York, as of 1949, 10 counties had no personal-registration coverage at all, while a majority of election districts in 35 additional counties also had no such coverage. In 1967 a permanent personal-registration statute was adopted for the entire state.

Table 1. Registration and Voting Turnout: Pennsylvania Presidential Elections, 1889–1968, By Extent of 1920 Personal-Registration Coverage

	Means					Variances				
Year	M_1	M_2	M_3	M_4		V_1	V_2	V_3	V_4	$F=$
1880	90.0	91.8	93.2	91.3		23.67	24.23	30.18	34.27	0.405
1884	81.0	84.7	85.3	86.9		72.77	43.23	13.16	61.68	1.417
1888	76.7	82.0	84.0	85.6		113.86	66.05	9.93	51.48	3.253
1892	73.7	74.7	78.6	79.5		79.43	67.94	18.95	50.28	2.338
1896	82.2	83.1	86.3	85.5		49.84	70.57	5.29	36.58	1.024
1900	75.3	78.4	79.3	81.2		87.97	23.94	10.71	37.58	2.323
1904	72.9	74.2	75.6	76.5		66.96	47.59	56.68	81.12	0.538
1908	68.1	73.5	75.8	77.7		61.25	41.71	130.45	41.14	4.340
1912	61.2	67.0	66.5	70.4		31.96	24.75	76.01	32.45	5.841
1916	60.6	64.3	64.9	69.2		26.78	23.50	23.68	43.69	5.912
1920	40.4	41.3	45.2	48.2		36.67	16.45	25.25	43.65	7.099
1924	42.4	46.5	48.5	52.8		10.23	31.50	10.91	49.68	8.473
1928	58.9	63.0	64.1	65.8		24.56	25.93	79.20	47.12	2.629
1932	48.1	51.4	55.2	61.5		15.07	29.76	16.27	60.13	14.070
1936	68.5	69.5	73.9	79.9		61.13	28.86	10.38	55.15	12.048
1940	65.1	63.3	65.1	68.8		52.58	24.46	11.67	52.80	2.947
1944	57.9	55.9	55.6	58.7		41.45	13.44	13.45	44.93	1.095
1948	53.1	51.1	52.1	52.1		80.54	34.36	26.52	56.83	0.153
1952	64.4	62.9	64.8	64.3		58.60	16.63	26.37	59.22	0.210
1956	63.1	62.8	63.4	66.9		48.32	21.15	14.12	63.70	1.759
1960	69.5	68.4	70.6	71.9		49.61	18.38	20.11	52.99	1.132
1964	66.7	63.1	65.7	67.0		44.48	21.82	7.92	47.69	1.455
1968	64.6	61.2	65.1	64.7		39.29	28.70	8.98	46.54	1.300
$N=$	8	15	7	37	d.f. =	7	14	6	36	

turnout declines in the metropolitan areas themselves. Four states have been examined by this method: Pennsylvania, New York, Ohio, and Missouri.

One major point about which we must be clear in measuring longitudinal change is that statistical measures will produce quite discrete findings depending upon the observer's analytical focus. This is not to say, with the conventional popular wisdom, that "there are lies, damned lies, and statistics," but rather that two apparently discordant readings can often be reconciled by, for example, bearing in mind the distinction between longitudinal and cross-sectional orientations. Thus, in the case of Pennsylvania, we have chosen to begin the discussion with analysis of variance. The analysis of variance is based upon a stratification of the state's 67 counties in terms of the proportion of their populations covered in 1920 by the state's 1906–1937 personal-registration requirements.[10] (Tables 1 and 2).

While there are some occasional "sports" in the presidential-election array, the values of the F ratio are generally gratifyingly high for the 1908–1936 period, that covered by the 1906 statute. Moreover, these values tend to show longitudinal increase from the beginning to the end of the period. They are correspondingly lower —usually well below the level of statistical significance—both for the 1880–1904 and for the 1940–1960 periods. On the presidential level, at least, it can be taken as extremely probable that selective personal-registration requirements did produce a statistically significant effect in differentially depressing turnout in the state's urban areas. There is very little doubt that similar analysis-of-variance tests performed for New York, Ohio, and Missouri would show comparable results. So we

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[10] Pennsylvania is a typical example of the personal-registration transition which is occurring gradually in Ohio and has been completed in New York. The 1906 personal-registration statute applied these requirements to cities of the third class and larger, and was replaced by a statewide registration requirement in 1937. For text of the 1906 law, see any volume of *The Pennsylvania (Smull's) Manual* between 1907 and 1935. For text of the subsequent statute, see the 1937 or succeeding volumes of this publication.

Table 2. Registration and Voting Turnout: Pennsylvania Gubernatorial Elections, 1890–1918, By Extent of 1920 Personal-Registration Coverage

	Means					Variances				
	M_1	M_2	M_3	M_4		V_1	V_2	V_3	V_4	$F=$
1890	69.4	73.2	77.7	79.1		18.33	18.41	5.60	59.72	4.273
1894	66.6	70.0	72.2	74.4		16.62	32.23	4.90	73.10	1.666
1898	62.4	67.4	69.2	72.3		14.65	20.95	7.29	62.15	3.751
1902	65.1	66.2	68.3	71.3		11.21	21.67	9.93	68.99	1.976
1906	54.1	59.1	60.3	63.7		13.89	21.27	9.95	74.18	2.721
1910	50.6	53.8	56.2	55.8		12.23	13.83	8.64	50.22	1.570
1914	50.7	56.5	59.5	61.6		7.96	15.88	9.18	48.37	7.525
1918	38.0	43.8	43.8	48.7		8.75	13.06	6.02	57.05	6.513
$N=$	8	15	7	37	d.f.=	7	14	6	36	

Throughout this period, d.f. = 3, 63. Values of the *F*-ratio at the several levels of significance with d.f. = 3, 63 are:

level	*F*
.05	2.76
.01	4.13
.001	6.17

Note: The stratification is based on registration coverage as of 1920. M_1 and V_1 are derived from counties 50% or more of whose populations were covered by personal-registration requirements. M_2 and V_2 correspond to counties 20.0 to 49.9% of whose populations were so covered; M_3 and V_3 correspond to counties 0.1 to 19.9% of whose populations were so covered; and M_4 and V_4 correspond to counties which were left unaffected by the 1906 statute (37 in all).

certainly have an intervening variable of statistical significance for subsequent outcomes. But cross-sectional analysis of variance does not tell us anything about *trends* in any of the categories analyzed. How large a share of the post-1900 turnout decline can be attributed to this variable? How important is it?

Considerations of length preclude an elaborate discussion of Pennsylvania's party history from 1890 through 1932, but we may wish to begin analysis of this question with the Pennsylvania gubernatorial data of Table 2. This is a much shorter analysis-of-variance series, but the table contains a number of obvious anomalies, and masks others. Despite the 1906 statute, the values of the F ratio do not climb to the significant range until the third election after its enactment, 1914. Indeed, the 1910 value is the lowest of the entire 1890–1918 series. The explanation of this phenomenon ties in closely with Converse's discussion. Let us summarize.

(1) Presidential years were generally years of harmony within the hegemonic Republic majority after 1896, largely because no really important statewide offices were at stake. (2) The profile of off-year electoral politics between 1898 and the First World War was one of extreme party fragmentation and massive but unsuccessful uprisings of Progressives against the machine Regulars who controlled the state's political process. (3) Within Philadelphia particularly, the 1906 registration statute utterly failed to curb fraudulent vote surplusage during the locally important off-year elections. In fact, methods of controlling votes reached new heights of sophistication and comprehensiveness precisely during this period. One result of this was that, in the 1902–1918 period, this city—which was 100 per cent covered by the 1906 statute—had a higher turnout rate in all elections but one than did a *majority* of the 37 counties where no personal registration was required at all[11] (Table 3).

[11] The effectiveness of machine manipulation of the vote in Philadelphia can best be appreciated by examining turnout and voting in two wards: the 12th, a river ward populated heavily by immigrants and solidly controlled by the city Republican organization, and the 21st, a middle-to-upper class and politically independent suburban ward.

A. Turnout and Machine Control in Philadelphia, 1922–1930

Year and Office	Ward 12 Turnout	%D	Ward 21 Turnout	%D	City Turnout	%D
1922-Gov.	61.8	6.6	30.7	28.8	31.7	20.5
1926-Pres.	74.2	3.8	47.3	15.7	43.0	12.1
1926-Sen.	78.2	1.7	37.4	29.4	35.9	19.1
1928-Pres.	63.9	28.3	78.6	35.8	63.2	39.5
1930-Gov.	61.6	96.5	49.9	60.1	41.2	75.6
1930-Sen.	61.6	6.8	49.9	23.6	41.2	21.0

Another major anomaly can be sensed in the array of Table 2, and it is important enough to require further discussion. The strenuous state conflicts of 1902–1914 pitted insurgent or Progressive Republicans against organization Regulars. The Democratic party as such was a hopeless minority throughout the post-1896 period and played an ambiguous role. In 1910, the high-water mark of local Progressive insurgency, it fielded a candidate at the behest of the Republican state machine to siphon off enough votes to permit the organization's candidate for governor to eke out a plurality.[12] On other occasions, 1906 and 1914, the party fused unsuccessfully with insurgent Republican fragments.

One could expect in this situation that elections would be locally most competitive in areas with partial or no registration, with the machine producing lopsided organization majorities in the big cities to win statewide victory. The partisan data reveal that this was exactly the case (Table 4).

We can accept that a strong machine could overcome the intentions of a personal-registration statute. But the literature also emphasizes that closeness of competition in elections tends to facilitate turnout. Here we find the opposite. Despite the closeness of contests in the nonregistration areas—indeed, all four elections from 1902 through 1914 revealed opposition *pluralities* in these counties—their decline in turnout was huge and, except for a small upswing in 1914, continuous from 1890 onward. In fact, the turnout decline accelerated markedly in 1906 and 1910, despite the intensity of local party competition, and opposition success in most of these counties. If we take all eight of the counties, more than half of whose populations were subject to registration requirements, and compare their negative 1890–1918 regression slope with that of the 37 nonregistration counties, we find that the steepness of the former is only 14.5 per cent greater than the latter on a standardized measure.

It is also worth noting that this gubernatorial regression line for the nonregistration counties of Pennsylvania is steeper than the 1900–1916 presidential election regression slopes for any category of counties in any other of our three states except for New York City; and that city, in addition to being 100 per cent covered by per-

Table 3. Extreme Cases: Philadelphia and Non-Registration County Turnout Comparisons, 1890–1918

Year	Number of Non-Registration Counties with Turnout: Lower than Philadelphia	Number of Non-Registration Counties with Turnout: Higher than Philadelphia	% of *N-R* Counties with Lower Turnout than Philadelphia
1890	9	28	24.3
1894	15	22	40.5
1898	5	32	13.5
1902	22	15	59.5
1906	19	18	51.4
1910	33	4	89.2
1914	17	20	45.9
1918	20	17	54.1

Statewide turnout rates during this period were 32.5 per cent in 1922, 45.8 per cent in 1924, 30.9 per cent in 1926, 62.6 per cent in 1928 and 40.3 per cent in 1930. The inversion of usual criteria of turnout—especially extreme nationally in the 1920s—is as remarkable in the contrast between lower-class Ward 12 and upper-middle class Ward 21 as in the extraordinary control of partisan outcomes in the former. Without further data presentation, it is also possible to make one more assertion: the decline in turnout among middle-class "independent" wards such as 21 from 1890 through 1930 was vastly heavier than in the lower-class wards which the machine controlled, in the former case approximating the rate of turnout decay in the state's 37 nonregistration counties. If the 1906 statute was designed to protect middle-class and native-stock interests against the big-city machines and their ethnic clienteles, it quite failed to achieve its purpose. In any event, there is a striking parallelism between post-1890 declines in most of those wards of Philadelphia where anti-organization sentiment was strongest and similar behavior in the nonregistration territories of the state at large. Once again—and forcefully—it suggests the intervention of a behavioral variable which can plausibly be linked to political alienation.

[12] One notes the relevant parts of the Keystone (Progressive-Republican) party platform of 1910: "A political trust, managed by cunning politicians, threatens the Commonwealth. Some of the conspirators are labeled Republicans and some Democrats, but they are all in league against the people and act in harmony with one treasonable purpose and under the orders on one head. . . . Both of the tickets nominated and both of the platforms adopted, it is well understood, were dictated by the same authority and were intended for the delusion of the voters and the further confirmation of the power of the political machine." *The Pennsylvania (Smull's) Manual*, 1911, p. 520. It is perhaps also worth noting that the Socialist party reached its off-year apogee of 5.3 per cent of the total vote in this election, with the Keystone candidate winning 38.3 per cent, the Democrat 13.0 per cent and the Republican Organization's candidate 41.6 per cent.

Table 4. Partisan Distribution of Pennsylvania Gubernatorial Vote, 1890–1918, by 1920 Registration Coverage

Area and *N*	1890	1894	1898	1902	1906	1910	1914	1918
Philadelphia (1)								
Dem.	44.3	27.6	22.3	29.8	42.4	6.8	25.2	27.5
Rep.	55.2	71.1	65.1	68.9	56.1	54.4	72.6	70.7
Prog.			12.2			35.9		
Other	0.4	1.3	0.5	1.3	1.5	2.8	2.2	1.9
50.0–99% Reg. (7)								
Dem.	52.6	33.2	39.7	39.0	42.1	12.2	41.6	39.6
Rep.	45.5	61.1	42.0	56.1	51.7	44.2	48.6	53.3
Prog.			12.4			32.3		
Other	1.9	5.8	0.9	5.0	6.2	11.2	9.8	7.1
20.0–49.9% Reg. (15)								
Dem.	49.8	37.1	39.6	45.1	47.0	13.8	46.2	34.4
Rep.	47.9	57.3	45.1	48.9	48.6	38.4	47.4	59.8
Prog.			15.1			41.9		
Other	2.3	5.6	0.3	6.0	4.4	5.9	6.4	5.7
0.1–19.9% Reg. (7)								
Dem.	53.5	39.3	41.7	46.3	45.9	12.7	44.9	37.9
Rep.	44.4	55.5	42.1	46.2	48.2	33.7	47.2	56.7
Prog.			15.5			40.5		
Other	2.1	5.5	0.7	7.6	6.0	13.0	7.9	5.4
No registration (37)								
Dem.	51.3	38.3	41.6	48.1	49.6	20.1	48.3	32.2
Rep.	46.6	56.2	45.4	47.3	46.5	32.2	46.0	61.1
Prog.			12.9			41.7		
Other	2.1	5.5	0.1	4.6	3.9	6.0	5.8	6.7

Counties falling in each classification:

100%:	Philadelphia
50.0–99.9%:	Allegheny, Berks, Erie, Lackawanna, Lawrence, Lehigh, Venango.
20.0–49.9%:	Blair, Butler, Cambria, Clinton, Crawford, Dauphin, Delaware, Lancaster, Lebanon, Luzerne, Lycoming, McKean, Mercer, Northampton, York.
0.9–19.9%:	Chester, Clearfield, Fayette, Northumberland, Schuylkill, Washington, Westmoreland.
0%:	All other counties (*N* = 37)

sonal registration, was subject to a periodic re-registration requirement as well. The turnout decline in the rural, heavily native-stock Protestant areas of Pennsylvania not covered by the 1906 statute constitutes a major limiting case on any registration-variable hypothesis. So far as we know, it was wholly "behavioral," not an artifact of rules change. Somehow, despite the intensity of insurgent activity, the terms of politics had been redefined from those of the late nineteenth century in such a way as to discourage participation in elections. What other barriers stood in the way for these voters?

From this point we can move to an examination of the longitudinal trends of the 1900–1915 period in our other states. Unfortunately, the criteria of county stratification are not entirely uniform. Accordingly, comparisons may have more limited value than the data might suggest. Still, all of these states permit some differentiation. Until the late 1960s, New York had registration provisions similar to those of Pennsylvania before 1937, and relevant volumes of the state's legislative manual provide a useful breakdown of the registration characteristics of precincts by county. Similar data are available for Ohio back at least to 1932. In the case of Missouri, the turn-of-the-century registration statute initially and for a long time afterward was for all practical purposes confined to the state's two metropolitan areas. Moreover, we are fortunate in being able to deal with secular trends of large size and some uniformity. So far as county-by-county analysis of 1900–1916 turnout rates can be made, we find no exceptions to the *fact* of decline in any of the 217 counties of New York, Ohio, and Pennsylvania during this period. There are numerous exceptions in Missouri, but these may find a subsequent and, I hope, convincing behavioral explanation.

Table 5 presents regressions on time for the

Table 5. Turnout Data for Four States, 1890–1918: Regression Metrics

I. Pennsylvania (Gubernatorial), 1890–1918				
Registration Coverage, 1920 and N		$Y_c=$	$S_{y.x}=$	$bX/a=$
0	(37)	$83.75–3.98X$	3.516	$-.0475$
0.1–19.9	(7)	$82.14–4.16X$	3.569	$-.0507$
20.0–49.9	(15)	$78.46–3.82X$	3.023	$-.0487$
50.0–100.0	(8)	$75.63–4.12X$	3.659	$-.0544$
II. Pennsylvania (Presidential), 1900–1916				
Registration Coverage, 1920 and N		$Y_c=$	$S_{y.x}=$	$bX/a=$
0	(37)	$84.03–3.01X$	1.745	$-.0358$
0.1–19.9	(7)	$83.79–3.79X$	2.051	$-.0452$
20.0–49.9	(15)	$82.10–3.54X$	1.191	$-.0431$
50.0–100.0	(8)	$79.95–4.11X$	1.472	$-.0514$
III. New York (Presidential), 1900–1916				
Mean Registration Coverage of Precincts, 1948, and N		$Y_c=$	$S_{y.x}=$	$bX/a=$
0	(10)	$90.84–2.16X$	1.914	$-.0238$
16.7	(14)	$90.89–2.23X$	2.256	$-.0245$
55.1	(33)	$91.80–3.12X$	2.427	$-.0340$
100.0	(5)*	$85.31–4.25X$	1.182	$-.0498$
IV. Ohio (Presidential), 1900–1916				
Registration category, 1932, and N		$Y_c=$	$S_{y.x}=$	$bX/a=$
None	(57)	$94.44–2.26X$	3.874	$-.0239$
Partial	(28)	$93.78–3.58X$	5.873	$-.0382$
"Full"**	(3)	$88.25–3.19X$	3.401	$-.0361$
V. Missouri (Presidential), 1900–1916				
Area and N		$Y_c=$	$S_{x.y}=$	$bX/a=$
Nonmetropolitan	(112)	$84.67–0.29X$	3.358	$-.0034$
Metropolitan***	(3)	$74.68–0.72X$	5.280	$-.0096$

* New York City. In this case, to avoid problems associated with the creation of Bronx county in 1914, the citywide total is the base.

** The three metropolitan counties, Cuyahoga, Franklin, and Hamilton. Because most suburbs and outlying areas were not initially covered, these are "relatively" full registration territories.

*** St. Louis city and county of Jackson. For all intents and purposes these were the only initial areas of personal registration in Missouri.

turnout rate during this turn-of-century transitional period, stratified by the relative extent of personal-registration coverage in groups of geographical areas (counties). In all categories and in all four states, the regression slopes are negative. But the differences among states in the slopes are considerable. Pennsylvania stands at one extreme, that of maximum decline, and Missouri reveals almost no decline at all, even in its three metropolitan counties. The other two states fall in the middle and approximately together, although somewhat closer to the Pennsylvania end of the spectrum. Indeed, a contemplation of the standardized regression slopes for these states and categories (bX/a) suggests that the *between-state* differentials are at least as significant as the registration-determined *within-state* differentials. In all of these states, to be sure, the posited relationship holds: very generally, the larger the registration coverage in a given set of counties, the steeper the negative regression slope is. But it should also be noted that the relationships are not quite monotonic either in Pennsylvania or in Ohio.

The idiosyncracies of the early personal-registration statutes make it possible to derive a first approximation of the relative contribution of the personal-registration statute to this nearly universal turnout decline. Leaving Converse's theory of apparently general rural corruption aside for the moment, the assumption is very strong that any secular trend in voting participation to be found in counties without personal-registration requirements must be attributed to behavioral factors. By definition, no known changes in electoral mechanics could be causes of such change in turnout. This being the case, let us assume for each state that the proportion of turnout decline in any area which corresponds to the rate of decline in nonregistration areas also corresponds

Table 6. A Crude Estimate of the Contribution of Personal-Registration Statutes to Turnout Decay: Pennsylvania and New York

A. Pennsylvania: Gubernatorial Elections, 1890–1918 (T to $T+7$)

Cat. of Reg. Coverage 1920	Regression line est.: "Expected" 1918	"Observed" 1918	$E(18)-O(18)$	$T-T+7$	$\frac{E(18)-O(18)}{T-T+7}$
0	51.95	51.95	0	27.83	0
0.1–19.9	50.78	48.83	1.95	29.15	6.7
20.0–49.9	48.60	47.87	0.73	26.76	2.7
50.0–100.0	46.57	42.71	3.86	28.80	13.4

B. Pennsylvania: Presidential Elections, 1900–1916

Cat. of Reg. Coverage	Regression line est.: "Expected" 1916	"Observed" 1916	$E(16)-O(16)$	$T-T+4$	$\frac{E(16)-O(16)}{T-T+4}$
0	69.0	69.0	0	12.0	0
0.1–19.9	68.2	64.8	3.4	15.2	22.4
20.0–49.9	66.9	64.4	2.5	14.2	17.6
50.0–100.0	64.6	59.4	5.2	16.4	31.7

C. New York: Presidential Elections, 1900–1916 (T to $T+4$)

Cat. of Reg. Coverage 1948 (Mean for each)	Regression line est.: "Expected" 1916	"Observed" 1916	$E(16)-O(16)$	$T-T+4$	$\frac{E(16)-O(16)}{T-T+4}$
0	80.04	80.04	0	8.64	0
16.7	80.02	79.74	0.28	8.92	3.1
55.1	80.04	76.20	3.84	13.48	30.8
100.0	73.16	64.06	9.10	17.00	53.5

D. Ohio: Presidential Elections, 1900–1916

Cat. of 1932 Registration Coverage and N	Regression line est.: "Expected" 1916	"Observed" 1916	$E(16)-O(16)$	$T-T+4$	$\frac{E(16)-O(16)}{T-T+4}$
None (57)	83.1	83.1	0	9.1	0
Partial (28)	81.3	75.9	5.4	14.3	37.8
"Full"* (3)	76.7	72.3	4.4	12.8	34.4

E. Missouri: Presidential Elections, 1900–1916

Grouping of Counties and N	Regression line est.: "Expected" 1916	"Observed" 1916	$E(16)-O(16)$	$T-T+4$	$\frac{E(16)-O(16)}{T-T+4}$
Metropolitan** (3)	83.2	83.2	0	1.4	0
Other (112)	73.0	71.1	1.9	2.9	63.8

* Cuyahoga, Franklin, Hamilton
** St. Louis City, St. Louis County, Jackson

to a "pure-behavioral" component of change. Thus, whatever residue is left can be provisionally attributed to the personal-registration requirement as an intervening causal variable.

The steps to be followed in analysis are these. First, the 1916 regression-line turnout value in nonregistration territory is calculated as a proportion of the 1900 value. Second, an "expected" 1916 value is computed for all other categories, based on a multiplication of this proportion by the 1900 regression value for each. Third, the differential between the "expected" 1916 value, based on the calculations in the second step, and the actual 1916 regression-line value is derived. Fourth, the absolute difference between the 1900 and 1916 values is computed by subtraction for each category, including the nonregistration category. Finally, the absolute loss on the regression line is divided into the expected-observed differential by a formula such as

$$C_r = \frac{E(T+n) - O(T+n)}{T - T + n} \times 100.$$

The resulting percentage can be described as that part of the regression-line decline in a given area which can be attributed to factors other than the behavioral ones operative in nonregistration areas[13] (Table 6).

Most of these percentages are remarkably small. Leaving aside the Missouri metropolitan counties for the moment, the highest contribution for the 1900–1916 period is found in New York City, where the residue is 53.5 per cent. Here we deal with a case of exceptional stringency. In addition to the personal-registration requirement, the state legislature—with malice aforethought—imposed a periodic requirement as well, one which was applied to this city alone until its elimination in the 1950s. For reasons discussed above, the percentage figure in Pennsylvania gubernatorial elections within the eight most urban counties is extremely small. Otherwise, nonbehavioral variables as measured by this indicator tend (except in Missouri) to explain about one-third of the gross turnout decline in urban areas.

The Missouri case is special, and leads us to the next part of this discussion. Here we have a gratifying high figure of 63.8 per cent contribution of nonbehavioral components in the metropolitan areas of the state, but the longitudinal movement is extremely slight, falling far below the standard error of estimate for the regression equation. So small is this 1900–1916 turnout shift in Missouri that it may even be nothing more than the product of random errors in data. We are left with the question of positioning all four of our states on a continuum which will help us understand the very wide interstate differentials in our "sample"; Missouri is at one end and Pennsylvania is at the other. What kind of continuum would this be?

My original discussion of the turn-of-the-century shrinkage in the American electorate placed heavy stress upon the immense changes in partisan alignments which followed the critical-realignment sequence of 1893–1896.[14] This realignment led, as is now very well known, to several profound consequences for party politics in the United States. One of these was the destruction of competition between the major parties in many states, and a more or less pronounced movement away from close two-party competition in many others. The second, closely related to the first, was the achievement of corporation-capitalist dominance over public policy. This dominance reached its peak in the 1920s. The relationship between elite hegemony, one-party politics, and suppression of the electorate is so conspicuous in the post-1890 South as to require no further discussion here.[15] But perhaps this was a localized, if extreme, relationship. My original argument suggested otherwise, since the focus of attention was on turnout decline in non-Southern states. But perhaps personal-registration requirements vitiated the force of the party-competition argument outside of the South.

The four states we have analyzed here cannot be said to be a representative sample of post-1896 voting behavior outside of the South, though they did contain among them 36.7 per cent of the total non-Southern population in 1910. With only four composite data points, the posited relationship can only be said to be suggestive. Still, they remain of sufficient interest and visibility to merit presentation here. If one examines the mean partisan percentages for the five presidential elections before 1896, and the five after for these four states, the following pattern appears (Table 7). The relationship between changes in competitiveness and in the magnitude of the mean turnout slope is both obvious and quite close.[16]

The 1896 realignment produced very different partisan aftermaths in different states. The Democratic party was virtually destroyed in Pennsylvania. Yet it retained great post-1896 vitality in New York City; and, as Thomas Flinn and John Fenton have shown, nineteenth-century alignments were not submerged in Ohio until well into the present century.[17] In Missouri, as in the other Border States, the realignment of 1896 had partisan effects exactly the opposite from those affecting states north or south of that region. It produced close two-party competition for the first time since Reconstruction, and this intense party competition endured until the New Deal. The creation of close party competition seems closely related to the virtual absence of major

[13] It is obvious that this metric is based upon an assumption of intercategory homogeneity in every respect other than that of the personal-registration variable. Such an assumption may or may not be partially or wholly valid. More elegant techniques of decomposition are undoubtedly called for. Even so, this is a possibly useful effort at *first* approximation.

[14] Burnham, "Changing Shape," pp. 23–26. See also Walter Dean Burnham, *Critical Elections and the Mainsprings of American Politics* (New York: W. W. Norton, 1970), pp. 71–90; and E. E. Schattschneider, *The Semisovereign People* (New York: Holt, Rinehart & Winston, 1960), pp. 78–96.

[15] The best discussion still remains that of V. O. Key, Jr., *Southern Politics* (New York: Knopf, 1949). See also Paul Lewinson, *Race, Class and Party* (New York: Grosset & Dunlap, 1965 ed.).

[16] It would be closer still if the partisan means included the Democratic percentages of the total vote cast for all statewide offices in the two periods.

[17] Thomas A. Flinn, "Continuity and Change in Ohio Politics," *Journal of Politics*, vol. 24 (August, 1962), 521–544; John H. Fenton, *Midwest Politics* (New York: Holt, Rinehart & Winston, 1966), pp. 117–154.

Table 7. Shifts in Partisan Competition and Turnout: Four States

	1876–1892			1900–1916			Mean Partisan Lead	
State	% Dem.	% Rep.	% Other	% Dem.	% Rep.	% Other	1876–92	1900–16
Pennsylvania	45.7	51.6	2.7	34.3	60.2	5.5	3.9R	25.9R
New York	49.0	48.3	2.7	42.3	52.8	4.9	0.7D	10.5R
Ohio	47.5	50.0	2.5	43.5	51.2	4.3	2.5R	7.7R
Missouri	52.7	42.6	4.7	48.8	47.8	3.4	10.1D	1.0D

State	Net Shift in Party Competitiveness: 1876–92/1900–16	Mean Standardized Turnout Regression Slope, 1900–1916 bX/a
Pennsylvania	−22.0	−.0439
New York	−10.5	−.0330
Ohio	− 5.2	−.0327
Missouri	+ 9.1	−.0065

turnout decline in Missouri before 1920. If it be true that personal-registration qualifications in the state's metropolitan centers explained nearly two-thirds of their 1900–1916 decline, the decline there was miniscule compared even to the secular trend in nonregistration territories of New York, Ohio, and Pennsylvania. Similarly, one doubts that the relatively very steep decline in Pennsylvania's participation—and in all categories of registration coverage in its counties—is in any way accidentally related to the destruction of major-party competition there.

Let us summarize the discussion so far. Examining the four states which form our data base, we can conclude that the intervention of personal-registration requirements involved the intrusion of a highly significant intervening variable. Of course, this variable must henceforth be explicitly included in any future model of change in the American electorate after 1890. But we can also conclude, if tentatively, that it explains very much less of the decline in turnout during the first two decades of this century than might be supposed. If our inferences from the cross-time political behavior of nonregistration territories are correct, this variable contributed between one-fifth and one-third at the most to the 1900–1916 statewide turnout declines in New York, Ohio, and Pennsylvania. The extremely rapid rate of decay in Pennsylvania counties not covered by registration, particularly when studied together with the local intensity of political opposition to the Organization and the closeness of elections, would reinforce a theory of cumulative political alienation—at least for that state.

Finally, the evidence is suggestive here of the relationship between the gross effects of the 1896 realignment on party competition and the relative magnitude of turnout decline in presidential elections from 1900 through 1916. Here, Missouri seems in particular to be a limiting case, since virtually no decline occurred, while two-party competition became extremely close during the era of the fourth party system. It may also be noted that there was an enormous gap in the relative level of economic development between states such as Missouri and Pennsylvania; and, in politics, Missouri retained (and still retains) a nineteenth-century element which has disappeared in more "developed" parts of the country.

Thus, on the basis of the foregoing analysis, it must be assumed for now that the bulk of the decay in participation from the end of the last century through the enfranchisement of women must be explained in behavioral terms. The magnitude and—except for Missouri—the universality of this decay is such that "nondramatic" variables cannot begin to explain it all away. The same is almost certainly true for our other indicators of electoral decomposition. Something in the redefined agenda of politics after 1896 drove large masses of voters away from active participation in electoral politics—most conspicuously where the immediate partisan effects of the "system of 1896" were most conspicuous, as in states like Pennsylvania. Alternatively, it may well be that there was a severe decay in age-cohort inputs into the active electorate after 1900; we cannot know for sure at this stage, if ever. Whatever that something was, it was profound and system-wide—mediated, of course, by the interposition of new structural variables which were themselves the result of system stress and system adaptation. I find no difficulty on this record in adhering to the view that an explicitly urban, bourgeois elitism—partly corporate-conservative, partly Progressive-"reformist"—was intimately and causally associated with these remarkable behavioral transformations.

Effects of Women's Suffrage on Electoral Decomposition in the 1920s. The preceding analysis points to the conclusion that decomposition in the American electorate had proceeded quite far by the end of the First World War. Other evidence has been presented elsewhere which points to the same conclusion.[18] If so, and if one can assume that these measures taken together are indicative of an overwhelming process of "political desocialization," it would follow that this process would have been well along toward completion before the emergence of a second intervening variable, the enfranchisement of women. It has been customary to accept the view that the addition of so quantitatively huge a variable explains subsequent anomalies in the American behavioral universe. One survey-research finding which has apparently universal generality is that women tend to be less politically conscious than men, are less "socialized" into the world of active political participation, and tend to participate less.[19] Similarly, the further decline in participation during the 1920s finds very much of its explanation by the sheer doubling of the electorate by the addition of this weakly socialized component. Even today, as Converse points out, the rate of female voting in the United States runs about 5 to 10 percentage points below male turnout.[20]

While these female political characteristics are well known, it might be useful to raise some pointed questions about the influence of women's suffrage in its early years, particularly in areas of voting behavior other than turnout. In addition to stressing its manifest importance for subsequent turnout rates, Converse evidently uses it to account in part for the peaks in such indicators as split-ticket voting and rolloff which developed in the 1920s. But does it? If the Burnham analyses of the changing American electorate in this period are valid, it could be expected that partisan decomposition and other evidences of "desocialization" were so far advanced by the 1915–1920 period that the still more weakly-socialized women would add little more to it. This would imply, of course, that most of the observable electoral volatility reached in the 1920s would have been present even if women had not received the vote until the advent of the New Deal.

One notes in the first place one very curious anomaly in statewide turnout rates during this period. In Kentucky—one of the Border States which became intensely competitive during the lifetime of the "system of 1896"—the highest recorded turnout rate in the entire period from 1920 to the present was achieved in 1920, the very first year of women's suffrage in that state.[21] But this curiosity, intriguing as it is, is also unique for statewide presidential-election turnout arrays in the 1920–1968 period. There is, however, one place in the United States where detailed and sex-stratified aggregate data are available during this transition period, and to this we now turn.[22]

Fortunately for researchers, Illinois gave partial but not complete suffrage to its women, effective with the 1915 Chicago mayoral election. As a result, detailed breakdowns of voting behavior by sex are available for Chicago from 1915 through 1920, and for the suburban towns of Cook county from 1916 through 1920. This series reveals certain attributes of female voting behavior which are well known. One which was shared in common with the behavior of women in other countries was a marked aversion to voting for leftist parties (Labor, Socialist, Farmer-Labor) by comparison with men. Another, of course, was a stratified differential in turnout. Suburban Cook county was (and is) much more native-stock, middle-class, and Republican as a whole than Chicago. Thus, the number of women voting in the city was 58.0 per cent of the number of men voting in 1915, 59.4 per cent in 1916, 58.5 per cent in 1919, and 59.7 per cent in 1920. By comparison, the female percentage in the suburbs was 69.0 per cent in 1916, and 69.2 per cent in 1920. These figures correspond to a sex differential in turnout of some 30 per cent in the city and 23 per cent in the suburbs, compared with the 5–10 per cent national differential today. So far, so good; one hardly needs to add that the turnout rates in both jurisdictions underwent a sharp decline when women were enfranchised.

[18] Burnham, *Critical Elections*, pp. 91–134, and Appendix Tables I-V.

[19] For a classic presentation of this point for mostly European data before 1937, see Herbert Tingsten, *Political Behavior* (London: P. S. King, 1937), pp. 10–78; see also Angus Campbell, Philip E. Converse, Warren E. Miller and Donald E. Stokes, *The American Voter* (New York: Wiley, 1960), pp. 483–493; and Stein Rokkan, *Citizens, Elections, Parties* (New York: David McKay; 1970), pp. 385–394.

[20] Converse, "Change in the American Electorate," pp. 269–270.

[21] Kentucky's 1920 turnout was 71.8 per cent, more than 20 points above the national average, and falling midway between a mean of 81.2 per cent for the 1900–1916 period and a mean of 63.1 per cent for the period 1924–1940. It is perhaps worth noting that the 1920 presidential election was closer in Kentucky than was any subsequent election except for 1952.

[22] The sex-stratified data—available down to the precinct level in the city of Chicago—are found in the 1916, 1917, 1920, and 1921 volumes of the *Chicago Daily News Almanac.*

Table 8. Sex and Partisan Swing: Chicago and Its Suburbs, 1915–1920

Pair of years and sex	Office	Shift in % of Total Vote Dem.	 Rep.	 Other	Defection Ratios
		I. Chicago			
1915–19 Men	Mayor	− 3.4	−20.5	+23.9	34.8 (Rep.)
1915–19 Women	Mayor	− 0.1	−18.9	+19.0	31.8 (Rep.)
1916–20 Men	President	−22.1	+18.6	+ 3.5	48.4 (Dem.)
1916–20 Women	President	−24.5	+23.0	+ 1.5	51.8 (Dem.)
		II. Suburban Cook			
1916–20 Men	President	−17.3	+15.2	+ 2.1	52.9 (Dem.)
1916–20 Women	President	−19.0	+19.8	− 0.8	58.2 (Dem.)

But the rest of the data point to very different conclusions about the electoral significance of this variable in the Chicago metropolitan area. Let us begin by a comparison by sex of the partisan swings from the 1915 to the 1919 Chicago mayoral elections, and from the 1916 to the 1920 presidential elections in both areas (Table 8).

There is no doubt that the relative velocity of pro-Republican movement from 1916 to 1920 was somewhat greater among women than among men, and greatest of all among suburban women, Nevertheless, stratified by sex, the swings are clearly of the same order of magnitude in each area. Between the two mayoral elections, indeed, the male Republican defection actually *exceeded* that for women. In absolute terms, shifts of 20 per cent or so between one election and the next are symptomatic of considerable electoral volatility by any longitudinal standard, particularly where—as here—they are not part of a critical-realignment sequence. If the mean female partisan swing in the 1920 landslide was only 16.5 per cent greater than the male swing for both major parties and in both areas, what significance should we attach to women's suffrage as an intervening variable promoting dramatic increases in electoral volatility? The difference between the 1916 and 1920 swing in the total vote and what it would have been had men alone voted can be easily computed. When this is done, the mean sex-related increment for both parties in both areas works out to 5.6 per cent (2.9 per cent for the Democrats, 8.4 per cent for the Republicans). While this variable thus produces a visible differential effect and, in the predicted direction, it is hard to get very excited about it.

The data become more interesting still when one examines split-ticket voting and rolloff, or net aggregate "ballot fatigue" (Table 9). Once again the posited relationships duly materialize. Women in each jurisdiction do reveal somewhat greater propensities to vote split-tickets than men, and rather more marked rates of ballot fatigue. But these differences within each area, particularly in split-ticket voting, are not very large. *Moreover, they pale into insignificance by comparison with the very wide differences in behavior between Chicago and its suburbs.* Again, such differential behavior is anomalous, conspicuously so where rolloff is concerned. For it might well be expected that the suburban areas—relatively affluent, native-stock, and so on—would display less rather than much more ballot fatigue. One quite orthodox hypothesis suggests itself: this may well reflect the workings of the machine in the city, and of Post-Progressive independence in the suburbs.

Whatever the explanation, one thing seems clear. The sex differential is quite small in each

Table 9. Sex and Indicators of Split-Ticket Voting and Ballot Fatigue: Chicago and its Suburbs, 1920*

Area	Sex	Mean % D	Variance	Standard Deviation	Rolloff
Chicago	Men	29.5	28.20	5.31	3.8
Chicago	Women	29.9	33.04	5.75	5.0
Suburban Cook	Men	21.8	82.99	9.11	13.8
Suburban Cook	Women	21.4	102.22	10.11	16.2

* Based on percentage vote for 7 offices: President, U.S. Senator, Governor, State's Attorney, Recorder, Clerk, and Coroner.

area, while the absolute size of the split-ticket and ballot-fatigue indicators *among men* appears quite substantial by comparative standards, and is conspicuously so for the rolloff indicator. With data which is this seedy along the posited sex dimension of political behavior, we may well suspect that there is less than meets the eye in this famous woman-suffrage variable. It is a pity that so little relatively hard data exist pertaining to this set of issues. What we have forcefully suggests that—if these large values may be taken as evidence of weak political socialization—those women *who voted* in Cook county between 1915 and 1920 were only very marginally weaker in political socialization than men.

Moreover, it is widely agreed that female turnout did not begin to move much closer to male participation rates in the United States until the 1928–1936 realignment sequence. Accordingly, one should find further increases in these measures of decomposition during and shortly after this sequence. But precisely the opposite happened: In general, levels of split-ticket voting and ballot fatigue *declined* markedly as turnout sharply *increased*. This 1928–1936 influx of new voters was heavily concentrated among working-class and women—particularly ethnic women—voters in large industrial states like Pennsylvania and Illinois. Such voters were hitherto exceptionally weakly socialized; yet their addition to the electorate was associated with *declines*, not increases, in measures of electoral volatility. When these two data points are reviewed together, the argument that behavioral rather than statutory change was the crucially important development in the evolution of the American electorate from 1900 to 1940 seems, if anything, to be strengthened.

The Introduction of the Australian Ballot. The analytic ground entirely shifts when we turn to this posited intervening variable. Since I have recently discussed it at some length in response to a *Review* article by Professor Jerrold Rusk, commentary here will be brief.[23] Unlike the other variables discussed here, the Australian Ballot, when adopted, tended to be applied universally within each state from the beginning. Moreover, it was adopted virtually nationwide in a very short period. Massachusetts was the first state to adopt it generally, in 1889; by 1896, far more than three-quarters of the states had followed suit. Hence, the analytic strategy of subunit differentiation cannot be pursued in this case.

The basic issues at controversy concerning this variable are conceptual in nature. They boil down to the question: What are we primarily interested in measuring? In this sense, there are close analogies to the problem of analysis posed by the Pennsylvania turnout arrays in Tables 2 and 3. If one is interested in detecting the existence of a statistically significant intervening variable, one can perform a series of cross-sectional analyses of variance and compute the appropriate F-ratios for each of the election years 1 through *n*. If one is interested in examining longitudinal trends in each grouping used in the preceding analysis, the matrix must be transposed so that regressions on time can be performed for each grouping. As we have seen, the first approach yields the conclusion that turnout decline was of very large magnitude even in counties where no personal-registration requirements were introduced. Both approaches seem to be essential to describe change in this universe with any adequacy.

We are on similar conceptual if not statistical ground when we contemplate the effects of the Australian ballot. Unlike either of the other variables discussed so far, this is in the first place a crucial and universal intervening variable of the *sine qua non* variety. But in the second, it is a *permissive* variable rather than one which is deterministic or directly influential as such in shaping subsequent behavior.[24] So far as the first point is concerned, the change from the printed party-distributed ticket to the official consolidated ballot in any of its forms was by definition an indispensable precondition for large-scale ballot-splitting in the electorate. The old partisan ticket did not make it impossible for voters to split their ballots, but it unquestionably and systematically constrained such behavior in ways which the official ballot was in fact designed by its authors to overcome. This dichotomous change is of such transcendent importance that elaborate statistical demonstrations of it become almost supererogatory.

But if this intervening variable was indispensable to all that followed, it also was clearly less immediately shaping in its effects on partisan voting than either of the other two examined here. This point can, like many others, be most easily perceived by the analysis of specific and probably extreme cases. To take just one of several examples, Massachusetts was the first state to adopt the Australian ballot. Moreover, it chose the office-block format, which has been in use ever since. This format was explicitly designed to confront the voter with each individual candidate on a long ballot, and hence made it as hard as possible for the individual to vote a straight party ticket. Yet there is virtually no change in the state's split-ticket indicators in the years after the ballot's first general use in the 1889 election. In fact, it took *fifteen annual trials* before split-ticket voting in Massachusetts suddenly began to rise sharply above traditionally low levels.[25] From this fact it must be inferred that traditionally high levels of partisanship survived for half a generation after the ballot was changed, and despite the exceptional procedural difficulties which that change put in the way.

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[23] Rusk, "The Effect of the Australian Ballot Reform."

[24] Except with regard to "rolloff," or ballot fatigue, which in Massachusetts underwent a sudden and substantial increase with the introduction of ballot reform. The failure of the partisan indicators to undergo any major change in the same reform-era period suggests the probability that ballot fatigue affected the followings of both parties about equally.

The Australian Ballot in this state thus opened the way to much later electoral change, which deeply affected the intensity of partisanship in the electorate. *But that is all.* The events of 1904 and subsequent years must find their explanation in profound change in popular attitudes and behavior, not in any directly measurable partisan effects of the Australian ballot itself. Nor, as I have tried to show elsewhere, is this a unique case in point.[26] There appear to have been three waves of increase in split-ticket behavior throughout the country before the most recent post-1950 upsurge. Each of these three crested at a higher peak than its predecessor, and the present fourth wave has moved to higher levels still. The first of these three occurred in the 1890s, concurrent with the establishment of the Australian ballot in most states. The second fell in the 1904–1912 period, and the third during the 1920s. All of them except the first must be explained on grounds other than those of changes in election mechanics; all except the first represent significant behavioral changes within the electorate. If one is primarily concerned with long-term trends within an electorate, he must, of course, be vitally concerned with such structural changes as the introduction of new ballot forms. *But we are also primarily concerned with the whys and wherefores of subsequent changes in measurable behavior.* These changes—all of them evidently of much greater magnitude than those of the ballot-reform era itself—cannot receive explanation through structural hypotheses.

Moreover, there is a problem even with the first or ballot-reform-era upsurge in split-ticket voting, one which may well turn out to be extremely refractory to quantitative analysis. This concerns the timing of the reform. Unhappily for analytic certitude, the period 1890–1896 is not only one of ballot reform but also of nationwide critical realignment. In his recently published study, *The Winning of the Midwest*, Richard Jensen makes the important point that each of the elections in this period—1890, 1892, 1894 and 1896—involved unprecedented partisan volatility which primarily reflected itself in landslides, the first two in favor of the Democrats, the second two in favor of the Republicans.[27] These closely spaced but divergent landslides themselves drastically loosened the bonds of partisan allegiance which had dominated the preceding third party-system era. Moreover, political independence took a second significant form during the period in the "colonial" regions of the country: massive support for the Populist party which ended only with the Bryanite fusion of 1896. It is reasonable to suppose, as Jensen suggests, that these behavioral upheavals themselves produced some irreversible consequences for partisanship in the succeeding era. It would thus be reasonable to posit a cause or causes arising within the American sociopolitical system in this period which generated Populism, massive landslides, critical realignment and the Australian ballot reform. This of course does not deny the permissive significance of the ballot-reform variable for subsequent electoral change. But it once again calls our attention to two propositions. First, reform was the consequence of behavior undertaken in response to systemwide strain. Second, an interlocking network of unprecedented behavior at the mass base occurred at the same time, and in response to the same systemwide political strain.

The Australian ballot and the Populist revolt grew out of the same primordial sociopolitical reality: the rapidly accelerating dysfunctions of a system of mass electoral politics—including electoral mechanics—which originated in preindustrial, preurban times. In particular, it grew out of the "swamping" of this system by the concentrated power of what our forebears called "combinations" in both politics and economics: the urban and statewide political machine on the one hand, the corporation and the "trust" on the other, often enough with close links between the two. As the Australian ballot and subsequent reforms of election machinery were promoted by a moralistic urban *bourgeois* revolt against the effects and beneficiaries of urban Machines, so the Populist revolt was aimed against the subversion of the national preindustrial political economy by concentrated corporate capital. The two events are, unfortunately, precisely synchronous with each other, and both occur at the approximate terminal date of a dying preindustrial nineteenth-century universe of electoral politics.

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[25] Burnham, *Critical Elections,* p. 115.

[26] *Ibid.*, especially pp. 52 and 109–110.

[27] Richard Jensen, *The Winning of the Midwest 1888–1896* (Chicago: University of Chicago Press, 1971), especially pp. xii–xiv. In the present author's view, this seminal work is "must" reading for anyone interested in diachronic analysis of American electoral politics.

Thus, one has more than a little reason for supposing that a presently indefinable but probably large component of split-ticket increase even during the first (reform-era) wave can be attributed to behavioral change. While the ballot reform of course facilitated it, this change was at its roots attitudinal rather than structural in origin, as the failure of split-ticket voting in Massachusetts to increase significantly from 1889 through 1903 rather clearly indicates. It would probably be quite fruitful to stratify and analyze the post-ballot-reform split-ticket measures both by region and by the extent of Populist voting in 1892 and 1894, and perhaps as well by the extent and direction of partisan swing in the 1896 election. For in Massachusetts, Populism and "colonial" revolt against Eastern finance capital were vanishingly small. Perhaps this is why we have to wait until the Progressive-era wave of 1904–1914 to see any convincing partisan effects of this reform in the Bay State.

Implications: Toward a Comparative Sociology of Past Voting Behavior

By this point, the reader may have some sense of enlightenment concerning some specific issues in the controversy but still be quite unpersuaded of its overall substantive importance. If we accept the view that these rules changes should be regarded as wholly mechanical intervenors with no origins of their own in system stress and system adaptation, even so we could surmise that taken together they explain no more than half of the decompositional changes discussed in the 1965 article. But assuming this is true, so what? A reply to this legitimate query must take us to Converse's discussion of the electorate which existed before these changes took place. For not only is this discussion the weakest and most ambiguous link in an otherwise splendid and provocative paper, it also discloses the heart of the controversy.

Converse devotes considerable attention to the possibilities for, and occasional realities of, voter fraud, including repeating votes, in nineteenth-century America.[28] That this was occasionally notorious and even locally widespread cannot be denied. It comes out with compelling vividness in those hidden gold mines for historical electoral research, congressional hearings on contested elections in the South and elsewhere between 1868 and 1901. This, too, is a controlling variable which, to the extent it was a reality, would serve to depress the magnitude of change after the 1890s by shrinking the initial baseline of measurement. The problem, of course, is the virtual impossibility of ascertaining what, if any, fraudulent component existed at a given time and place, particularly in the rural reaches of nineteenth-century America.

However titillating the exploits of "repeaters," "floaters," and other venal hangers-on appear in the official and unofficial literature of contemporaries, it is simple prudence to avoid a precipitate surrender to Progressive-era political clichés. As the example of Philadelphia and other cities reveals down through the 1920s and later, the many urban machines which continued to thrive after turn-of-the-century reform movements had no difficulty in generating surplus voters on demand despite the statutes. Moreover, outside of the South and border areas, most of the preoccupation with corruption was focused, with good reason, on the cities rather than the countryside. When one considers that turnout rates in rural areas tended to be high over very long periods of time down until about 1900, any speculations on corruption as an artificial inflator of general rather than local significance must be open-ended in time as well as space. The assumption must be made that not only were corrupt surpluses more or less universal on territorial lines, but that these surpluses remained a more or less uniform proportion of the total reported vote for two generations and then, for reasons left most obscure, declined toward insignificant levels after 1900.

The first assumption may have some validity for some nineteenth-century statewide political systems outside the South. In Indiana in particular, a "floating vote" of some ten per cent of the total electorate appears to have existed from the 1870s through the 1890s. Even here, however, it should be pointed out that by no means all of this purchasable vote produced artificial inflations of turnout: a vote which is for sale is not necessarily a case of repeating or fraudulent surplusage. Richard Jensen's recently published study of the issue raises an appropriately cautionary note:

> It is easy to accept the fulminations of embittered losers at face value . . . But the historian has to be more careful, especially in view of the long history of a "corruption" theme in American politics . . . It is vastly easier to make blanket allegations of illicit, secret activities than to disprove them; almost always there is a grain of truth in the charges . . . The myth of massive corruption so cleverly conceived that it cannot be detected is a ghost story.[29]

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[28] Converse is particularly concerned to create the impression of endemic and massive voter frauds in nineteenth-century rural areas—not surprisingly, in view of his basic hypotheses—though part of the discussion of the fraud problem is based on Progressive-era formulations by Joseph Harris and others whose focus was urban rather than rural. Converse, pp. 288–293, 300.

[29] Jensen, pp. 34–35.

The conclusion which Jensen derives from a massive sifting of the available evidence is that traceable corruption, being a dangerous enterprise for its practitioners, was at most a marginal phenomenon. Most particularly it was marginal in national elections: the motivation of politicians to engage in such practices was at its highest point in local elections of direct importance to local machines—precisely, it may be added, as the Philadelphia story from 1900 through 1930 amply demonstrates. More significant, in Jensen's view, was the amount of error which could creep into the tabulation and counting of votes. Even so, the overall conclusion which Jensen makes after his survey of the evidence is that corruption was episodic and marginal in the Midwest rather than universal. The implication is strong that "militaristic" mobilizations of voters and an extreme strength and intensity of party identification among the mass electorate were overwhelmingly responsible for the phenomenal participation rates found in the 1880–1900 period.

If corrupt surplusage or deliberate tabulation errors could explain *a part* of Indiana's phenomenal turnout rate between 1876 and 1900, such explanations are both less obvious and—for reasons of local political culture and party strategy alike—much less persuasive in states which politicians considered "safe" for one party, and where moralism was a conspicuous part of the local political culture. States such as Iowa and Wisconsin come to mind. Similarly, it is particularly doubtful—and supremely so in the absence of empirical findings—that rural Pennsylvania or the counties of upstate New York were dominated by pervasive vote frauds and overcounts. If one were to ask for a better and more generally plausible explanation for this mass mobilization, the answer must surely lie in the direction which Jensen suggests: in the rechristianization of the country through pietistic religious revivals between 1800 and 1850, and the development of increasingly polarized ethnocultural conflict in which political and religious perspectives became closely fused into a coherent framework for individual electoral decisions. One may indeed surmise that pietistic revivalism provided both the absolutism and the moral energy required to bring about the realignments of the 1850s and, hence, the war against secession and slavery.

It seems clear enough that the civil war trauma was of tremendous polarizing significance in its own right—particularly in the lower Midwest and states like Pennsylvania—and hardly surprising that most elections from 1864 through 1888 were of the "maintaining" variety. It is now still more certain that the struggles between pietists and liturgicals, not only over the Civil War but over local variants of the "social issue" of the time; were intense down until the 1890s. They were central to the building and erosion of political coalitions down to the beginning of the present century. They also were part of a sociological and political context which optimized the probability that any individual member of that "voting universe" would actually cast a ballot; and it is this largely vanished context which seems to explain much of the difference between 19th and 20th century American electorates and their behavior.

We shall return to this point in a short while. Before doing so, we should examine Converse's curiously opaque references to contemporary survey findings concerning rural voting participation in underdeveloped countries such as Turkey and the Philippines. Since the discussion in this version of Converse's chapter is so brief, our discussion will be correspondingly limited. It should be noted above all that the *general* relevance of these twentieth-century studies in the underdeveloped world to American rural politics in the nineteenth is not merely "limited,"[30] but negative. To be sure, politics in the South was marked by a number of "underdeveloped-nation" characteristics. In this region, overwhelming evidence attests to corruption and fraudulence in closely contested elections. Similarly, the voting of hordes of black *campesinos* by their white *jefes* was a conspicuous feature of post-Reconstruction Southern politics. In Alabama, for example, blacks were voted *en bloc* by local white elites to beat back a statewide Populist challenge in 1892 and 1894, and in 1901 they were similarly led to the polls to vote themselves out of political existence.

But these are a few of the many ways in which the South, particularly the Deep South, has been a deviant case in American politics since the Civil War. Even the implication that modern third-world survey evidence might have any applicability to most non-Southern rural voting behavior in the nineteenth century betrays a deep misconception of the historical sociology of the United States. Here the "conventional wisdom" of "impressionistic" American historians and such students of American political culture as Louis Hartz surely leave us on sounder ground.[31] That view argues that except for the very special case of the black population and perhaps some locally significant cases, the nineteenth-century American rural population was composed not of peasants in traditionalist social relations with local aristocracies, but of independent yeomen who were thoroughly bourgeois and "modern" in values and personal goals.

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[30] Converse, p. 287.

[31] Louis Hartz, *The Liberal Tradition in America* (New York: Harcourt, Brace, 1955); see also Louis Hartz, ed., *The Founding of New Societies* (New York: Harcourt, Brace and World, 1964), especially pp. 1–22.

A challenge to the general accuracy of this view, as of any other, is surely possible. But such challenge would seem to require empirical validation as a first step to acceptance. If Converse does not intend to make such an argument, then inclusion of these references to Turkish and Philippine voting behavior is irrelevant, and irrelevant in an important way. For its inclusion makes literally no sense except as a strained attempt to explain away anomaly. The particular anomaly in question here is evidently the educational variable. The levels of formal education in the American electorate a hundred years ago were vastly lower than they are today. Yet, by all aggregate measures, that electorate was obviously far more politically mobilized and active than it is today. And since a majority of Americans lived in rural areas until about 1880, this activity was associated both with low formal education and rurality.

So a problem exists. This problem is a real analytical conundrum only in the context of a particular mode of reality perception. While Converse duly disclaims that modern survey findings are "immutable,"[32] the implicit assumption which informs his entire critique is that of the basic uniformity and generalizability of such findings, at least for the United States. In particular, the broad sociopsychological model of the contemporary American electorate which is laid down in *The American Voter* and in Converse's subsequent works is implicitly adhered to from beginning to end. But this model obviously does not fit the data of change in the American electorate. It is inherently static, without historical depth and, for a variety of reasons relating to the intellectual history of the discipline, as weak in its sociological dimensions as it is strong in its social psychology.[33] Since it does not fit the data of long-term change, and since above all its findings in the here and now can give us rather little guidance in the analysis of past political upheavals such as that surrounding the Civil War, any longitudinal analysis made on its premises will turn up a very long list of very important anomalies.

Above all, the shape of the aggregate data for the last century—and particularly for the crises of the 1860s and 1890s—almost compels the conclusion that the level of political cognition in mass electorates is not more or less uniformly constant and low, but is subject to great fluctuations which are heavily dependent upon historical and sociological contexts. Such a conclusion, of course, leads to relativist skepticism, not about the methods and techniques of contemporary survey research, but about the relevance of the findings derived from them at a given point in time and space to other times, places, and contexts. More than this, however, it requires a complete rethinking of the celebrated pluralism and overarching harmony in the American political system of today, and to necessarily cautionary reflections on the health of that system. It is hardly surprising that such a conclusion would be stoutly resisted, or that major efforts should be mounted to discount or explain away the data which seem to support it.

Let us make the assumption that when politics matters, when it is not a spectator sport but is widely perceived to raise life-and-death issues, people behave as though it matters. In other words, let us assume that a significantly larger fraction of the electorate will be politically active and relatively politically aware in crisis situations than surveys of contemporary American presidential elections would lead us to believe, and that the deeper the crisis the more politicized the electorate.[34] Let us finally assume that the sociological context of electoral politics in America was quite different a century ago from the situation today. When we make such assumptions, the anomalies which have perturbed both Converse and Rusk vanish. In the first place, we find ourselves on the trail of a useful explanation of that immensely important American peculiarity, the periodic critical-realignment sequence. Second, we can accept with entire ease the numerous behavioral properties of nineteenth-century electorates which are inverses of the American electorate's behavior in the twentieth century. Third, we can cheerfully get to work on constructing a better model of change than the one which I stated in 1965—a model which incorporates structural and behavioral components, gives each its proper weight, and integrates both into theory.

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[32] Converse, p. 289.

[33] For an extremely valuable discussion of the intellectual background of American political science generally and of voting analysis in particular, see Richard Jensen's two articles, "History and the Political Scientist" and "American Election Analysis: A Case History of Methodological Innovation and Diffusion," in *Politics and the Social Sciences*, ed. Seymour Martin Lipset (New York: Oxford University Press, 1969), pp. 1–28 and 226–243.

[34] One perhaps suggestive quantitative characteristic of American electoral history has been the close association between periods of critical realignment such as those of 1856–60, 1894–96, and 1928–36 and waves of electoral mobilization or participation increases. Analysis of extreme-polarization situations in current American electoral politics likewise suggests inversions of "conventional wisdom" when racial polarizations are involved: for example, in the Cleveland mayoral election of 1967 the mean turnout in wards more than two-thirds nonwhite was 69.2 per cent, compared with a citywide average of 54.8 per cent; and the Republican (white) candidate received about 80 per cent of a workingclass white vote which has typically been Democratic by margins of 2 or 3 to 1.

Let us take a concise example of where this might lead us, the Lincoln-Douglas debates of 1858 and their contextual setting in the American electorate of the time. Historians have repeatedly brought to our attention the intensity of both oral and written political communications in that period, and the great expenditures of time and money which practical politicians put into both. As is well known, practical politicians viewed Horace Greeley's *Weekly Tribune* as indispensable to the Republican mobilization in the 1850s. As is equally obvious, the ideational content of the speeches which Lincoln, Douglas, and others delivered was of a richness and complexity which, say, the Kennedy-Nixon debates of 1960 did not begin to match. These debates and speeches were reprinted in diamond-point type in the *Tribune*—as likely as not along with dispatches on European politics filed by Greeley's European correspondent, Karl Marx.

If we assume that political elites make informed and rational efforts to achieve maximum gain with minimum expenditure of scarce resources, we can deduce a good deal about the people they were attempting to reach and influence. While—like Lincoln himself—they had little formal education, they were mostly literate. Otherwise, the major investment in densely packed written communication which politicians made was an irrational investment. The wide spread of weekly newspapers for which subscriptions had to be paid bespeaks as well some active interest in the external world among this largely rural electorate. The massive physical movements of farmers from miles around to the central points of rallies such as the Lincoln-Douglas debates likewise suggests political motivation, as do occasional monster rallies in cities from the "log cabin" election of 1840 through the "sound money" and other parades of 1896. Moreover, the relative richness of content in written and oral political communication presupposes a judgment by the politicians of the time that the voter was both able and likely to pay considerable and sustained attention to the communication. Finally, it goes without saying that the political communications of the 1850s concerned political issues of transcendent importance.

All of this is compatible with an electorate having certain properties which no longer exist. First, voters were, for the most part, immensely dispersed across a continental countryside. Second, most of the voters lived on the land, or were at most a generation removed. Third, the primitive communications technology of the time sharply limited the voters' options for entertainment and diversion. This would help to ensure that politics would absorb a much greater share of the interested individual's attention than is now the case. Fourth, religion was primordially important in social and individual life. It now seems extremely likely that the clash between what Jensen describes as "pietist" and "liturgical" ethnoreligious subcultures was basic to the formation, maintenance, and transformation of partisan coalitions, at least in the Midwest and throughout the nineteenth century.[35] The mutual reinforcement of religion and politics which appears to have existed throughout the nineteenth-century political system was undoubtedly promoted by the extent to which Americans tended to settle in "island communities"[36] composed largely of people like themselves. Thus, a visit to a Congregational or Methodist church on Sunday and a Republican rally on Monday in the Ohio Western Reserve a century or more ago would tend to produce cognitive consonance and partisan reinforcement. It would also be likely to produce intense party identification, highly stable party voting, and very high levels of political participation—at least so long as the military model of political campaigning was ascendant.

One very brief and partial, but instructive, example of the relation beteeen religion, party identification and party voting may be taken from a survey of Hendricks county, Indiana, which was made in 1874 and coded by Jensen a century later.[37] This was a county in which the pietist-liturgical ratio was lopsided in favor of the former, and one in which non-Protestant European immigration was extremely limited. It was correspondingly much more Republican than the state as a whole. Reworking the Jensen data and joining the global results of the 1874 survey with presidential voting behavior thereafter, we find the pattern described in Table 10. A comparison of this 30 per cent 1874 sample's aggregate of individual party preferences with the voting behavior of the county's entire electorate in the next five presidential elections reveals a correspondence so close that statistical demonstration is unnecessary. The mean Democratic percentage of the 1876–1892 sample was one per cent less than the 38.5 per cent of 1874 preferences: the mean Republican voting percentage was 0.6 per cent

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[35] In addition to Jensen, see also Paul J. Kleppner, *The Cross of Culture: A Social Analysis of Midwestern Politics, 1850–1900* (New York: Free Press, 1970). For a review of this and two other recent works in the "new history," see Walter Dean Burnham, "Quantitative History: Beyond the Correlation Coefficient," *Historical Methods Newsletter,* 4 (March, 1971), 62–66.

[36] The term was developed in detail by Robert Wiebe, *The Search for Order, 1877–1920* (New York: Hill & Wang, 1967), a nonquantitative but extremely useful reference for this critical transition period.

[37] Jensen, *The Winning of the Midwest,* pp. 59–60.

Table 10. Religious Attachments, Partisan Preference and Voting Behavior: the Case of Hendricks County, 1874–1908

A. Party Preference by Broad Religious Classification, 1874 Survey

Religious *Tendence*	% Dem.	% Rep.	% Other	% of Total Sample in Category
Pietistic	23.8	72.3	3.9	49.5
No Denomination	48.4	47.1	4.5	43.2
Liturgical	79.7	14.4	5.9	7.3
Total County	38.5	57.2	4.3	

B. Party Preference and Aggregate Voting Behavior, 1876–1908[a]

Year	% Dem.	% Rep.	% Other	*N*
1874 Survey	38.5	57.2	4.3	1618
1876	37.1	58.4	4.5	5156
1880	36.9	59.1	4.0	5408
1884	38.9	56.4	4.7	5322
1888	37.0	58.6	4.4	5624
1892	37.8	56.4	5.8	5359
1896	40.3	58.1	1.6	5872
1900	39.7	57.6	2.7	5943
1904	37.1	58.8	4.1	5838
1908	43.2	54.3	2.5	5953

[a] Presidential elections.

more. The central relevance of religious *tendence* in the largely native-stock population of this county seems clearly established here, as elsewhere in the Midwest where Jensen could generate similar microlevel data.[38] In this respect the nearly equal division of the large nondenominational vote between the major parties suggests nothing quite so much as the *absence* of a profoundly constraining group characteristic, a kind of aggregate "cross-pressure" situation.

I have argued elsewhere that a crucially relevant variable for the analysis of European political behavior may be the presence or absence in specific social strata of *political confessionalism.*[39] Such confessionalism, in European contexts, has tended to be closely associated with the development of specific religious-clientele parties (such as the *Zentrumspartei* in Germany between the 1870s and 1933), and also with the rise during industrialization of Marxist working-class parties with explicit ideological commitments which stand more or less sharply in opposition to the existing political and economic order. Strong confessional identities of this kind appear both to stimulate participation among the relevant sociological groups in the larger population, and to insulate them more or less thoroughly from electoral appeals arising from their traditional political oppositions on one hand or new and often extremist political movements on the other. In such cases, parties themselves become "churches," as it were' or are secular components of formally religious organizations.

We are used to thinking of such categories in terms of the class struggle in modern politics, or the epic conflicts between Catholic and Protestant in Ireland, but the work of a new crop of American political historians is now making it increasingly clear that American nineteenth-century electorates mediated their social experience and political goals through an intense, if typically American, form of political confessionalism. This confessionalism involved sharply antagonistic conflicts over right belief and right behavior, informed by highly divergent and intense polarizations over the "proper" form of Christian relatedness to society and politics. Religion and politics tended to constitute an extremely large part of the total formal communication likely to be received by any individual in that vanished political universe; and such communications heavily reinforced each other. The basis was thus laid for the mass participation and the political enthusiasm which defined nineteenth-century electoral politics, particularly during periods of critical realignment. Such linkages provided both ideologies and a coherent vehicle through which the individual could make sense of his world and his proper place in it. They likewise tended to produce an electoral structure whose thorough mobilization, extreme long-term stability and other characteristics resemble nothing so much as the behavior of twentieth-century European confessional electorates. Finally, they provided the basis for a moral energy without which the great Republican revolution of the 1850s and 1860s could have been neither launched nor completed.

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[38] *Ibid.*, pp. 61–62, 310–314.

[39] Walter Dean Burnham, "Political Confessionalism and Political Immunization," *Journal of Interdisciplinary History*, 3 (Summer, 1972), 1–30.

Indeed, elite strategies of the 1850–1890 period make no sense except in the context of a high level of something very like political confessionalism at the mass base: the rallies, the dense packing of political information into party newspapers and broadsides, the staging and content of performances like the Lincoln-Douglas debates. These and other elite responses to politics bespeak an electorate which was intensely politicized, which had the opportunity, the capacity and the motivation to pay relatively close attention to political argument, which participated—not only by voting—at rates phenomenal by contemporary standards, and which adhered tenaciously to partisan commitments once forged in the heat of realignment.

Thus, our explanation is reduced to what would be a truism in any other context than this. High levels of political involvement existed in nineteenth-century America because the historical and sociological conditions of that time were at an optimum for such involvement. If modern religious or Marxist political confessionalism did not exist in this electorate, something very analogous in its political effects almost certainly did. It is hardly surprising that the grass-roots organizational effectiveness of the parties reached its peak in the last half of the nineteenth-century, or that the existence of a Republican/Union *party* organizational structure in the northern states was so profoundly important to the success of the Union cause during the Civil War.[40]

A crucial measure of the comparative difference between electoral change in the United States and party development elsewhere in the Western world is the fact that this primordial quasi-confessionalism was progressively dissolved after the turn of this century *without being replaced by any other variety*. As the old cosmos of "island communities" and pietist-liturgical cleavages dissolved under the pressure of transition to urbanization and industrial capitalism, the modern shape of the American electoral universe emerged—a shape in which merchandising techniques tended to replace the old military campaign style, in which masses of Americans were selectively excluded, or excluded themselves from participation in politics at any level, and in which party linkages between voters and their decisions at the polls suffered severe erosion.

If I were to make any comment on the original 1965 hypothesis which sought a coherent explanation of these remarkable developments, it would be to suggest that this process was largely unconscious and sociologically determined at all levels. It was certainly not a matter of conspiracy but of circumstance that things turned out as they did during and after the realignment of the 1890s: capitalist and party elites, like everyone else, were fumbling for the ball during that chaotic decade. Moreover, it would only be fair to emphasize how much of the demobilization and antipartisanship of the post-1900 era was achieved by a common mass consensus—at least among those who were still left in the shrinking electoral universe. As for the dissidents and dropouts, what viable alternatives did they have? Granted the manifest absence of conditions favoring class-solidaristic substitutes for the political confessionalism of the vanishing nineteenth-century religious cosmos, what other vision of the commonwealth and human liberty than one or another variant of liberal capitalism could survive the transition?

But when all is said and done, we would return to two basic points made in 1965. First, American electoral politics since 1900 has pursued a course which is retrograde to change elsewhere in the developed Western world, and the cumulative effect has been profoundly elitist. Second, the American electorates of the nineteenth century were *in fact*—and not in artifact—extensively different from contemporary American electorates, though perhaps similar in some of their attributes to some contemporary European ones. If this is indeed the case, then it is necessary to say once again that contemporary survey-research models of the American electorate should be used to explain voting behavior only at the times and places for which supporting survey evidence is available. They should be extended elsehwere only with great care. This may disturb paradigms, but it will also yield great benefits; for it will permit us to recover the historical and sociological dimensions of American electoral politics. These, in turn, will make it possible for our discipline to develop far better, far more complete empirical theories of democratic linkage and of American politics than any which we now have.[41]

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[40] For a lucid and illuminating discussion of this party contribution to Union victory, see Eric McKitrick, "Party Politics and the Union and Confederate War Efforts," in *The American Party Systems*, ed. William N. Chambers and Walter Dean Burnham (New York: Oxford University Press, 1967), pp. 117–151.

[41] To a significant extent, this effort may require pursuit of exemplary "case studies" down to a very microscopic, "local-history" level. A good case in point is that of corruption in rural voting among the northern states. Here we may choose the case of Adams County, Ohio, which received extensive dis-

cussion in Jensen, *The Winning of the Midwest* (see also sources cited there). One may summarize this case concisely. (1) Between about 1870 and 1911 the county was the scene of "undoubtedly the worst example of the corruption of the ballot ever known in American history . . ." (Jensen, p. 38). (2) About 90 per cent of the county's voters came to be involved, with each receiving on the average of $8.75. (3) The parties evidently wanted to curb the practice, but the voters would not let them. (4) In 1911, 1690 men were finally disfranchised for five years by a judge for illegal vote selling (about one-fifth of the electorate). (5) At *no* time did this lead to turnout figures in excess of 100 per cent, or to landslides for either party. As to the former, it may be inferred that the local (corrupt) voters had no incentive to bring in outsiders to share the spoils; and there were very possibly normative constraints on "repeating" by local voters as well.

This is a curious case indeed; but systemwide inferences about "corruption" in earlier periods must be subjected to such micro examination. Between 1872 and 1908, the mean turnout rate in Adams County was 93.1 per cent of the estimated potential electorate, only 2.9 per cent higher than the statewide mean turnout rate. Moreover, in 1916—after elections had supposedly been "purified" but the 1,690 disfranchised culprits of 1911 had been restored to their voting rights—the county's turnout rate was 87.3 per cent, now more than 10 per cent above the state's participation rate, but less than 6 per cent below its corrupt pre-1911 mean.

It should also not go without remark that—as is well known—notorious examples of electoral corruption were widely used by the Progressives to obtain enactment of personal-registration statutes. But this *rural* county, perhaps proportionately the most corrupt of all, was entirely uncovered by such requirements until 1962; and a very large part of it is uncovered still.

Comment on Burnham's "Theory and Voting Research"

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I am very grateful to Dean Burnham for his labor in checking out a number of "plausible rival hypotheses" that I proposed some years ago might account for the bundle of changes in American voting statistics he had first described in his seminal 1965 paper. I had pointed out these possibilities with some vigor, since if they could be shown to have empirical merit, they would seem to put a rather different light on those historical changes, relative to the interpretations Burnham originally provided.

I now read the current paper as indicating that for the areas covered, changes in registration practices do appear to have affected turnout adversely, following one of our original rival hypotheses. It also suggests, however, that this registration change does not begin to account for all of the decline in turnout that occurred around 1900, particularly in states like Pennsylvania. It goes on to imply that some other familiar factors affecting turnout, such as party competition, also contributed to that change. In effect, the turnout decline not accounted for by structural change seems greatest where the realignment of 1896 moved states from competitive to rather noncompetitive status (the more frequent case); but states moved by the same events from noncompetitive into competitive status, like Missouri and, in fact, numerous other upper midwestern states, do not show the same pattern as Pennsylvania.

All this is well and good, particularly as Burnham in his penultimate paragraph sets aside the 1965 conspiratorial interpretation of the change that had originally prompted our own brief inquiries as to probable causes. Therefore we feel little need to generate either challenge or defense in this note where the main lines of argument are concerned. There are, however, two or three lesser issues which are worth highlighting.

First, I suspect Professor Burnham would agree that the data he presents are both partial and preliminary estimates of the effect of registration measures on turnout in the period under discussion. In some ways this is obvious: four states do not give us a very definitive picture of this period. Less obvious, but fully as important, is the marginal correspondence between the timing of registration coverage data and the periods over which turnout trends are actually examined (see Table 5). In the extreme case of New York, the date cited for the definition of registration coverage is 1948, although the trend examined is from 1900–1916. I assume that changes in coverage were limited enough that there would be a reasonable correspondence between coverage differences at a later date and areas affected by such legislation in the period for which the turnout trend is examined. However, any lack of perfect correspondence could be expected to muddy the estimates of actual effects, leaving the Burnham data as likely *under*estimates on the turnout decline of the period.

Perhaps more important still, I remain uncomfortable with an assumption which Burnham makes to provide a baseline for all of his estimation. Indeed, this is a problem mischievous enough that we already devoted a paragraph to it in our original "Change . . . " essay. Burnham observes that "any secular trend in voting participation to be found in counties without personal-registration requirements must be attributed to behavioral factors. By definition, no known changes in electoral mechanics could be causes of such change in turnout." Now it is surely true that in a narrow and limited sense, a change in electoral laws cannot be invoked as a causal factor for other change in areas where no legal changes were in fact imposed. There are looser senses, however, in which exactly this can be true; and these seem so plausible to me that I feel they should be made clear. We verge here on the subject of rural corruption in voting in the nineteenth century, a point at which my treatment bothers Burnham considerably.

I originally argued that various kinds of corruption such as the adding of fraudulent votes or the purchase of votes from persons unlikely to have otherwise voted, well-known for urban metropolises of the period, were not unknown in rural areas as well, and in fact produced a significant artificial boost to apparent rural turnout in the nineteenth century. This surmise, if true, would only address the Burnham argument if such practices were not limited to a few states or a single region, but rather were "widepsread," in the sense of some such rural practices going on at a low level in all or most states. I have since been told by several historian friends that whereas there was clearly rural vote corruption in that period of the kind I described, it could not be described as "massive." When I ask, however, whether it might amount to a small but visible fraction of rural votes—say, some proportion between 5–10 per cent, or the amount that I had originally roughly calculated to be necessary on the average to account for features of the Burnham changes not otherwise accounted for—they are much less willing to doubt the possibility. Therefore let me use this forum to point out that I had not intended to claim that a fifth or a quarter of rural votes in the late nineteenth century were manufactured by corrpution: one in twelve or one in twenty would address the case quite well, provided they were folded in on a "widespread" basis, sprinkled across all or most states.

With this said, we are now ready to address the possibility that rural fraud declined in magnitude around 1900 even where registration laws were not imposed on rural areas (the typical case). The legislative debates over coverage of the stricter registration laws being newly proposed around the turn of the century invariably pitted "out-state" or relatively rural forces against the urban machine. The out-state people had raised the issue of stricter control against fraudulent urban voting, and wanted tight registration laws for the city. The urban people initially tried to avoid such controls in general, although they were quickly beaten back to a position of demanding that any registration controls imposed in the cities should be imposed in the countryside as well. It is of high interest that the out-state forces resisted the latter possibility tooth-and-nail, on grounds that most voters were friends and neighbors in rural areas, and hence the urban recognition problem did not exist there. The out-state resistance to statewide controls is interesting because in most struggles where one protagonist suspects the other of cheating but is simon-pure himself, the obvious tactic is to suggest that *both* sides submit to more stringent surveillance. Indeed, had the out-state forces been willing to accept this equitable solution, they would in many instances have achieved stricter urban control much faster than they did. But this was not in general a palatable solution to them: they demanded that rural voting be left uncontrolled. It is hard to understand the vigor of this resistance unless uncontrolled rural voting were deemed a political advantage by those out-state elites most knowledgeable about it and dependent upon it. And it is not clear what that political advantage might be short of significant increments of votes that would not survive tighter rural controls.

Now let us imagine that such a debate begins to rage in the state capital, with out-state forces arguing that controls are unnecessary in rural areas because fraud is not a problem there. What would be more natural than to imagine that rural fraud would be considerably toned down during the period—often lengthy—that such debates were being conducted, and that this could be expected without actual controls ever having been imposed on rural areas? Surely there is evidence that voting fraud had become almost casual in many areas by the 1880s, meaning that concern about its detectability was at low ebb. The reform ferment of the 1890s and 1900s clearly increased concern again over detectability, which would imply that the more blatant forms of fraud were trimmed back even in areas without controls. For the out-state politician arguing that rural controls were unnecessary because fraud was urban, nothing would be more compromising than a rash of rural vote scandals turned up by the now-heightened community vigilance (although, of course, such did arise: see Burnham footnote 41). Hence it would make excellent sense to "lie low" in rural areas until indignation and debate had subsided, assuming that the new, postdebate state of play—stiff controls in the cities but none in rural areas—would be a fertile field for political gain.

We need not imagine that dynamics of this sort account for all of the Burnham rural turnout decline that accompanied the fraud and registration debates of the turn of the century. I would in fact doubt that they would do so. The important point is more simply that these dynamics are extremely plausible, and in whatever degree they registered in the situation at all, by that same degree the Burnham method of assigning weights to the effects of registration on turnout decline will again, and on a new score, produce an *under*estimate of the impact of the broader reform movement toward reducing *apparent* turnout, even though in fact he does find substantial effect.

None of the above should be taken as criticism of what Burnham offers us here. It takes extremely laborious digging to put together data even in this preliminary form, so that four states loosely reconstructed are a considerable accomplishment in itself. Moreover, the general Burnham strategy of asking how registration areas differed from nonregistration areas is the only obvious way of assessing such effects. We have no better to suggest at this point, although it does seem reasonable to keep in the mind the lively possibility that there was a reform-associated decline in turnout within a reformed state even in areas not immediately brought under reform laws. Surely there can be no question but that the public ethos concerning voting fraud is much different in 1973 than it was in 1873, and that during those hundred years fraud has declined from frequent practice to the rare event, although a city or two may remain lingering bastions of such "practice" to the present day. The fading of the frequent practice is fairly well charted for the cities, but when did it occur for the countryside? The turn-of-the-century time of indignation with its proliferation of court cases over voting frauds would seem to be an obvious candidate as a dampening force, even for areas not thrust under any tight registration control. And if this period did not in fact produce such a dampening, what later times and causal agents do we then invoke for the unquestionable rural as well as urban decline in "repeating," vote-buying, ballot-stuffing and other malpractices that now seem so esoteric?

There is one other broad theme dominating this Burnham article that strikes us as a straw man. In several publications Burnham has been developing a caricature of the general body of work from the Survey Research Center in general, and *The American Voter* in particular, which has the following elements. An orthodoxy has been proclaimed, it is argued, based on studies of the 1950s, which purports to be entirely generalizable to all times and places. When anomalies arise which do not fit the terms of the 1950s, the investigators are traumatized, either into silence or into attempts to sweep the anomalous facts under one or another rug.

This caricature has been picked up by Pomper and others and deserves closer examination. We find it somewhat amusing where anomalies are concerned, since we see anomalies as the life blood of inquiry and the main winch for raising the generality of theories. Moreover, it seems that most of our time since *The American Voter* has been spent pushing outward in space to other countries or backward in time historically, all with an object of finding anomalies that require revisions and broadenings of perspective. Naturally, anomalies do not exist save as one has expectations, or proto-theories, capable of being countered. We have such expectations, as does Burnham. Where we appear to diverge is that Burnham feels current survey evidence somehow does not deserve the same status as common sense or available fragments of the historical record in setting expectations—and thus finding interesting anomalies to broaden theory—for treating past electoral events.

We simply disagree on this score. To be sure, current survey evidence would be a weak surrogate for survey materials directly out of the epoch involved, but since such data do not exist for the periods under discussion here, the point is academic. The problem is that the historical record, at least as mined thus far, is dreadfully thin, and needs every bit of illumination it can get, including at least those relationships from current survey work proving most robust cross-nationally and most reliable over a widening period of time. A good example is provided by the firm Burnham expectation that a record of high voting turnout is a manifest indicator of an underlying high political engagement and politicization. To some degree, current survey evidence supports this expectation, as does common sense. But the lengthy passage that I inserted in the "Change . . ." essay concerning cross-national data on turnout and involvement, a passage Burnham finds particularly upsetting, was included exactly to make the point that the simple equation Burnham (and many others) count on cannot be fully trusted: among the limited instances in which survey data underlying aggregate turnout data are available, there is a distressing proportion of cases showing high turnout to mean very nearly the opposite from the presumed politicized populace Burnham expects. This does not mean that Burnham is wrong or right in the specific case of the latter half of the nineteenth century, but it does mean that we should have more information before we leap to conclusions. We regret that this straightforward lesson continues to elude him, as he himself confesses, to the point that he argues it has neither logical nor aesthetic place in the essay.

As for the part of the caricature which charges us with overgeneralizing data from the 1950s, this was a piece of the growing myth that I was almost beginning to believe had been true in public print (if not in spirit) until I recently had occasion to re-read parts of *The American Voter*. I was pleasantly surprised at the liberal use of explicit caveats or other types of subordinate clause, all aimed at keeping the reader aware as to how situation-bound these data from the 1950s might be.

Indeed, there are lovely cases in point that address the immediate substantive issues Burnham emphasizes in this article. He raises the issue of generalizability most forcefully when it comes to the question as to whether "the level of political cognition in mass electorates" (is or) "is not more or less uniformly constant and low . . ." (p. 37). He attributes to us an argument of constancy and takes pains to instruct us that it would be reasonable to expect public attentiveness to surge forward in time of crisis, particularly "realigning" crises such as the 1860s and 1890s. When I look up the main discussion of low levels of public cognition of politics in *The American Voter*, I immediately find the point being made more than once in within the space of three or four paragraphs (pp. 255–256 of that volume), that of course the levels depicted from the 1950s' data would show some upward shift—magnitude unknown—in time of crisis.[1]

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[1] Actually, I am just finishing a manuscript which treats, *inter alia*, the features of public opinion, attitude formation and ideology that showed marked change in the crises of the late 1960s, as opposed to those features which, while not immutable, turn out to have been relatively inelastic despite great gains in public attention to politics. The hunches in *The American Voter* as to what elements would be more or less elastic in this sense turn out to have been quite accurate.

Similarly, there is frequent mention elsewhere in *The American Voter* that the 1950s did not represent a realigning crisis, thereby leaving unanswered what data patterns might look like under such circumstances. There are conjectures of some length concerning the impact of great crises like the Civil War. Moreover, we devoted a fair amount of analytic time to an effort to locate the residual imprint of some of the catalyzing political events of the more recent Depression realignment, such as cohort effects in class consciousness and the like. All of this is plainly enough written up, yet Burnham announces that this general body of work "is inherently static, without historical depth," and wanting in relevance to past political upheavals.

It would be sad indeed if we did not know a good deal more now about earlier electoral history than was available to us fifteen years ago; and surely the catalyzing crises of the late 1960s in the United States are giving us a firsthand view of what survey data look like at such a juncture, information also not available fifteen years ago. But the allegation that *The American Voter* and surrounding work left no room for such possibilities is an amazing one. For *The American Voter* is explicit, and persistently so, on that score. If knuckles are to be rapped, we suggest that Burnham, Pomper, and others who have been erecting this straw man rap their own knuckles until they take the time to read the text a little more carefully.

Comment: The American Electoral Universe: Speculation and Evidence*

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In the last two decades the empirical approach to political science has been heavily preoccupied with the study of contemporary voting behavior. Few have been sufficiently curious about or motivated by the mysteries of our electoral past to sustain a concentrated research effort in this direction. Yet, as V. O. Key often noted, a knowledge of our electoral past provides us with a better understanding of our electoral present, with how our current system has evolved and changed over time.[1] Only recently have scholars begun to heed the call of Key and new historians like Lee Benson and Samuel P. Hays for an empirical analysis of historical voting behavior. The initial research efforts, although often crude and unsystematic in method, have prompted interesting speculation as to "divergences" in our thinking from what previously had been assumed about past electorates. They have discovered "anomalies" in several different contexts—in earlier unconfirmed theories of voting behavior, in data-oriented contemporary work on the American voter, and even among the various historical research efforts themselves. While the presence of such anomalies characterizes the first stages of any research effort in virgin territory, unraveling the apparent inconsistencies must always be part of the process of a more fully developed cumulative research program. What is the task of the moment is to examine these anomalies in light of the theories and research of Professors Burnham, Converse, and myself—the principal elements in this continuing dialogue about the proper interpretation of our electoral past.[2]

Burnham's Theory

Burnham's theory of past electorates is contained in several pieces, not all of which hew to the same interpretation. His initial theory was presented in a famous 1965 *Review* article, "The Changing Shape of the American Political Universe."[3] This article changed many scholars' ideas of our electoral past, from views which held that our system of pluralist democracy and constituent parties was working well to ones which portrayed such a system as markedly "retrograde" and "deteriorating." The "golden age" of politics, to Burnham, was the nineteenth century, particularly the post-Civil War period. During that era the electorate was presumed to be active, issue-conscious, and "enlightened," reaching heights of political involvement which no further electorates have matched. Issues were presumably more important, the parties were more active and intensely competitive, and the electorate responded accordingly by being acutely aware of the issues and voting straight-party tickets.

The collapse of this remarkable system of electoral politics came in 1896 through an odd assortment of circumstances. Burnham, being something of an "economic determinist," felt that the exploitative aspects of the Industrial Revolution would lead to an inevitable confrontation between the captains of industry and the working classes in the United States as it had in other industrialized nations. Such a confrontation was even more likely to occur in America since, unlike its Western European counterparts, this nation had extended the necessary weapons for the masses to take over the government—universal male suffrage—before the Industrial Revolution began. The tensions between corporate power and the strong democratic and pluralistic elements in the political system came to a head in the 1890s. At stake was the power of government to be used to pursue either corporate or pluralist goals. Only the chance occurrence of two events prevented the inevitable confrontation from occurring: the depression of 1893 and the Bryan candidacy of 1896. The severe depression of 1893 alienated the working classes from their own party, the Democratic party, which was in power at the time, prompting their receptivity to the business-oriented Republican party. The Democratic candidate of 1896, Bryan, also was unable to relate to the needs and the demands of his party's urban constituency, further insuring that the workers would abandon their party. At the same time, McKinley preached the themes of a business-worker coalition and prosperity to entice the working class to the Republican banner.

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* Part of this paper was written while I was on leave to the Institute of Political Science, University of Aarhus, Denmark. I would like to thank the Institute for its support and encouragement. I would also like to thank Ellen Siegelman for her editorial assistance on the paper.

[1] V. O. Key, "The Politically Relevant in Surveys," *Public Opinion Quarterly*, 24 (Spring, 1960), 54–61.

[2] The dialogue first started in an exchange of views between Professor Burnham and myself in the December, 1971 issue of the *American Political Science Review*. For a proper understanding of the history of this dialogue and some of the arguments used in the current paper, see Walter Dean Burnham, "Communications to the Editor," and Jerrold G. Rusk, "Communications to the Editor," *American Political Science Review*, 65 (December, 1971), 1149–1157. For the statement which prompted this exchange, see Jerrold G. Rusk, "The Effect of the Australian Ballot Reform on Split Ticket Voting: 1876–1908," *American Political Science Review*, 64 (December, 1970), 1220–1238. The latest addition to this dialogue is Philip E. Converse, "Change in the American Electorate," in *The Human Meaning of Social Change*, ed. Angus Campbell and Philip E. Converse (New York: Russell Sage Foundation, 1972), pp. 263–337.

[3] Walter Dean Burnham, "The Changing Shape of the American Political Universe," *American Political Science Review*, 59 (March, 1965), 7–28.

The verdict of 1896 brought victory beyond expectation to the Republican Party. The business-Republican elite, according to Burnham, had succeeded in stealing the electoral system from the people through a series of fortuitous events. Once established in power, a realignment which was to leave them the majority party for more than thirty years, the Republicans proceeded to consolidate the Industrial Revolution, dictating the revolution's development without having to be subjected to mass pressures and demands. Corporate power had triumphed, and pluralist democracy suffered as a result. This elite capture of the electoral system would lead, in Burnham's words, to a major breakdown in party organization and competition in the aftermath of the 1896 election and, correspondingly, to an alienation of the voter from the political system. No longer would the voter feel his political participation was meaningful; no longer would he consider his party loyalty to be an important commitment to the system.

Burnham's second statement of his theory, his book on *Critical Elections and the Mainsprings of American Politics*, was more elaborate.[4] Drawing on the economic orientation of his previous work, Burnham expanded such notions into a more general theory of the interrelationships between the economic and political systems. The economic system was seen as constantly developing throughout history, while the political system was remarkably resistant to change and adaptation, partly because of the decentralized nature of the party structure and partly because of the pervading Lockian individualist ethic which implied a laissez-faire type of government. Unfortunately, the development of the economic system was not wholly smooth or beneficial, causing injury to various segments of society. Such economic dislocations, while harmful, were generally uncontrolled by the parties and government until they reached such a tension point that people tried to redress their grievances through political action. Once these economic dislocations spilled over into the political arena, a party realignment was usually inevitable, attempting to bring the antiquated party system into some kind of harmony with the more advanced and changing economic system. All realignments occur, according to Burnham, because of this fundamental tension between the developing economic system and the backward political system. Even the realignment of 1896 was a product of such tensions, although the direction this realignment took was largely unforeseen in the early 1890s. The fact that realignments occur periodically, according to Burnham's statistics, means that the problem of the interrelationships between the two systems is a continuing one, and only in realignment phases does the new majority party exercise some meaningful control over the economic system.

The notion of a periodic realignment cycle in politics caused by tensions between the economic and political systems leaves Burnham speculating about the possibility of a current realignment. While he hedges his bets, Burnham feels that such a realignment is probably occurring, possibly starting with the 1964 election. He sees the Democrats becoming the party of the technologically competent and superfluous strata—the top and bottom of the economic ladder—while the Republicans are moving more and more toward the defense of white "Middle America." Burnham, however, does not yet see a detonator to stimulate the massive tension needed to complete the realignment, although he considers the socioeconomic implications of both the Vietnam war and the urban crisis as possibly significant. At the same time, he advances another possible prediction from his theory—that the parties will decay and fade away rather than realign since they no longer perform the functions necessary to bind the voters to them. This is Burnham's well-known "end of parties" argument—an argument which, to Burnham, may become reality if the parties cannot control the economic system for the benefit of the people, cannot integrate the people into the political system, and cannot effectively ally the voters to the party. In this sense, he says, the party system may have already moved beyond the possibility of critical realignment because of the dissolution of party-related identification, leaving only the possibility of an era without parties as we know them.

Burnham's emphasis on economic causes of change in the electoral universe is so strong that it leaves little room for the consideration of other possible causal variables. Nevertheless, Burnham, in his book, does consider some of the legal-institutional factors I have proposed (e.g., ballot, registration systems) as possibly useful for his theory in the crucial 1896 realignment period.[5] While noting that such variables had some effects on voting behavior, Burnham concludes that the breakdown in party organization and the decline in voting turnout in this period were due mainly to systemic rather than rules-related causes. Attempting to incorporate legal-institutional factors into his thesis rather than revise his central contentions, Burnham states that the changes in these rules (ballot, registration rules) were "devices by which a large and possibly dangerous mass electorate could be brought to heel and subjected to management and control within the political system appropriate to 'capitalist democracy.' "[6] In other words, implementation of new institutional requirements defining the electoral system could have effects on behavior in the direction predicted by Burnham's theory, but this was only natural since these laws had been deliberately enacted to have these effects by the capitalist elites after they came to power.

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[4] Walter Dean Burnham, *Critical Elections and the Mainsprings of American Politics* (New York: W. W. Norton, 1970).

The last statement of Burnham's theory is the present article in the *Review*.[7] In many respects, it is similar to the two earlier pieces, but three distinctions remain. First, Burnham relies partially on another theory—Jensen's religious-ethnic theory—to explain the high mobilization and straight-ticket voting rates of the electorate prior to the 1890s.[8] (The reader will also note that Kleppner espouses a theory similar to Jensen's which will be referred to in subsequent sections of this paper.[9]) Prior to this time, Burnham depended mainly on an interpretation of an active and enlightened electorate motivated by issues and the party organization.[10] The portrayal of "militaristic drilling" of party loyalists to the polls by the party organization—an idea also borrowed from Jensen—characterizes the earlier Burnham work even before Jensen's religious-ethnic theory became salient.[11] The later adoption of the religious-ethnic theory represents a partial departure from Burnham's strong economic orientation. It will be shown later that too heavy a reliance on Jensen's theory leaves some internal contradictions between the two authors.

The second distinction in the present Burnham work is his shifting of crucial dates marking the deterioration of the electoral response. In his 1965 work, Burnham maintained that 1896 was the date of record:

> This revolutionary contraction in the size and diffusion in the shape of the voting universe was almost certainly the fruit of the heavily sectional[12] party realignment which was inaugurated in 1896. This "system of 1896," as Schattschneider calls it, led to the destruction of party competition throughout much of the United States and thus paved the way for the rise of the direct primary. It also gave immense impetus to the strains of anti-partisan and anti-majoritarian theory and practice. . . .[13]

In his latest work, in order to explain certain anomalies, Burnham pushes the crucial dates back to the beginning of the 1890 decade (or earlier) when he says: "Unhappily for analytic certitude, the period 1890–1896 is not only one of ballot reform, but also of nationwide critical realignment. . . . It would thus be reasonable to posit a cause or causes arising within the American sociopolitical system in this period which generated Populism, massive landslides, critical realignment, and the Australian Ballot reform."[14] Actually, the main new element in this statement allowing the moving back of dates is Populism. Once this is introduced, the new reference point becomes the birth of the Populist Party (1891).

The last addition to Burnham's theory is a matter of emphasis rather than idea. Considering legal-institutional variables once again in his theory, Burnham finds that they are substantially more important than his earlier 1970 reading—in this case, explaining at least one-third and sometimes up to two-thirds of the variance in voting behavior. Still, despite their upgraded importance, the behavioral or systemic theory is considered superior in explanatory power, although Burnham now maintains that both structural and behavioral theories must be included in any final theoretical determination of the forces involved in the crucial turn-of-the-century period: " . . . we can cheerfully get to work on constructing a better model of change than the one which I stated in 1965—a mode which incorporates structural and behavioral components, gives each its proper weight, and integrates both into theory."[15]

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[5] For a description of these legal-institutional factors, see Rusk, "The Effect of the Australian Ballot Reform."

[6] Burnham, *Critical Elections*, p. 90.

[7] Walter Dean Burnham, "Theory and Voting Research: Some Reflections on Converse's 'Change in the American Electorate,' " *American Political Science Review*, 68 (September, 1974), pp. 1002–1023.

[8] Richard J. Jensen, *The Winning of the Midwest: Social and Political Conflict, 1888–1896* (Chicago: University of Chicago Press, 1971).

[9] Paul J. Kleppner, *The Cross of Culture: A Social Analysis of Midwestern Politics, 1850–1900* (New York: The Free Press, 1970).

[10] What the particular issues were is left largely undefined by Burnham.

[11] Burnham first referred to Jensen's notion of a "militaristic" campaign style in his book, *Critical Elections*. His source was Jensen's paper, "American Election Campaigns: A Theoretical and Historical Typology," delivered at the 1968 Convention of the Midwest Political Science Association. Three years later, Jensen incorporated this idea as a subsidiary element in his broader religious-ethnic theory, published as the book, *The Winning of the Midwest*. It is the latter source that Burnham quotes in his present paper.

[12] The reader should not be confused by Burnham's use of the word "sectional." As a reading of his 1965 article makes clear, the "sectional" realignment was actually underlined and hence caused by an "economic" realignment. See Burnham, "The Changing Shape," especially pp. 24–26.

[13] *Ibid.*, p. 23.

[14] Burnham, "Theory and Voting Research," p. 1016.

The empirical base on which the Burnham theory rests depends on which aspect of it one considers—the transition from the enlightened to the deteriorating electoral period, or the modern period with its alternate possibilities of a new emergent realignment or the end of parties. For the moment, our concern will be only with the transition from the enlightened to the deteriorating period—the main emphasis in Burnham's work. With regard to this transition, Burnham has looked at the changing distribution curves across time of five key variables of voting behavior—split-ticket voting, roll-off, turnout, partisan swing, and drop-off. Basically, Burnham noted that turnout levels were high while split-ticket voting, roll-off, drop-off, and partisan swing levels were low up to the 1890s. After that, the reverse became true—turnout started declining, and the other voting indices began to rise in value. Burnham believes these changes are linked to the 1896 election, the electorate showing a steadfast party loyalty and a high mobilization prior to this date and the exact opposite thereafter—the latter being interpreted as the subsequent peripheral and alienated nature of the electorate after the business-Republican elite captured the electoral system and reigned dominant for at least a generation thereafter. Following these curves across time, one also notes further increases in these indices of voter peripherality in the 1920s which were only reversed, to a certain extent, in the realigning period of the 1930s. Without much risk of error, one can say that almost the entire Burnham theory relies simply on how these five indices behave across time.

General Perspectives

By any definition of the term "theory," Burnham has to be classified as a large-scale or grand theorist. Theory construction at this level of abstraction has both assets and liabilities. On the one hand, it can extend our range of vision to the complex interrelationships between macro-level variables. The new insights and perspectives gained can often lead to specific, detailed research projects—selecting at given times small chunks of the much larger theory in order to nail down our empirical knowledge on a certain score. On the other hand, in its present state, such theory is too large-scale and broad to permit adequate empirical testing taken as a whole. Its very level of abstraction actually precludes some portions of the theory from ever being tested. Also, this type of theory is potentially all-inclusive—being able literally to encompass every facet of political life, whether it seems an apparent contradiction to the theory or not.

A specific example of the problems with such theory is the way in which Burnham can use it to explain any anomalies that other research poses. If the effects of legal changes are considered as an alternate and perhaps more viable explanation of the behavior Burnham predicts from his theory, the theory is sufficiently broad that it can be used to encompass such empirical facts and events, even if, on the surface, they do not seem so readily reconcilable with the Burnham theory. This is exactly what has happened when Burnham has included, at a later stage in his theoretical statements, the Rusk-Converse concern for the effects of legal-institutional variables such as ballot and registration systems.[16] The laws are incorporated into the theory, being viewed as merely another way in which the capitalists consolidated their power and weakened the opposition party. Whether in fact such laws were passed with this intent in mind is an empirical question which I will address in greater detail below, but at this stage it will suffice to say that Burnham's theory can subsume many such facts and events, reconciling them with his more general thesis. One does not know, however, if such a reconciliation paints an accurate picture of reality, and this point should be of some concern to empirically-oriented political scientists.

The broad and abstract nature of the Burnham theory also poses problems for empirical testing. Burnham is dealing with such concepts as business elite take-overs of voting systems, breakdowns in party organization, and psychological alienation of the voter from the political system. It is extremely difficult to operationalize such concepts, and it is equally difficult to find empirical information on them. Historical accounts of such matters are often scarce or of dubious validity, and the psychological aspects of past political behavior are difficult to probe, unlike their contemporary accessibility through sample survey techniques. Such problems put the empirical political scientist in an awkward position—calling for broader-ranged theories, such as Burnham's, to stimulate inquiry, while at the same time realizing that key aspects of these theories may be difficult, if not impossible, to verify by empirical means.

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[15] *Ibid.*, pp. 1019–1020.

[16] For another statement on this same point, see Richard W. Boyd, "A Review of Walter Dean Burnham's *Critical Elections and the Mainsprings of American Politics*," *Midwest Journal of Political Science*, 15 (August, 1971), 595–597, especially 596.

The actual evidence Burnham uses to support his theory points up the very problems of verification to which I have been alluding. The 1965 article by Burnham must be essentially viewed as laying out the distribution of five dependent variables and then attempting to set forth a theory to explain the systematic changes in them. The theory may be correct, partially or completely, or it may not, but it is obvious that there has been little attempt to operationalize the concepts in the theory (e.g., elite conspiracies, party organizational strength, voter alienation) as independent variables in order to test empirically their relations to the systematic changes in the dependent variables of split-ticket voting, turnout, etc. What one has is simply the distribution of five dependent variables—Burnham's theory is an attempt to explain the changes in these variables, but other theories, such as the Rusk-Converse institutional theory, can also be offered as explanations for them. The merit of the latter theory is that it can be subjected to empirical testing, i.e., the legal variables can be operationalized and tested against these dependent variables. Without an empirical grasp on some such independent variables, one is left speculating about the meaning conveyed by these dependent variables of voting behavior.[17] As Converse notes, "... most single time series of broad societal interest represent little more than the movement of superficial 'symptoms' whose true causes and genuine human significance remain quite unspecified."[18]

Even the data displays of these dependent variables show the limited empirical range within which Burnham is working. Except for his turnout figures, Burnham contents himself with presenting empirical information on only a few states—usually four or five. The same procedure is followed in his later works, his *Critical Elections* book and the present piece in this *Review*. Given such a limited data base—and the fact that this data base is only concerned with dependent variables of voting behavior (except when Burnham uses the Rusk-Converse legal variables)—the sweeping assertions derived from such data displays must be viewed with some caution. The apparent irony is that the most wide-reaching conclusions, the most general and sweeping of theories, are predicated on the most limited of data bases. This irony leads me to conclude that while Burnham has proposed a most interesting and ingenious theory—one which will stimulate research for years to come—it cannot be regarded as empirically verified at this point. One can speculate that most of the anomalies encountered in this dialogue are perhaps a natural consequence of dealing with a theory which is so revolutionary in outlook but so limited in its empirical foundation.

The Enlightened Electorate

The crux of Burnham's theory lies in the assumption that our political system had a golden age from which we have seriously declined. If this assumption of a golden age is inaccurate, the remainder of the theory concerning electoral deterioration falls by the wayside. In the early Burnham work, this assumption seems to be based on a survey of turnout and split-ticket rates for the period preceding the 1890s—high turnout and low split-ticket values apparently signaling an active, involved, issue-conscious electorate with strong party loyalties. In the later Burnham work, this assumption appears to rest on Jensen's and Kleppner's historical accounts which emphasize the primary influence of local factors on the electorate—particularly, religious and ethnic backgrounds.

The first step in analyzing Burnham's assumption is to determine if the electorate was really as active and involved as the turnout figures tend to indicate. If the figures are taken at face value, they do comment on the massive extent to which citizens participated in elections, although it is a major inferential leap to conclude from such statistics that the electorate was also highly issue-conscious and sophisticated. According to Converse, however, such figures cannot be taken at face value since a corruption factor also entered in during this period, artificially inflating the turnout values.[19] The key question is, of course, how important was the corruption factor of this period in terms of the sheer number of votes involved. Burnham may be right in saying that the exact figures for states will never be known, but his criticism of "colorful history" overplaying the prevalence of vote fraud leaves something to be desired in light of his failure to present any contradictory evidence. Also, a careful reading of Jensen's work fails to show documentation for his concluding, as Burnham did, that vote fraud was probably not a widespread or major phenomenon.[20]

One argument Burnham uses to support his claim of little fraud is that traceable corruption was a dangerous enterprise for its practitioners. My research on the unofficial or party ballot which existed in this period, and the electoral milieu in which it was dispensed and voted, indicates just the opposite.[21] The parties instituted and maintained the unofficial ballot system expressly to control the vote, to guarantee knowledge of how people voted. Bribery, intimidation, and corruption were facilitated simply because the ballot system required public voting rather than a secret vote. The vote buyer could guarantee by simple observation that his purchases were voted as he instructed.

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[17] For a similar viewpoint, see Rusk, "Communications."

[18] Converse, "Change in the American Electorate," pp. 263–264.

[19] *Ibid.*, pp. 276–295.

[20] In any event, Jensen's analysis was only concerned with six states.

Another condition existed which favored vote fraud—the weak or nonpersonal registration system. This system required no proof that the person presenting himself at the polls be the same as recorded in the poll book other than a simple declaration to that effect. This allowed such vices as "repeating votes," "voting the graveyard," and the like.[22] Without some proof as to identity, such as a handwritten signature, or a certain diligence in purging the poll books from election to election to eliminate those who had died or moved away, the attraction for fraud in the form of vote buying and selling was prevalent and, indeed, necessary if the competition was also known to be engaging in such activities.

Both the weak registration and the unofficial ballot systems had to exist in order for corruption to flourish. The registration system allowed people to vote who were not qualified, often permitting a single person to vote many times; and the ballot system with its open vote guaranteed the knowledge that purchased voters were voting as instructed. If either one of these elements had been missing, it would have been much more difficult for corruption to play an important part in the political process. But it was no accident that the two systems both existed—they had been expressly implemented by the parties for that purpose: to control the vote. It would only be with the widespread clamor from the public to end this corruption—in itself highly indicative of the prevalence of corruption at this time—that reform would come in the form of the secret ballot (the Australian ballot) and a strong personal registration system, both being enacted in the 1890s and both being successful in reducing the corruption of earlier decades.

The probability that considerable corruption existed in this period simply because the machinery for it was established and used casts some doubts on Burnham's claim of an enlightened era. The most anomalous finding of all would be that an enlightened electorate could have existed side-by-side with a massive corrupt element. Yet this may well have happened, the enlightened electorate eventually pushing for the ballot and registration reforms to end this corruption. But was this electorate as enlightened as Burnham wishes us to believe? What motivated this electorate to act as it did in the political arena? Would contemporary electorates be wise to follow the example of such an earlier electorate? Questions such as these prompt us to analyze critically some of Burnham's beliefs about such an electorate.

To Burnham, the nineteenth-century electorate, especially the post-Civil War electorate, was (1) active, (2) issue-conscious, and (3) party-oriented. Of these three assumptions, the third is easiest to believe. Despite Burnham's assertion that survey findings can rarely be extrapolated to time past without considerable risk of error, Burnham himself relies on one of the central concepts of contemporary voting studies—party identification—for his theory of the nineteenth-century electorate. According to Burnham, this electorate had a strong party loyalty which was reflected in its heavy straight-ticket voting. One would not deny that a sense of party identification was prevalent at this time as it is in the present day—hardly an anomaly—or that heavy straight-ticket voting occurred. It was not party loyalty alone, however, that dictated a straight-ticket vote in the nineteenth century—the party ballot in use at the time, as I have shown elsewhere, virtually prevented a split-ticket vote being cast.[23] The aura surrounding the polls was also one of heavy party pressure and intimidation.[24] In addition, many straight-ticket votes were cast by bribed voters. While party identification is an important variable in this era, it does not by itself explain the tendency toward straight-ticket voting. Even Burnham, in his later statements, acknowledges the presence of other factors—notably the ballot—when he says: "I take it as axiomatic that the pre-1888 absence of split-ticket voting was largely an artifact of the unofficial party ticket. . . . "[25]

Forgetting for the moment the importance of such factors as the party ballot and the bribed voter and concentrating only on party identification, one still must raise a central question: Would an enlightened electorate always vote a straight-ticket? One's conception of such an electorate would generally be that of rationally deciding its vote on the basis of the issues and the candidates' stands on them, not on the basis of party. The fact that Burnham emphasizes the importance of issues in this period gives further credence to such a conception of an issue-voting public. Under what circumstances, then, could the public vote on the basis of the issues and still cast straight-party tickets? The only situation in which the two would seem to occur together would be if the "responsible parties" doctrine held for the late 1800s and if the parties kept their same distinct positions on the issues across the entire time span (since Burnham not only shows very little split-ticket voting within election years, but also very little crossover of party voting between election years). The fact that the 1865–1890 period was mainly one of very close elections would seem to argue against a responsible parties theme in favor of a Downsian notion of the two parties vying for the crucial center of the electoral distribution by making increasingly ambiguous and vague statements on the issues. The Downsian logic, however, would not work if the electoral distribution was bimodal (polarized) rather than the unimodal situation he conceives the American case to be.[26] The other alternative would be for the parties to change issue positions over time to gain new elements in their coalitions (hopefully not losing past ones as a result), something which actually happened with some of the religious-ethnic issues of the period according to Jensen and Kleppner. Yet, if the parties changed their issue positions, the electorate did not seem to respond, since Burnham's partisan swing index shows few such crossovers of party voting. In short, although issues may well have been prevalent in this period, a supposition to be treated below, the enlightened electorate did not seem to be responding to them, at least not in the magnitude presumed by the Burnham theory. Burnham's later adoption of Jensen's militaristic notion of campaigning—of merely drilling the enlightened to go to the polls to vote their party identification—seems more appropriate for this era.

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[21] See Jerrold G. Rusk, *The Effect of the Australian Ballot Reform on Split Ticket Voting: 1876–1908* (doctoral dissertation, University of Michigan, 1968); chapter 2. Also see Eldon C. Evans, *A History of the Australian Ballot System in the United States* (Chicago: University of Chicago Press, 1917).

[22] Converse, "Change in the American Electorate," pp. 282–285.

[23] Rusk, "The Effect of the Australian Ballot Reform" (1970).

[24] Rusk, *The Effect of the Australian Ballot Reform* (1968), chapter 2.

[25] Burnham, "Communications," p. 1150.

The tenuous position of issue voting in the Burnham theory does not derive from a historical analysis alone. Current survey information shows that education is positively correlated with issue salience. If one can extrapolate this notion to time past, then it is difficult to explain how the earlier American electorate could have been so thoroughly issue conscious in the virtual absence of much formal education. Contrary to Burnham's claim that few were illiterate in this period, the figures show that 20 per cent were illiterate in 1870 and 40 per cent had less than a high school education.[27] Also, at this time, there was little formal development of a mass communications system which we take for granted today. As a result, it was more difficult to confront the voters with the issues, or for the self-motivated voter to obtain information on them. The general reply to these arguments is that local newspapers filled the mass media vacuum. But even if this is true, these papers seem to have been more propaganda rags stirring up party loyalty than conveyors of in-depth information on the issues.[28] One wonders, in any event, how much they were actually read given the lack of education among the people.[29]

In light of these anomalies, Burnham seems to rely on the assumption that the electorate had a strong intrinsic interest in politics. Visions of C. Wright Mills's "crackerbarrel society" come to mind—of an interested and enlightened electorate despite the twin handicaps of little educational training and little mass-media input.[30] In defending his intrinsic interest argument, Burnham, at times, even turns these handicaps into assets by particularly clever pieces of reasoning—such as when he says that the primitive communications technology actually worked for, not against, an interest in politics by virtue of not distracting the electorate from the only "spectator sport" in town, politics. Overall, however, the assumption of such a strong interest in politics depends on politics being central rather than peripheral in people's psychological life space. Contemporary theorists assert that the daily affairs of individuals—their family, friends, and jobs—and *not* their politics, are the primary concerns in their life space. In an earlier age when making a living was more difficult and taking care of the family probably more pressing, it is not easy to accept the notion that politics so deeply and constantly engaged the people's attention.

The prototype of Burnham's enlightened electorate—the essence of crackerbarrel society—was the rural folk who supposedly were more bourgeois and modern in outlook than earlier "peasant-oriented" theories have led us to believe.[31] It is in just such a context that Burnham's assumptions of intrinsic interest are most severely tested—since rural residents suffered more from the handicaps of little education and lack of access to communications media than did most other groups in society. To a certain extent, these conditions still characterize rural society, as contemporary survey evidence shows, with the corresponding results that rural people, in general, show less interest in politics and lower turnout rates than city dwellers.[32] What Burnham is asking us to believe is that an earlier electorate characterized by these same handicaps, only more so, behaved in just the opposite fashion from their contemporary counterparts. Burnham cites the high turnout values of a few select rural electorates of the past as his evidence for this point. While turnout does not necessarily imply the deep and abiding interest in politics that Burnham assumes, the more immediate question is why the rural turnout figures were high in the first place. Converse's corruption argument comes readily to mind, although Burnham contends that rural elections were basically not tainted with vote fraud. Harris disagrees: "It is sometimes supposed that election frauds are confined to large cities. The plain facts are that the elections in rural districts are conducted much more irregularly than in cities and often fraudulently."[33]

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[26] Anthony Downs, *An Economic Theory of Democracy* (New York: Harper and Row, 1957), especially chapter 8.

[27] See Philip E. Converse's original version of his paper, "Change in the American Electorate," p. 16.

[28] See Jensen's descriptions of the newspapers as "party presses" in *The Winning of the Midwest*, pp. 5–6, 172, 196–197.

[29] One imagines that a large part of the elite dialogue on issues that was carried in the newspapers attracted only the attention of a small segment of the population. Extensive elite dialogue does not necessarily imply a widespread interest on the part of the public. While the elites may or may not have known the limited audience to which they spoke (many still do not even have accurate notions on this score today), it seems most reasonable, given the lack of education, that only the elite and politically active, a small minority of the population, paid much attention to such debates.

[30] Burnham actually cites Mills's image of the "crackerbarrel society" in "The Changing Shape," p. 16. Also see C. Wright Mills, *The Power Elite* (New York: Oxford University Press, 1956), pp. 298–324.

While it is an appealing explanation, probably fraud alone cannot account for the high turnout of rural electorates. Realizing this point, Converse maintains that a considerable amount of deferential voting also occurred, mobilized by rural political leaders.[34] In one sense, this argument supports Burnham since it relies on the importance of viable party organizations and leaders to mobilize the vote (and this, according to Burnham, was an era of strong party organizations), but, in another sense, it does not, since the product of the mobilization effort—deferential voting—severely questions the political interest and enlightenment of rural electorates. Once strong registration systems came to rural areas, one would suspect that they would not only eliminate the corrupt vote but also a considerable segment of this deferential vote since it would be heavily composed of the less interested, less involved elements of rural society. Important declines in rural turnout after the turn of the century could well be explained by the implementation of such registration systems. Burnham shows a few cases, however, in which registration was not implemented and yet turnout still declined. While the cases are too few to speculate about the generality of the trend, if the pervading ethos of corruption-free elections did not discourage the artificial aspects of rural turnout, then no simple logic exists to explain the political behavior of rural electorates in these particular cases.

These arguments concerning the causes of rural turnout can be applied to the urban setting as well. A definite, and probably sizable, corruption factor undoubtedly existed with the rise of the political machine. At the same time, the machines probably mobilized large segments of the voting population from the lower class, immigrant, and transient sectors of the city. Whether such a vote can be characterized as deferential may be questionable, but the fact that large numbers of lesser involved people voted who probably would not have done so without the impetus of the party machine speaks well for the deferential argument. A finding which tends to bolster both the corruption and deferential arguments is one which shows that in many cities the transient districts voted on a par with, or in greater proportion than, the silk-stocking districts.[35] There seems to be no plausible explanation for such behavior other than the one given here. All of this tends to show that while Burnham had a handle on one important variable—party organizational effort—he misinterpreted the effects it had on the electorate: it did not produce an electorate with a great interest in politics but only one which was large in size.

With these reservations in mind, Burnham has changed much of his theory of the nineteenth-century electorate in the present article. While there were slight inklings in his earlier writings, Burnham now comes out quite clearly and forcefully for the Jensen-Kleppner theory of religious-ethnic effects. He calls Jensen's book, in the present article, "a seminal work which is 'must' reading for anyone interested in diachronic analysis of American electoral politics."[36] One can understand Burnham's enthusiasm for Jensen's book by noting that, in certain important particulars, it might well free him from some of the criticisms of his earlier work. Since his theory is so broad, Burnham seems to be able to assimilate the

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[31] Burnham, "Theory and Voting Research," pp. 1018–1019. Also see Louis Hartz, *The Liberal Tradition in America* (New York: Harcourt, Brace, 1955).

[32] Angus Campbell, Philip E. Converse, Warren E. Miller, and Donald E. Stokes, *The American Voter* (New York: John Wiley, 1960), chapter 15.

[33] Joseph P. Harris, *Election Administration in the United States* (Washington, D.C.: The Brookings Institution, 1934), p. 370.

[34] Converse, "Change in the American Electorate," pp. 287–288, 299–300.

[35] *Ibid.*, p. 289.

[36] Burnham, "Theory and Voting Research," footnote 27.

Jensen-Kleppner theory without apparent contradiction. The synthesis, however, actually involves Burnham in some major difficulties which will be treated below: the question of whether economic or ethnocultural factors are the primary influences on voting behavior and the problem of whether the consequences of the 1896 election are viewed as good or bad for the political system.

The Jensen-Kleppner theory views religious and ethnic conflict as determining voting behavior in the late 1800s. The intensity of religious-ethnic values would provide the impetus for voters to be active, attentive to the issues, and loyal to their parties. The basic conflict is usually summarized as that between the "pietists" and "liturgicals"—those who wanted to reform society morally in their image (ending drinking, parochial schools, etc.) and those who wanted freedom for their particular religious practices and social customs. Although the claim is made that religious factions usually coincided with distinct ethnic groupings, there was by no means a one-to-one correspondence between the two, and hence Burnham's assertion of "island communities" becomes somewhat less viable. To the extent that the theory has merit, however, it would seem, at least on the surface, to support Burnham's notion of an active, involved electorate.

But the theory seems to have several limitations, both in conceptualization and in research design. The assumption is initially made that religion is a salient factor for political behavior throughout the period studied. Correlations between religious affiliation and the vote are shown as evidence for this assumption, but mild correlations can often appear between these two variables when they have no meaningful causal connection between them. In contemporary research, we find correlations between the two, but usually they are explained by a third factor. Only in unique electoral situations—such as 1928 and 1960—does religion seem to take on a political salience of its own among a substantial portion of the electorate. This may not have been the case, of course, in the late 1800s—perhaps religion was salient politically throughout this period. From the evidence presented, we simply cannot know for sure. Granting for the moment that religion might have had an independent effect on voting, one then encounters another problem: the failure to employ a multivariate design to pinpoint the relative effects of religion vis-à-vis other possibly important ariables.[37] One has the feeling that Jensen and Kleppner might have discovered one useful factor in this period, but that their exclusive reliance on it results in a large overstatement of its importance. The possibility of such an overstatement arises from the sample base as well—Jensen used only six states in the Midwest to test for religious effects and Kleppner used only three states in the same region. Any generalization of the theory to other regions would be tenuous at best.

If for the sake of argument the Jensen-Kleppner theory is accepted at face value, one can see that it reshapes the Burnham theory in certain key respects. The most obvious reshaping results from the diminishing role economic factors play in the Burnham theory. No longer are such conditions as the exploitative effects of the Industrial Revolution, the depression of 1873, and the agrarian plight the key factors of attention. They have been replaced in importance by ethnocultural factors on the local level. A necessary corollary of this is that party identification is no longer seen as rooted in the sectional-economic strife of the 1850s or in the economic exploitation of the late 1800s but instead is taken to have a local base in religious and ethnic groupings. National campaign issues in general and national economic issues in particular are not the important issues of the day, but instead a sort of pseudo-issue emerges which is actually nothing more or less than an extension of the value systems of religious-ethnic groups. The entire orientation of politics seemingly changes in the late 1800s, presenting Burnham with both anomalies and opportunities.

The opportunities would arise because presumably religious conflict was so intense in this period that it had to spill over into the political system. Religious groups so intent on preserving their value systems would be sufficiently motivated to search out the party sympathizing with their views and vote in large numbers for it. The intensity of the conflict would force the parties into a bipolarized situation, a situation in which responsible parties would exist and the unimodal Downsian pattern would have no applicability. Parties would have little trouble mobilizing the people to vote under such conditions, the militaristic drilling merely reinforcing people's natural inclination to vote. The credibility of this scenario could be bolstered by the simple argument that religion and ethnicity are more important to people than economics—that values are more difficult to compromise than interests—and Alford could be aptly quoted as supporting this point of view.[38]

The anomalies which this approach poses for

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[37] For example, Burnham cites economic factors as important to his theory—a key question would be how important were such factors in relation to religion, something which could be tapped by multiple regression analysis. While Jensen and Kleppner do look at occupation and class variables, respectively, they do so only in a limited and cursory sense, never employing such variables in any multivariate analysis.

[38] Robert R. Alford, *Party and Society* (Chicago: Rand McNally, 1963).

Burnham comment on his entire view of the enlightened electorate. They raise such fundamental questions as the following: Is an electorate which plants itself in a noncompromising, polarized position an enlightened electorate, an electorate which we wish to emulate? Are the parties which foster and reinforce such patterns the type of responsible parties we want today? An era of conflict based on religious-ethnic values and prejudices hardly seems like a model for contemporary politics to follow. Even the issues surrounding these conflicts, which seem so crucial to Burnham, resemble more "style" than "position" issues—intensely held negative group referents in which there is only one side to the issue.[39] Morality and religious crusades often degenerate into style issues, but these seem just the opposite of the types of issues we need to stress today. The debate on local values—instead of national issues—seems to represent a retrograde, not a stable or developing trend in politics. Alford heartily agrees, saying that religious and ethnic conflicts are detrimental to the stability of a society because compromise is difficult to obtain, while debates on economic matters can eventually lead to a focus on the nation-state and a compromise on the issues.[40]

Finally, it must be emphasized that only if religion generated a tremendous intensity in the late 1800s could it be useful for the Burnham theory. The intensity component is the most important part of the argument because, without it, such a massive mobilization of the electorate could not have been obtained. Even with it, one wonders how much it could have motivated the lesser involved elements of the electorate—the worker, immigrant, and transient. Even a national survey of the parties at the time does not show the major "playing" of the religious issue that Jensen and Kleppner suppose occurred. It seems instead that the religious issue was a more local phenomenon—probably affecting local races more than national ones. With national elections so close, the parties may well not have wished to heat up such an issue for fear of alienating the middle-of-the-road votes they needed for victory. All of this presupposes the importance of a religious issue in the first place, a condition by no means fully documented. Instead, the political salience of religion in this period should probably be seen as more an interesting theoretical question than as a proven empirical fact. Even without this qualification, the religion variable still seems to present anomalies which, when joined with others mentioned for this period, seriously question Burnham's inference of an enlightened electorate. The anomalies discovered in connection with the Burnham theory, however, are not confined to the pre-1890 period alone, but continue to manifest themselves as the electoral scene proceeds to the 1890s and the crucial election of 1896.

The 1896 Election

Political behavior in the decade of the 1890s seemed to depart in significant ways from that of the earlier decades. Voting behavior became more volatile as turnout started to decline (save for the election of 1896) and split-ticket voting and partisan swing began to increase. The problem was to account for these changes in voting behavior. Burnham, in his early work, attempted to explain these changes as a result of the 1896 election. This particular election was extremely crucial to Burnham since the forces shaping it, as he perceived them, justified his theory of economic determinism and electoral deterioration. In his latest piece, Burnham has moved the dates somewhat backward in time, vaguely referring to the entire decade of the 1890s, but it is clear that his emphasis is still on the Schattschneider-defined "system of 1896." The only rationale for moving the dates back seems to be to account for changes which occurred in these voting patterns prior to 1896.

The Burnham explanation for the changes in these voting patterns rests with economic forces—the business-Republican elite capturing the electoral system in 1896 in order to consolidate the Industrial Revolution. This Republican-business hegemony was assumed by Burnham to have resulted in the decline of party organization and competition, with the inevitable consequence that the voter either withdrew from the political system (evidenced by low turnout figures) or vacillated in his choices between the parties if he stayed within the system (evidenced by high split-ticket and partisan swing figures). What enabled the business elite to capture the system was another economic force—the depression of 1893 which drove the mass base of the Democratic Party into the hands of the Republicans. If that was not sufficient cause, many disgruntled Democrats left the party because of either fear or hatred of their candidate, William Jennings Bryan. Workers thus united with the upper-class business elite to form one of the oddest coalitions in American history—a coalition which was dominated by and run expressly for the business elite.

Burnham faces a crucial dilemma in using this theory for the 1890s period. While the theory is consistent with his early work, which stressed an economic interpretation of elections and political history,[41] it seems at variance with his later adoption of the Jensen-Kleppner theory. Having adopted the Jensen-Kleppner theory for the pre-1896 period, Burnham suddenly has to drop it as he approaches the 1896 election to avoid downplaying (or even ignoring) his more basic economic-business conspiracy theory. The heated religious-ethnic battle of the previous decades suddenly dies away almost by fiat, disappearing from the political scene in the wake of economic forces. While this may have actually happened, it would seem, from the Alford perspective, that such a life-and-death struggle over religious values and moral issues could not suddenly come to a halt, even in the presence of an economic depression like that of 1893.

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[39] Kleppner also refers to these issues as "style issues." See Kleppner, *The Cross of Culture*, pp. 149–178, 371–375.

[40] Alford, *Party and Society*, chapters 10–11.

[41] See, in particular, Burnham, *Critical Elections*, chapter 7.

Kleppner agrees with the Alford point of view. He sees the religious strife continuing into the 1896 election, although in a somewhat different form from earlier elections. The two parties, in 1896, changed their religious bases of support—the pietists going to the Democrats under Bryan's evangelical leadership, while the liturgicals switched to the Republicans who were trying to create a new image of cosmopolitan integration. Previously, the two parties had conveyed exactly the opposite images.[42] While Kleppner notes that some of the religious shift was conditioned by the economic misfortunes of the 1890s, he neither places the emphasis Burnham does on this economic event, nor does he believe that the business elite captured the electoral system in 1896. Kleppner maintains the primacy of religious-ethnic variables in the 1896 election and, indeed, throughout the 1890s.

While Jensen agrees with Kleppner on several points, including some semblance of a pietist-liturgical division in 1896, his theory is much more complex. The predictions from his theory would lead to the same data findings as Burnham's predictions, but Jensen's interpretation of electoral history is fundamentally different. This is especially anomalous because Burnham quotes Jensen as if they are basically in agreement on the Burnham theory. In Jensen's eyes, a transition in politics was occurring in the 1890s—from the religious battles of the pietists and liturgicals, typified by militaristic campaigning, to a "politics of pluralism" ushered in by William McKinley, the Republican candidate, who believed that groups should not be antagonistic to one another but integrated into a broader coalition working together for security and prosperity for all. McKinley also believed the older militaristic campaigning should be abolished in favor of an "advertising" or "merchandising" style—trying to educate the people on the issues (such as the tariff, the issue for which McKinley was the chief spokesman in this period). According to Jensen, McKinley deliberately set about forming a new Republican coalition, one in which the pietists would have no part since he disagreed with their stringent views and also thought they threatened the electoral success of the party. With McKinley pushing the pietists out in his move to establish a pluralistic base, the only remaining home for them came under the banner of Bryan, the populist evangelist. The new Republican coalition would be a combination of liturgicals, workers, ethnic groups, and capitalists, with a distinct liturgical-pietist division still remaining visible in 1896.[43]

This theory departs from Burnham's in several ways. First, Jensen agrees with Kleppner that religion was still an important variable in 1896, although diminished in importance from previous years. Second, Jensen believes that the other fundamental causes of the 1896 election were basically not economic but political. McKinley's move to a pluralist-based party was crucial. Economic conditions helped such a new strategy, but without this strategy, the electoral decision might well have been different from what history records. Third, Jensen sees the consequences of the 1896 election as good, not bad; this is probably his most important disagreement with Burnham.[44] Jensen views McKinley as making one of the most positive contributions in the history of American politics—dampening and eventually stopping the religious wars by ousting the pietists from the party and starting a new pluralist politics in which groups learned to bargain, compromise, and get along with one another. Even the switch to an advertising campaign style had the positive benefits of getting away from mass drilling of people to vote their party identification to a new era in which politicians attempted to educate the people on the issues. As Jensen says, the era "was not a backward-looking emphasis on party loyalty, but McKinley's new spirit of pluralism."[45] And in another vein, Jensen lends his highest compliment to McKinley when he says that his hero, Franklin Roosevelt, incorporated the McKinley pluralism in a later age:

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[42] While the two parties generally conveyed the opposite images prior to 1896, they were not above shifting positions on certain key issues, according to Kleppner, to gain temporary vote advantages from religious-ethnic groups which normally supported the other party.

[43] Jensen, *The Winning of the Midwest*, p. 296; also see pp. 291, 308, and chapter 10 in general.

[44] Jensen, in the next-to-last page in his book (p. 307), attempts to reconcile his interpretation of the 1896 election with Burnham's, but this limited effort is unconvincing. On the last page of his book (p. 308), Jensen summarizes his theory, vividly pointing out his positive feelings about the 1896 election and the subsequent period.

[45] *Ibid.*, p. 308.

> Franklin Roosevelt perfected McKinley's strategy of inclusive pluralism by giving practically every major economic, ethnic, and regional interest group in the country the recognition and legislation it wanted; in the process, the Democrats recaptured the support of the liturgical and metropolitan electorate that had formed the key to McKinley's coalition.[46]

Jensen's theory also has implications for both lower turnout and higher split-ticket voting after the 1896 election. McKinley's themes of prosperity and pluralism would be conducive to a Eulau-type "politics of happiness," which would manifest itself in a reduced need or motivation to participate in politics.[47] When people are content with their lives, politics seems even more peripheral to their life spaces. Higher split-ticket voting could result from less emphasis on militaristic campaign techniques and greater emphasis on advertising techniques. The older mode of party presses stirring up people's emotions and party loyalties would be supplanted by a different style in which candidates informed the people about the issues in much the same way as McKinley discussed the tariff in the 1896 election as a means to economic recovery and prosperity. With these themes in mind, Jensen is little persuaded by Burnham's idea of an inevitable confrontation between corporate interests and pluralist elements which led to the downfall of those pluralist elements and to the subsequent deterioration of the system. In Jensen's version, pluralist politics won —not lost—in the 1896 election, vanquishing the previous system which was anything but enlightened. Rather than declining, democracy had actually come of age.

Anomalies such as these tend to weaken the Burnham theory. But one key element of the theory has more than a slightly plausible ring to it—the effects of the depression of 1893.[48] This economic force could easily be important in shaping the realignment of the 1896 election. Our knowledge of the effects of the 1929 depression on another realigning era testifies to this point. The difficulty arises when Burnham maintains that capitalist elites took advantage of such a depression to steal the electoral system from the people and consolidate their rule. One can buy one factor of the theory—the effects of the 1893 depression—without buying the other—the capitalist conspiracy. Burnham, now somewhat sensitive to his earlier implications of conspiracy, downplays this notion in his present article—calling it more a matter of circumstance than conspiracy that capitalist and party elites were able to control the system. The outcome, however, is the same—a system which Burnham still calls "elitist," "capitalist-dominated," and "retrograde" in character. In order to maintain that the system was declining from the enlightened peaks of earlier decades, Burnham must hold that the capitalist elites obtained power, regardless of how they did it.

Unfortunately, we may never know exactly what happened in the election of 1896. Ever since Key's article on "critical elections," this election has been assumed to be vitally important for any theory of electoral politics.[49] Precisely what its significance is remains a matter of debate. Jensen, Kleppner, and Burnham all have different interpretations of this election. The problem comes in deciding if these three theories can be integrated in any way in order to build a better theory of the 1896 contest. The task would be difficult because anomalies abound, but some consistent elements may also be present. One element, however, which is not consistent is Burnham's assumption of the retrograde effects of the system of 1896, a topic to which we now turn.

The Deteriorating Electorate

Burnham's assumption of a deteriorating electorate is based on two things—the statistical changes noted in voting variables and his particular interpretation of the 1896 election. To these he adds, in the present paper, a vague reference to the cause or causes which generated Populism, the landslide victories of the early 1890s, and the eventual critical realignment of 1896. Obviously, several key inferences are being made. With respect to the statistical information, Burnham is implying that lower turnout and higher split-ticket voting are signs of a less involved, less issue-conscious, and more alienated electorate. This view reads with considerable latitude the statistical evidence at hand. Why would lower turnout necessarily convey less involvement and issue awareness among the public, especially since the higher previous turnout, in Burnham's own words, depended on a militaristic drilling of party loyalties and what can be considered to be a pseudo-type issue based on moralism? By implication, Jensen, in his theory, believes that the new advertising style of campaigning would create a greater involvement and issue awareness among the public. Such a shift in campaign style should also cause more split-ticket voting. As contemporary survey research shows, split-ticket voting is a reaction to such short-term forces as issue appeal, allowing a breaking away from slavish loyalty to one's party identification.

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[46] Jensen, *The Winning of the Midwest*, p. 308.

[47] Heinz Eulau, "The Politics of Happiness," *Antioch Review*, 16 (1956), 259–264.

[48] Burnham. *Critical Elections*, pp. 38–39.

[49] V. O. Key, "A Theory of Critical Elections," *Journal of Politics*, 17 (February, 1955), 3–18.

A more crucial belief of Burnham's is that lower turnout suggested an alienation of the electorate.[50] Alienation, however, implies a conscious withdrawal from the system because of intense dislike or disgust with it. A better theory would be that politics is peripheral to most people, and in the absence of intense machine efforts to mobilize the vote, the public would become more lax in their civic duties. Those who stayed in the electorate would then be assumed to be more susceptible to short-term forces such as issues than earlier electorates. If it seems anomalous that I can maintain that both lower turnout and greater issue awareness probably occurred at the same time, it will be shown below just how such a situation could, and probably did, happen once the crucial institutional reforms of ballot and registration were implemented. For the moment, however, all I wish to point out is that no hard evidence exists to link lower turnout to a conscious alienation from the system.

Burnham's inferences with regard to his conceptual arguments also merit close attention. His interpretation of the 1896 election as a capitalist capturing of the electoral system has already been discussed and found wanting. In no way does this interpretation constitute hard evidence for any notions of voter alienation. The vague reference to earlier causes before 1896[51] presents a perplexing situation—the very vagueness with which they are discussed precludes much intelligent debate about them. The only handle on them seems to be allusions to some of the same Industrial Revolution tensions which were supposed to have influenced the 1896 election. If one does not accept Burnham's theory of the 1896 election, then one presumably would not agree about the influence of these earlier causes. The question that naturally arises is why Burnham introduced these early causes in the first place. Two reasons come to mind. First, the initial changes in the voting indices came before 1896, weakening Burnham's original argument about the importance of this election. The causes of 1896, therefore, had to be pushed back in time in an attempt to explain the initial changes in these voting curves. Second, the other plausible explanation for these changes—the introduction of the new ballot and registration systems—also took place in the early 1890s. Pushing back the causes which led to 1896 might also conceivably be used to explain the rise of these institutional reforms.

Without these conceptual arguments, Burnham's claims of a deteriorating electorate rest only on the changing patterns of voting variables. The key question must be: Can factors other than the ones Burnham suggests explain these changing vote patterns, and if so, does this invalidate Burnham's notion of a retrograde age? I maintain that my legal-institutional theory goes to the heart of the matter, explaining many of the anomalies which have confronted us in this paper. The legal reforms of registration and the ballot, in particular, are important for explaining the changing vote patterns in the 1890–1920 period. After that, further legal variables are found to be important—most notably, the women's suffrage amendment.

The effects of these legal variables fall into two classes—intended and unintended. It is only when one considers both kinds of effects that a full understanding of how voting patterns changed is possible. For example, the intended effect of the new registration system was to end vote corruption, which it succeeded in doing. But its unintended effect was to put up an additional barrier to the vote, eliminating many honest votes on the part of marginally involved citizens. Turnout thus declined on two scores, as both the corrupt voters and the marginally involved were removed from the polls. The intended effect of ending the corrupt vote was accomplished in several ways. First, the new law required a frequent and supervised purging of the election rolls to validate them and keep them up to date. Second, it stipulated that each person wishing to vote must present himself before the registrar to justify his qualifications, providing a signature which could be used for verification on election day. Such requirements ended the practice of unqualified people voting themselves or for others who would not vote, were deceased, or had moved away. "Repeaters" became uncommon. The new system promoted professional bipartisan honesty on the election boards. At the same time, the law, in its attempt to end corruption by requiring the voter to take the initiative to prove his voting qualifications and become registered, also had the unintended consequence of pricing out of the system those who simply did not have the motivation to take such action. Before this time the nonpersonal registration laws had put the burden of registration on the election officials rather than the voters—the election officials having to take the initiative through canvassing, directory searches, or other means to establish who was qualified to vote.

With such facts in mind, the anomalies regarding turnout no longer seem so strange. A sizable portion of the enlightened electorate's turnout is reduced by the elimination of its most unenlightened element—the corrupt voter. Another group in this electorate—the deferential voters drilled by the party organization to participate—falls by the wayside, not having sufficient motivation to vote once an additional barrier has been placed in its path. These two elements comment on the Burnham theory in a most meaningful way—it is not the enlightened who are dropping out of the electorate because of alienation but rather those citizens who have always been characterized by their marginal involvement. Converse notes that the turnout drop in presidential elections in the North between 1896 and 1912 is slightly above 10 per cent.[52] This figure, it would seem, can be easily accounted for by the twin movements out of the electorate of the corrupt and peripheral elements. With these elements removed, the pre-1896 electorate no longer has any superiority in turnout to bolster the claim of an enlightened electorate and a golden age.

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[50] Burnham, "The Changing Shape," p. 27.

[51] Burnham, "Theory and Voting Research," p. 1016.

In actuality, the logic presented here would view the post-1896 electorate as more enlightened than the earlier Burnham electorate. Once the corrupt and marginal voters dropped out of the electorate as a result of the new registration barrier, those remaining would constitute the more issue-conscious core who had sufficient interest in politics to stay in the electorate despite the legal obstacles involved.[53] It is because of this that we can say that a decline in voting turnout does not necessarily mean a decline in issue consciousness, but could mean just the reverse. Those dropping out of the electorate were deadweight—voters who seemingly were motivated more by the militaristic drilling of parties than by the issues. If issues had been more important to them, they would have stayed in the electorate after the personal registration system was adopted. But these were people who were not much interested in politics and therefore needed the persuading of the party organization just to stay in the electorate in pre-registration reform days. Even the religious-ethnic variables of Jensen and Kleppner did not seem sufficiently important to keep many of these voters in the electorate once a deterrent had been placed in their way. Burnham's analogy likening the early American electorate to the highly mobilized European electorate—in which religious feelings were supposed to be the common cause of high turnout behavior—might better be replaced by an analogy which attributes the two electorates' high turnout behavior to possession of a common registration system—the nonpersonal registration system. The later American change to personal registration is a more likely explanation of our declines in turnout vis-à-vis the European electorate than is any disappearance of confessionalism from the political scene.

Assuming that registration is an important variable, we then move to the other reform in this period—the Australian secret ballot. Like registration, its intended effect was to stop vote fraud but its unintended effect was to permit and even encourage split-ticket voting (and roll-off) on the part of the electorate. The new law attempted to end corruption in several ways. It took the function of ballot preparation and distribution out of the hands of the parties and put it under the regulation of the state. No longer could party hawkers persuade, cajole, intimidate, and bribe voters to take their ballots as the voters walked to the polling place. Under the new system, the ballot could only be obtained from state election officials inside the polling station. The ballot law also provided for a secret vote, ridding the system of the bribery and intimidation of the earlier public voting arrangement. The ballots were, in addition, identified by numbers, stubs, initials of election judges, stamped endorsements, and the like in an attempt to stop ballot stealing and the "endless ballot chain" (or "Tasmanian dodge"). Finally, in an attempt to allow a free and equal choice between the parties, unhindered by party pressure, the states consolidated the ballot—placing the two major parties on the same ballot rather than continuing to allow the earlier system of separate party ballots. While this, too, was an aid to stopping party intimidation and vote fraud, it—combined with the secret vote feature—was to have the unintended consequence of changing vote choice patterns by permitting split-ticket voting. The voter, safe from party pressures, could view the candidates of both parties on the same ballot in the privacy of the polling booth, certainly enhancing the potential for split-ticket voting relative to the earlier party ballots system. The new ballot could also cause increases in roll-off, the tendency for the electorate to vote in lesser numbers for lower-placed offices on the ballot. Having to consider all offices on the new consolidated ballot and to vote on them was definitely a more tedious and difficult decision-making task than simply depositing the earlier system's party ballot in the ballot box, either marked or unmarked. No decision making was required in the latter situation, other than which party's ballot to pick up in the first place.

The impact the Australian ballot would have in stopping corruption should be quite apparent. Often, however, scholars do not seem to see how the ballot could affect the corrupt element of turnout; even Converse's treatment follows the standard pattern of attributing most of the decline in the corrupt element of turnout to the personal registration system. The fact remains that the personal registration system would have been much less effective in stopping corruption if the Australian ballot had not been enacted. As Wigmore aptly notes, "by compelling the dishonest man to mark his vote in secrecy, it renders it impossible for him to prove his dishonesty, and thus deprives him of the market for it."[54] Or, as he says in another place, " . . . take away all interest in committing an offence, and the offence will soon disappear."[55] In short, there was no incentive for the vote buyer to pay for votes if the secret ballot prevented him from knowing whether the goods were delivered. As a result, the corrupt vote would seriously decline unless some way could be found to circumvent the new ballot system. The only way attempted was the "Tasmanian dodge" or "endless chain" which, according to Harris, was relatively unsuccessful.[56] The practice consisted of stealing an official ballot, marking it, giving it to a bribed voter to vote, and using the unmarked ballot he received in the next round of the chain. In this way, the vote buyer could guarantee, even with a secret ballot, that his money produced the desired results. States, however, countered such activity by incorporating in their ballot laws various provisions to ascertain that the ballot given to the voter at the polls was, indeed, the one he marked. According to Albright, twenty-five states had the most effective provision—the numbered stub.[57] This stub, detached from the ballot before it was deposited in the ballot box, was checked against the number of the ballot recorded in the poll book by the voter's name. If the two agreed, the vote was allowed.

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[52] Converse, "Change in the American Electorate," p. 290. The actual turnout difference figure between 1896 and 1912 is unknown; various estimates place it in the 10–17 per cent range.

[53] Those marginal voters dropping out of the electorate because of the registration obstacle would have an easier system to face a few decades later when the personal system changed from a "periodic" to a "permanent" basis of registering voters in many states.

The Australian ballot not only reduced turnout (in conjunction with the personal registration system) but also changed vote choice patterns. In this latter context, ballot was not only a "contributory" or "permissive" variable, as Burnham claims, but also probably had some direct causal effects as well. Empirically, the evidence shows different adjustment patterns to the party-column and office-bloc versions of the Australian ballot, and even today Campbell and Miller find independent effects of ballot on voting behavior.[58] The ballot system, like registration, was introduced in the early 1890s, antedating Burnham's crucial 1896 election. From the first, it was correlated with rises in split-ticket voting, although more major effects came later in some states, particularly party-column ballot states. Considerable split-ticket voting, however, occurred before 1904—for instance, 1896 was a high split-ticket year.

Probably the effect of ballot on voting is best combined with a theory of short-term forces. The Australian system had loosened up party voting enough to allow short-term forces such as candidate and issue appeal to play a role in determining the vote. Such forces would often be translated into split-ticket votes much as they are today. The fact that large spurts of ticket-splitting did not occur continuously after the 1896 election calls into question the existence and widespread impact of any Burnham business conspiracy or anti-party forces in this period. The variable nature of split-ticket voting would better support a theory which states that a high or low split-ticket election is dictated by the salience and intensity of the short-term forces in that election year. Of course, the theme of ballot is central throughout—the new ballot had to be introduced in order for people to split their vote, and even a low split-ticket year would usually have a higher split-ticket value in the Australian ballot period than previously, simply because some people would be using the new ballot's split-ticket feature. When one looks back to the pre-Australian days, one would acknowledge that short-term forces existed then also (although in no degree comparable to Australian days because of the nature of the pre-Australian ballot and its corresponding partisan culture), but the crucial point is that it was extremely difficult to translate such forces into split-ticket votes using the party ballot system.

Two other movements probably contributed, by default, to the increased ticket-splitting in the new ballot period. The elimination from the electorate of two sets of straight-ticket voters would correspondingly cause split-ticket figures to rise. These two sets were obviously the corrupt voters who were paid by party hacks to vote straight tickets and the marginally involved who would only go to the polls in response to the militaristic drilling of the party, dutifully casting their straight-ticket votes. The voters who would remain in the electoral stream in the reform period would be more likely to consider short-term political forces in their vote decisions, instead of staying as enslaved to the party organization as they had in the past.

Basically, ballot and registration go a long way toward explaining the changing vote trends in the North for the 1890–1920 period. But what about the South? This region has always been an analyst's nightmare, and it is no wonder that Burnham's focus on changing vote patterns deals almost exclusively with the North. If legal-institutional variables are critical to understanding this period as Converse and I maintain, it would bolster our cause if we could show that such variables also explained the changes in southern voting behavior. The bizarre behavior of vote choice variables leaves little hope for illuminating this aspect of voting behavior, but the declining curve of southern voting participation is another matter entirely. The registration variable seems of little use here, since the South generally adopted personal registration systems much later than the North did, but the South enacted other laws governing the vote around the turn of the century which could be useful as explanatory variables—the well-known "Southern System" of poll tax and literacy test legislation. Such restrictive legislation could dampen voting turnout in the South much as personal registration did in the North. As Stucker and I show in an extensive empirical analysis of this period, the Southern System had major effects in curtailing voter participation.[59] Two data presentations will make this clearer. In Figure 1, one can see the widespread difference in turnout patterns between poll tax and non–poll tax states in the South and the similar declining patterns of the "poll tax state" and "all southern state" curves. The latter suggests the dampening effect which poll tax states had on the general southern turnout curve, pulling it down over the years. In contrast, the non–poll tax curve seems to have suffered no corresponding declines, but rather maintained a steady rate over time. Obviously, such states were not hampered by any legal effects which would discourage voting turnout. Table 1 gives average estimates of vote turnout behavior for various types of Southern System states in the 1892–1916 period. The message of the table is straightforward—both the poll tax and the literacy test laws had independent effects in decreasing voter participation, and, when combined, they had a strong cumulative effect, reducing turnout by 47.9 per cent from states not having such restrictive legislation. Although not shown here, another mode of analysis reveals that such legal variables explained much more of the vote decline in the South than did other factors, such as Key's notion of the development of the "one-party South" and the subsequent apathy it presumably produced.[60]

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[54] John H. Wigmore, *The Australian Ballot System as Embodied in the Legislation of Various Countries*, 1st ed. (Boston: Charles C. Soule, 1889), p. 32.

[55] *Ibid.*, p. 31.

[56] Harris, *Election Administration*, p. 73.

[57] Spencer A. Albright, *The American Ballot* (Washington, D.C.: American Council on Public Affairs, 1942), pp. 43–44.

[58] Rusk, "The Effect of the Australian Ballot Reform" (1970), and Angus Campbell and Warren E. Miller, "The Motivational Basis of Straight and Split Ticket Voting," *American Political Science Review*, 51 (June, 1957), 293–317.

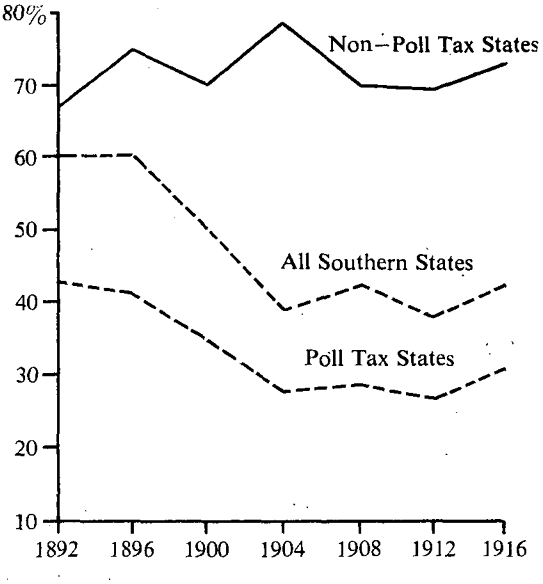

Figure 1. Presidential Turnout Curves for All Southern States, Poll Tax States, and Non–Poll Tax States, 1892–1916

Table 1. Relation of Poll Tax and Literacy Test Laws to Presidential Turnout in Southern States, 1892–1916[a]

	Absence of Literacy Test	Presence of Literacy Test	Marginals
Absence of Poll Tax	72.1 (38)	57.2 (2)	71.3 (40)
Presence of Poll Tax	40.2 (28)	24.2 (33)	31.5 (61)
Marginals	58.4 (66)	26.1 (35)	47.3 (101)

[a] Cell entries are presidential turnout means computed over all election years, 1892–1916, for states having the legal combinations listed in the table.

With the 1890–1920 period largely accounted for, in both the North and the South, by legal-institutional variables, the focus shifts to explaining the further changes in voting behavior in the 1920s—the continued decrease in turnout and rise in cross-party voting. Consonant with the Rusk-Converse theory, the variable which immediately comes to mind is women's suffrage. Opening of the system to a relatively politically unsocialized contingent could provide the further flux in electoral figures which Burnham and others

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[59] Jerrold G. Rusk and John J. Stucker, "The Effect of the Southern System of Election Laws on Voting Participation: A Reply to V. O. Key," a paper delivered at the Mathematical Social Science Board's Conference on the History of Popular Voting Behavior, Cornell University, June 11–13, 1973. A revised version of this paper will appear in a forthcoming volume on the historical dimension of popular voting behavior, edited by Joel H. Silbey, Allan G. Bogue, and William H. Flanigan and published by Princeton Press.

[60] V. O. Key, *Southern Politics* (New York: Alfred Knopf, 1949).

Table 2. Mean Difference Scores Between Two Elections Before and Two Elections After Adoption of Woman Suffrage, for Two Indices of Voter Turnout, Averaged Across States

	$\frac{(\bar{E}_{+1,+2})-(\bar{E}_{-1,-2})}{N}$
Presidential Turnout	−.097
Off-Year Congressional Turnout	−.104

have noticed. According to an empirical study by Stucker and myself, the evidence seems to support this notion, at least with regard to turnout.[61] Using a variant of the immediate before-after analysis technique, Stucker and I find, on the average, a 10 per cent drop in both presidential and off-year congressional turnout immediately after women's suffrage was adopted in the several states. (See Table 2.) The decline is undoubtedly due to the fact that women were unaccustomed to political participation and probably heedful of the long-standing social norm against it.

The other aspect of the argument would be that women, initially without a strong party loyalty or, perhaps, any party loyalty at all, would engage in considerable split-ticket voting. We have no empirical evidence on this score, but Burnham shows evidence from Chicago and suburban Cook County which supports a mild version of this contention. While his evidence of only a mild relationship between sex and voting volatility may be accurate for a larger universe than Cook County, it would be best to postpone judgment on the size of the effects until a more sweeping analysis has been made. Burnham also states that he would expect ticket-splitting to increase among women over time. On the contrary, given the tenets of political socialization theory, it would be hypothesized to decline—as women engage in more and more election trials and become more socialized into the political system, they should gain a greater psychological attachment to party which would manifest itself in reduced split-ticket voting in succeeding elections. As for turnout behavior over time, Burnham's assertion is probably correct: turnout among women would increase over several election trials. Such turnout might be modest at best, however, compared to new cohorts of women entering the electorate in later periods when the social norms were more favorable for political participation.

Perhaps the entry of women into the electorate could be viewed by Burnham as a deteriorating element since they engaged in politics in a less stable fashion than men. However, any new group entering the electorate—the immigrant, the black, the young, etc.—must be expected to show some volatility as in any learning or socialization process. That these groups eventually integrate themselves for the most part into the system is a plus for democracy. With regard to the wider scenario for the period, 1890–1930, little evidence of electoral deterioration is observed; instead the patterns of political behavior for this period are best understood in terms of the legislation enacted to govern them.

The Legal-Institutional Theory

Throughout this paper, I have proposed the legal-institutional theory as one perspective by which to view the political behavior of our electoral past. I initially formulated this theory in 1970, elaborated it in 1971, and Converse extensively elaborated it in 1972.[62] Basically, the theory postulates that legal-institutional properties of the electoral system—ballot and registration systems, voting systems (e.g., plurality, proportional representation), suffrage requirements, and the like—have important effects in influencing and shaping voting behavior. In essence, they define the conditions and boundaries of decision making at the polls. Often, though, they are taken for granted as "givens" instead of being probed for their effects on voting behavior. A better viewpoint would be to look upon institutional variables as providing the causal framework—the initial effects—within which the effects of other independent variables must be judged. By first ascertaining the effects of institutional variables, the researcher provides a reliable and much-needed baseline against which to measure the effects of other variables. Stanley Kelley and cohorts probably stated it best: it is a matter of "putting first things first."[63]

Thus far, we have seen empirical analysis conducted on the following legal variables: ballot (Rusk), poll tax and literacy tests (Rusk and Stucker), women's suffrage (Rusk and Stucker), and registration (Burnham). The first and third analyses involved the universe of all states; the second, the South; and, the fourth, four states. While the first three have completely covered their relevant universes, more work needs to be done in the fourth area, eventually extending the analysis to all states. From the evidence collected from these studies, the legal-institutional theory can be seen to have a strong empirical foundation—at least with regard to ballot, the Southern System, and women's suffrage. Indeed, it is the only theory mentioned in this paper which is established on such a strong empirical footing. The Jensen-Kleppner theory also has some empirical standing, but it is weakened by some of the assumptions it makes. The Burnham theory seems to have the least empirical foundation of all.

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[61] Jerrold G. Rusk and John J. Stucker, "The Effect of Legal-Institutional Factors on Voting Participation: An Historical Analysis," a paper delivered at the 1973 Convention of the Midwest Political Science Association.

[62] Rusk, "The Effect of the Australian Ballot Reform" (1970); Rusk, "Communications"; Converse, "Change in the American Electorate."

[63] Stanley Kelley, Jr., Richard E. Ayres, and William G. Bowen, "Registration and Voting: Putting First Things First," *American Political Science Review*, 61 (June, 1967), 359–379.

Confronted with this evidence, Burnham still does not view the legal-institutional theory as an alternative to his theory, but as a subunit within it. By and large, such an assumption is based on the notion of "legislative intent." To Burnham, the businessmen and Republicans who formed the capitalist conspiracy deliberately enacted such laws as ballot and registration as part of their plan to consolidate the stealing of the electoral system. At other times, Burnham states that they enacted the laws as part of a move toward antipartyism, presumably this being consistent with their plans to consolidate their power. Two initial points need to be made. First, the legal reforms generally antedated the 1896 "capture," forcing one to use Burnham's amended version of his theory which stipulates that the causes of 1896 had their roots in the political system since at least 1890. Second, if one does not accept Burnham's notion of a capitalist capture of the system, the question of legislative intent becomes immaterial. For the moment, we will set these objections aside, concentrating only on the issue of legislative intent.

The paramount question then becomes—*who* urged the passage of these laws and *why?* Earlier I stated that such laws were most likely passed to end vote corruption. Most historians would agree with this position. The forces most instrumental in pushing for these laws were probably reformers and good government people and not business-Republican elites intent on seizing control of the electoral system. Various histories of the Australian ballot law give this impression. According to Fredman, Henry George, the reformer, was as much the father of the Australian ballot in the United States as Dutton and Nicholson were in Australia.[64] Fredman also cites civic groups, Mugwumps (a reform wing of the Republican party), the Populist party, and trade unions as supporting the ballot movement. Jensen and Kleppner believe pietist elements lent strong support to the cause as well. What is most revealing is Fredman's claim that the voting on the ballot issue in state legislatures was relatively impartial. He states:

> If the party machines were prepared to support or to mangle the laws, they did so impartially. . . . Wigmore tabulated the legislative votes for 1888–1889. He found that the vote followed party lines in six states and ran across them in eighteen. If each party received one point for providing a majority for the bill, or for being the minority when it was defeated, the score was Democrats, fourteen and Republicans, ten.[65]

Such data fail to confirm the Burnham theory that the Republican party was the sole or main supporter of the ballot reform. If this party was the main beneficiary of such a law, the voting statistics do not seem to indicate much awareness of this fact.

Even if the Republicans were the main element behind these laws, which hardly seems the case, one must still explain how these laws could benefit them. To even a casual observer, the laws would seem to loosen rather than tighten control over the system—pushing people away from their partisan anchors to more of a split-ticket and floating voter status. This move to antipartyism (or whatever one wishes to call it) would seem to hurt the Republican as well as the Democratic party. If the urban workers were in the Republican coalition as a result of the 1893 depression, stringent registration laws would prevent many of the marginally involved among them from voting and the new ballot would tempt others to vote split-tickets in the presence of strong short-term forces. In general, one can envision little help that such laws would give the Republican party in maintaining or consolidating the power of any capitalist conspiracy.

If Burnham wished to find a conspiracy which used legal means to control the system, he should have looked at the Democratic party in the South instead of the Republican party in the North. According to Woodward, the Southern System of legislation was used by the Democrats deliberately to eliminate the Negro from the political system. According to Key, it could also have been intended by the Bourbon Democrats to rid the system of the poor white element.[66] Here was a conspiracy for Burnham but it had nothing to do with the exploitation of the Industrial Revolution or any capitalist capture of the electoral system. Its roots were in a different socioeconomic and cultural framework, one which would seem quite unfamiliar to the Burnham theory. It is indeed ironic that the intent factor which could not be found in Burnham's industrial North with its Republican dominancy was found in the agricultural South, a Democratic stronghold. Burnham was correct in stating that the laws had effects on voting behavior, but the effects Burnham was alluding to, except in the South, were unintended in every sense of the word. Few people anticipated they would occur, and no capitalist elite deliberately tried to promote their occurrence.

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[64] L. E. Fredman, *The Australian Ballot: The Story of an American Reform* (East Lansing, Michigan: Michigan State University Press, 1968), p. 32.

[65] *Ibid.*, p. 50. Also see Table 6 in John H. Wigmore, *The Australian Ballot System as Embodied in the Legislation of Various Countries*, 2nd ed. (Boston: Boston Book Co., 1889), p. 205.

[66] C. Vann Woodward, *The Strange Career of Jim Crow*, 2nd ed. (New York: Oxford University Press, 1966); Key, *Southern Politics*, especially Part 5.

The Modern Period

No discussion of the Burnham theory would be complete without tracing and critically analyzing its implications for the modern period—the 1930–1970 time span. Burnham basically continues to hold that throughout this period politics was retrograde and deteriorating. Much of this assertion, of course, is the consequence of Burnham's image of what the capitalists did to the political system in the aftermath of the 1896 election—putting politics on a deteriorating path which could not be reversed. But other aspects derive from Burnham's broader theory of politics: the continual tensions between the economic and political systems throughout history make their own contribution to the deterioration of the electorate. All realignments are the product of such tensions: the economic system, being out of step with the political system, causes severe dislocations and injuries which can only be solved, albeit temporarily, by party realignments. Such tensions did not stop with the 1896 realignment, but have continued to the present time. They forged the realignment of the 1930s, and the cyclical nature of these economic-political tensions calls for a new realignment in the present period. If such a realignment is not occurring now, it is only because, to Burnham, the electorate has deteriorated so thoroughly since 1896 that no viable bond remains between the voter and his party system. If this deterioration has reached the point of no return, we will be faced not with a party realignment but instead with an era characterized by the end of parties. We will examine each of these aspects of the modern period in turn—the nature of the 1930s realignment, the possibility of a new realignment, and the prediction of an end of parties.

The 1930s realignment marked the beginning of the modern period of politics. Undoubtedly, Burnham is correct in saying that it was, at least partially, the product of economic tensions and forces—this realignment was heavily affected by the 1929 depression just as the 1896 realignment was influenced by the 1893 depression. But, according to Burnham, this realignment did not have the bad consequences that the 1896 realignment did—instead it temporarily revitalized the electorate as the capitalist elites were replaced by what Burnham calls the "pluralistic, welfare-oriented elites and veto groups." This revitalization of the voters, to Burnham, is evidenced in the higher turnout and lower ticket-splitting r ates o the 1930s—just the opposite of the patterns characterizing the earlier 1896 realignment. The irony of this is that Burnham contends that both realignments were the result of common explanations—economic tensions and forces culminating in depressions—yet one leads to good consequences and the other to bad consequences, one leads to one type of voting pattern and the other the opposite pattern. In my view, the 1930s realignment was behaving more as one would expect a realignment to behave—economic forces causing increased interest among the voters, especially among the marginally involved who came off the sidelines to vote in large numbers against the party of depression, the Republican party. The 1896 realignment either basically had just the opposite effect of not generating much interest, a point I find difficult to believe, or else the newly enacted institutional factors placed an effective damper on much of the interest that it did create.[67] The use of the legal-institutional theory could provide much of the answer to why the 1896 realignment was atypical. It would find unnecessary Burnham's assumption of a capitalist takeover of the system in 1896 with its attendant bad consequences.

The revitalization of the electorate was, however, only temporary in Burnham's view. The 1930s realignment could not satisfactorily resolve the economic tensions of this period any more than earlier realignments were able to do for their periods. Economic tensions and dislocations continued, bringing us either to a new realignment crisis in the present period or else to an end of parties. Such predictions place Burnham at odds with the current paradigm of the American voter laid out by the Michigan team of researchers.[68] In a key footnote, footnote 4, Burnham explicitly indicates his skepticism of the Michigan work, calling for a retesting or replication of *The American Voter*.

Burnham's evidence for either a new realignment or the end of parties rests on the same empirical base—the increasing number of Independent identifiers, split-ticket voters, and partisan swing voters in the current period. The most crucial datum for the end of parties argument would seem to be the increase in Independents or, to reverse the terms, the decrease in party identifiers.

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[67] The 1896 election actually showed a small increase in turnout over the previous presidential election, 1892, testifying that some citizens were sufficiently motivated to overcome the legal obstacles to turnout. However, the legal obstacles discouraged any major increase in turnout, and succeeding elections continued to show this effect.

[68] Campbell, Converse, Miller, and Stokes, *The American Voter.*

But this decrease has been mild at best; still 70 to 85 per cent of the electorate identifies with a party, figures which probably rival those of Burnham's enlightened electorate.[69] While defections in voting may be increasing because of the greater import of problems in the late 1960s compared to the complacent 1950s, party identification has only narrowly declined at the same time. As Converse notes, " . . . even with the loosened levels of partisanship in 1968, party allegiance remained far and away the strongest determinant of most political cognitions and behavioral choices in the electorate. It could wane many times as much again without losing its dominant role."[70] Or, as Silbey and McSeveney comment, " . . . partisan identification remains a powerful political influence, particularly in elections other than those for the presidency. Certainly the success of the Democrats in the congressional elections of 1968 and the midterm elections of 1970 suggests that neither party identification as a determinant of popular voting behavior nor the continued strength of the Democratic coalition should be discounted as the nation moves into the 1970s."[71] Thus, a key Michigan concept—party identification—which Burnham so enthusiastically used to describe the enlightened electorate, still shows its dominance in the present period, making questionable any argument concerning an end of parties.

The possibility of a new realignment presents a more complex question. To have a realignment, important issues and forces must be present in the political environment to change party loyalties. But as Weisberg and I point out, there was very little concern in either 1968 or 1970 with economic issues—the key variables in Burnham's theory.[72] Also, Converse, Clausen, and Miller are unable to find any major economic forces in the 1964 election, the election Burnham believes started this new realignment period.[73] Other issues, however, were important in the late 1960s. As Converse, Miller, Wolfe, and I point out, the 1968 election showed considerable public concern over Vietnam, civil rights, and law and order.[74] But even here the vote for Humphrey and Nixon was based much more on party identification than on the issues. In a more extended analysis, Weisberg and I probed the public's perceptions of all possible presidential hopefuls, instead of just the nominees alone, and found that such perceptions were structured along issue as well as partisan lines.[75] The issue dimension that was identified (the "new issue factor") was relatively independent of the partisan factor, giving credence to the belief that this variable could cause a realignment. But longitudinal analysis of such perceptions, using the 1970 election data as a contrast to the 1968 data, revealed that the two dimensions shaping candidate perceptions—party and issues—were no longer independent of one another, but rather had come close to a convergence in the later election.[76] Subsequent results from the 1972 election study showed that this relative convergence had almost been completed.[77] The "new issue factor"—a forerunner of Scammon and Wattenberg's more publicized "social issue"—consisted of the elements which basically had bothered the 1968 electorate—Vietnam, civil rights, the urban plight, and law and order. Its effects, while it was a relatively independent factor in 1968, were to cause increased volatility in the electorate. The absorption by the parties of these issues in 1970—often by taking different stands on them—produced further volatility and possibly some realignment as the convergence became completed. But the primary effect of such an issue factor was to promote volatility rather than realignment. The new issue factor was basically defused by the 1970–1972 period.

Thus, while issues were more important in the 1960s than previously, they probably should be viewed as intense short-term forces causing voter volatility—increased ticket-splitting and partisan swing—instead of as crucial factors shaping a major new realignment. Burnham agrees that Vietnam and the urban plight were important issues, but the differences between us reside in that I neither perceive such issues as economic in nature nor do I regard them (or others) as successfully effecting a realignment. While there may be increasing vote defections which tend to paint the Democratic party as the standard bearer of the technologically competent and superfluous strata and the Republican party as the keeper of white Middle America, the vote defections have not led to basic changes in party loyalties. The relative increase in Independents may have some bearing on this as a "half-way house" theory would imply, but until actual conversions take place, there is little evidence for inferring that a major new realignment—economically or socially motivated—is currently happening. And until declines in party identification occur at a much faster rate than is presently the case, and the reasons for them are known, an end of parties argument could not be viewed as a plausible substitute for the empirically-weak new realignment thesis.

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[69] The exact figures depend on how one defines a "party identifier." The Michigan question taps a particular category of citizen—the "party leaner"—which some analysts classify as an "Independent" and others, as a "party identifier."

[70] Converse, "Change in the American Electorate," p. 322.

[71] Joel H. Silbey and Samuel T. McSeveney, eds., *Voters, Parties, and Elections* (Lexington: Xerox College Publishing, 1972), p. 306.

[72] Jerrold G. Rusk and Herbert F. Weisberg, "Perceptions of Presidential Candidates: Implications for Electoral Change," *Midwest Journal of Political Science*, 16 (August, 1972), 388–411, especially 399.

[73] Philip E. Converse, Aage R. Clausen, and Warren E. Miller, "Electoral Myth and Reality: The 1964 Election," *American Political Science Review*, 59 (June, 1965), 321–336.

[74] Philip E. Converse, Warren E. Miller, Jerrold G. Rusk, and Arthur C. Wolfe, "Continuity and Change in American Politics: Parties and Issues in the 1968 Election," *American Political Science Review*, 63 (December, 1969), 1083–1105.

[75] Herbert F. Weisberg and Jerrold G. Rusk, "Dimensions of Candidate Evaluation," *American Political Science Review*, 64 (December, 1970), 1167–1185.

[76] Rusk and Weisberg, "Perceptions of Presidential Candidates."

[77] Personal communication with Professor Arthur H. Miller, Study Director of the 1972 Michigan Center for Political Studies National Election Study.

In a yet broader perspective, perhaps Burnham's theory of realignment needs to be substantially revised. According to Burnham, our rudimentary party structures have never been able to adjust to or to control an autonomous economic system. Realignments result because they have been "the chief means through which an underdeveloped political system can be recurrently brought once again into some balanced relationship with the changing socioeconomic system."[78] But as Boyd notes, if the inability of our rudimentary party system to control the accelerating pace of the economic system with its myriad dislocations is the cause of realignments, why have realignments also occurred in countries such as England whose party governments exercise a tighter control over economic changes than do American governments?[79] Boyd finds McClure's explanation for realignments more satisfying—McClure emphasizing the difficulty of any majority party (rudimentary or not) to respond quickly to changing issues, even issues of major import and consequence.[80]

Summary and Conclusions

In this paper, the Burnham theory has been investigated using both logical and empirical modes of analysis. Through such analyses, I have drawn the following conclusions. Basically, I do not believe that Burnham's enlightened electorate was all that enlightened, that the 1896 election resulted in a capitalist take-over of the system, that the consequences of the 1896 election for the subsequent period were retrograde and deteriorating for the electorate, or that further manifestations of this deterioration in the present day have resulted in either a major new realignment or an end of parties. I do feel that Burnham's assertion of economic forces influencing political behavior is valid, in the limited sense that the depressions of 1893 and 1929 helped to shape the 1896 and 1930 realignments, respectively, and that economic forces must always be considered as one variable in any multivariate analysis aimed at unlocking the mysteries of our electoral past. I also agree with the Jensen-Kleppner component of the Burnham theory that religion is another variable that should be added to this multivariate equation, but that it probably was not the major factor causing intense polarizations and strong partisan loyalties that Burnham believed it to be. Rather, religion seemed to be a local factor of some limited importance, coming to the fore only when major campaign issues pointed to the salience of religious feelings and paved the way for their easy translation into political terms. Essentially, the portrait of the electorate that does emerge from this paper is one which is a product of institutional and party forces in the pre-1890 period (especially in the 1865–1890 period) and a consequence of the interaction of institutional, party, and short-term forces in the post-1890 period.

The pre-1890 electorate lived in a highly partisan and corrupt atmosphere. The parties in this period attempted to control the vote by implementing registration and ballot systems which were conducive to this goal. They militaristically drilled voters to the polls, relying on their pressures on the voters, coupling this with the buying of votes and the padding of election rolls to assure the desired outcome. Obviously the most important single cue the electorate received for voting was a partisan cue. Whatever short-term forces existed favoring a split-ticket vote would be difficult to implement in a system with such a strong party aura and on a ballot which did not easily allow a split-ticket vote to be cast. The extent of such short-term forces in this heavily partisan era will never be known since they could rarely be translated into split-ticket votes, but the lack of a mass media system to communicate such forces on a wide scale probably comments on their relatively modest occurrence in this period. The absence of significant educational training among a public characterized by a pervasive sense of party identification also means that there would be little involvement with the political issues of the day.[81]

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[78] Burnham, *Critical Elections*, p. 181.

[79] Boyd, "A Review of Walter Dean Burnham."

[80] Robert McClure, "The Flow of American Politics and Party Systems: Cycles of Stability and Change," a paper delivered at the 1970 Convention of the Midwest Political Science Association.

[81] The political issues of the time (1865–1890) were, in any event, not "life-and-death" issues, but rather ones of less pith and moment. Parties were often drilling the voters to the polls with loyalist and patriotic appeals, such as the Republicans "waving the bloody red shirt" as a constant reminder that they were the party which preserved the Union. Even later national issues—such as the tariff—would only take on an importance, however limited, among the

The institutional reforms of the 1890s changed the nature of the electoral universe. Strong registration systems and the new secret ballot combined to eliminate the corrupt vote. Such registration systems by themselves eliminated much of the marginally involved vote which the parties had been able to drill to the polls in the earlier period. With the loss of these two elements, the electoral universe also lost a considerable portion of its earlier straight-ticket voting component. The new ballot offered to those remaining in the electorate the first clearcut opportunity they had ever experienced to vote a split-ticket. The troubled times of the 1890s would provide sufficient short-term forces to guarantee that the ballot would be used in this way. In addition, frustrated voters of the earlier period who had wanted to vote split-tickets could now do so. The Australian ballot had eliminated the aura of the "party polling place," freeing the voter from party, employer, or other pressure in the casting of his vote. In essence, the public clamor for reform ended the earlier party practice of using the institutional framework for its own benefit. Instead of a party conspiracy to pass the new reforms, it was the reformers who initiated such action to end the earlier party conspiracy of party ballots and weak registration systems.

The electorate subsequently produced by these reforms would seem more enlightened than deteriorating. The most questionable elements of the earlier electorate had been eliminated, leaving an electorate which was more educated, more informed about the issues, and less likely to always cast a straight-ticket vote. This was an electorate which was sufficiently motivated to overcome the legal obstacles to voting and which, with the new ballot system, was no longer a slave to party but free to react to short-term forces as it wished. With the advent of the advertising style of campaigning, rising levels of education, and the development of the mass media, short-term forces would become more and more a prevalent factor of the electoral scene. The 1896 election was not an isolated case with its strong short-term forces of issues—the depression and the tariff—and candidates—the attraction of McKinley and the fear of Bryan. Other important issues and candidates would rise to the fore in the next seventy years, prompting voters to defect from their party identification as a result of these short-term forces. In actuality, the institutional requirements of the 1890s allowed a new theory of voting behavior to come into play—the Michigan theory of party identification and short-term forces. The ballot had permitted short-term forces to enter as a major new variable, yet one which was an adjunct to party identification rather than a replacement for it; it provided the opportunity for people to defect in their voting for certain candidates, not being a slave to party as they had been in the past but still retaining that important psychological attachment to it which would affect much of their behavior. The interaction of these two forces—party identification and short-term forces—still characterizes the electorate today, testifying that we are neither in the midst of a new realignment or an end of parties. The nature of party identification has undoubtedly changed somewhat as a result of the institutional reforms of the 1890s, possibly manifesting itself in more people claiming weak rather than strong loyalties. But the reforms in no way destroyed its meaning to people, only their exclusive reliance on it. With 70 to 85 per cent of the electorate claiming such an identification, one wonders if we will ever see an end of parties.

In the final analysis, the Burnham theory should probably be turned around—Burnham's enlightened electorate should be a deteriorating electorate and his deteriorating electorate more an enlightened one. If we accept this reversal, then the conclusions emerging from it are optimistic—that politics advanced after the 1896 election to a period of better times, that pluralist democracy was not defeated in 1896 but, as Jensen notes, received a new impetus for the modern period. The basis for such conclusions is largely the fact that undramatic variables—institutional factors—were the causal variables of note in this early period, rather than Burnham's more dramatic ones. Burnham has contributed immensely to political science with his unique perspective, promoting important discussion and research on our electoral past. But, all things considered, I believe my perspective offers a closer match to reality than Burnham's—one which explains more consistently, with the data at hand, the past and provides a better continuity to what we know about the present. It is this theory I offer to V. O. Key as the perspective by which to understand the nature of our historical past and its linkage to our present system of electoral politics.

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public when the troubled times of the 1890s had arrived. As for the pervasive sense of party identification, it did not seem to be forged out of whatever religious conflict existed in the late 1800s, but instead was most likely a product of the sectional-economic realignment of the 1850s. People after the War would often be voting the same loyalties that had existed prior to the War, the behavior becoming so routinized that frequently the original motive for such loyalties was later forgotten or considered of little import.

Rejoinder to "Comments" by Philip Converse and Jerrold Rusk

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Professors Converse and Rusk have replied to my present paper in two different ways: the former by a short, sharp rejoinder and the latter by a longer and more diffuse article. For space reasons alone, my commentary on their remarks must be concise; and while there is much which I should like to discuss further, there comes a time when the case must be sent to the jury.

Professor Converse's Note

It is best, I think, to confine most of my comments on Philip Converse's Note to the last half of it. We begin with *The American Voter* and the SRC "paradigm" as I see them. Having also reread this book, I will go even further in my praise of it than Converse has. It is not only a meticulous piece of scholarship; it is also truly a classic, a seminal work which justly gives permanent professional honor to those who wrote it. A seminal work is one which not only contributes to knowledge but permanently reorganizes the shape of a scholarly field. This can be done only by providing a conceptual framework for the myriad nonseminal but essential scholarly activities which take place in that field thereafter. As a meticulous piece of work, *The American Voter* was indeed qualified and judicious in its statements and generalizations. It also had at least as much sensitivity to the time dimension as could be expected for 1960. In case the point has been lost to view in the clash of arms, I will simply reiterate the obvious: every one of us in this field owes a permanent intellectual debt to *The American Voter* and its authors, and it is only common decency cheerfully to acknowledge that debt.

Associated with Thomas R. Kuhn's analysis of "scientific revolutions" are a few elementary observations concerning the sociology of organized scientific research communities. First, the members of such communities tend to have their own interests and value positions, which in the case of the social sciences usually involve ideologies either explicitly or implicitly. Such communities tend not only to organize subjects into research projects, but to organize them out as well. Second, there is the law of the twenty-four-hour day. Graduate students and young professionals who are involved in learning the methodologies of something as complex as survey research are unlikely to have been educated as thoroughly in historical context, nonstatistical contingency in political affairs, and issues of political and social theory. Third, in this particular case it remains a fair statement, I believe, that the conceptual core of *The American Voter* itself is derived from social psychology rather than from, say, concepts to be found in political sociology. Such focus naturally strengthens the work's powerful organizing character, but at the same time necessarily circumscribes it.

In the context of the organized research community's own interaction patterns, one must remember that the seminal work is not all there is to the story. Assuming that *The American Voter* is all that Converse says it is, this study has also developed a very rich gloss over the past dozen years or so. The development of gloss is a process very similar to that we find in religions, both theistic and secular. It is no news that Christianity has developed in ways which could not have been predicted from analysis of the Gospels, or that Marxism is a phenomenon which has passed far beyond its founder's vision. So too here: the paradigm discussed here starts from *The American Voter* but incorporates the gloss as well.

Of all the enormous corpus of work done within the paradigmatic boundaries set up by the authors of *The American Voter*, it is singularly hard to point to those which have—at least until extremely recently—displayed awareness that there was a relevant time (and therefore contingency) element to the subjects they were investigating. I think it a fair statement that this voluminous gloss has left a vivid impression among students of the field which is quite close to my own description in this article. If I discussed *The American Voter* in a way which falsifies the intent and text of the authors, I recant herewith. But—as is usual in such cases—*The American Voter* is not only a book; as the *Urtext* of a thriving scientific community, it is also a symbol. Any such references to it as I may have made here should be understood accordingly.

I will conclude this discussion by asking Converse two questions. First, does a defense of the text of *The American Voter* really rebut the proposition that, as a whole, this scientific community has worked within the intellectual perspectives discussed in my paper? Second, I appreciate Converse's kind references to Gerald Pomper and myself. But if so, why should we and possibly others make so crass an error of interpretation? Are you sure that the man we have attacked—the *whole* man—is really composed entirely of straw?

The original 1965 article, with all of its deficiencies, developed from an informed hunch that scholarship had not come to terms with important and disturbing aspects of American political reality, past and present. It has, at last count, stimulated at least the production of a doctoral dissertation, an article and exchanges in the *Review*, and the first half of Converse's "Change in the American Electorate." Naturally, this is gratifying; but the author could perhaps be pardoned for reacting as any discoverer of anomaly could be expected to react when subjected to so systematic a barrage from such eminently reputable sources. The original questions which informed the inquiry were simple ones: Has it always been so? Must it be so always? I submit, with all respect, that in the 1965 state of the profession, the asking of such questions was both appropriate and necessary. The obvious response to attack, this pointing out of alternative "possibilities with some vigor," to use Converse's phrase, was to re-establish the credibility of the argument by showing that none of these alternative explanations—nor indeed all of them taken together—can come close to explaining all the change which occurred. That is what I have tried to do.

I leave it to our readers to judge for themselves whether or not Converse's hypothesis of general but marginal rural corruption in the nineteenth century holds water. Similarly, they may judge for themselves whether his references to contemporary examples of high rural participation in politically underdeveloped countries have any probable relevance to rural society and politics in the non-Southern states during the last century. Naturally the evidence must be circumstantial at the individual level. But if we have evidence which so widely represents inversions of the contemporary pattern of *aggregate* data, is it reasonable to assume that the *individual-level* patterns have been largely the same across the past century?

The circumstantial evidence suggests very strongly, to my view, that elections meant something very different to 19th-century Americans than they mean to their descendants today; that parties also meant something very different. While we cannot know everything about that past, we can and will get much closer to it than we are now. It would not surprise me at all if we finally learned that Converse was broadly right for most voters at most times in the nineteenth century ("The Nature of Belief Systems in Mass Publics"), but also that the linkage structures then were fundamentally different from what they are now—and different in ways which are of the utmost importance in mapping the evolution of this political system.

It is of course gratifying for Converse to inform us that work is going on which clearly reveals significant changes in the post-1960 survey profile of American voters. Perhaps it is a little ungracious of me to point out that the country has been in an overt, steadily worsening political crisis ever since the assassination of John F. Kennedy and the beginning of the Vietnam war. If a crisis of such magnitude did *not* produce significant changes in survey profiles of voters and their relationships to issues, candidates and parties, that would truly be a wonder. At the same time, there is strong reason to suppose that these reactions, in the aggregate, have produced the analytical opposite to the classic nineteenth-century critical-realignment sequence. One major reason for this, I suspect, is that both the public and the situational contexts which surround it, from imperial foreign policy to media penetrations, have profoundly changed since the time of the Lincoln-Douglas debates or in the great Free Silver controversy.

There are deep problems involving the use of the time dimension. A study may very well be explicitly "longitudinal" without contributing very much to our knowledge of historical process or the evolving political sociology of a nation; and a great many of such studies fall in that category. Much more is required for effective work in this field than time-series analysis—so much more that one may doubt that any one man can hope to master all important parts of his task with adequacy. Still, teams of scholars with complementary skills and experiences may be able to unravel these difficulties. It is precisely here, indeed, that contemporary survey-research hypotheses and inferences must play a crucial role in the exploration of "past public opinion." Cooperation rather than antagonism between scholars of differing but complementary backgrounds is clearly essential to the scholarly enterprise in this field. The bridges must be built. But they are not to be constructed by explaining away anomalies which are inconvenient to the perpetuation of intellectual constructs. Instead, they can only be dealt with successfully by incorporating such anomalies into a more complete explanatory theory of electoral politics than anyone now possesses. It may be said that the dialectics of this kind of controversy distract from cumulativeness of knowledge. I cannot agree; for any supposed cumulative theory-construction which fails to find adequate explanation for these anomalies involves, explicitly or implicitly, premature certitudes. Whatever infelicities of expression or harshness of comment about the dominant perspectives of an organized research community may have appeared here, that is just what this symposium is all about.

Professor Rusk's Paper

Commentary on this elaborate critique will be confined to a very few points which seem of particular substantive importance. I think it true, as Professor Rusk argues, that the whole setting of the controversy before us requires a reference back to the original 1965 article in this *Review*. There I made a first approximation at identifying and explicating a major reality of American political history which had not only somehow escaped most historians, but which seemed to cast very peculiar light upon the American electorate. At its simplest level, the nineteenth-century American electorate and its behavior patterns make altogether too little sense when viewed through the lenses which the Survey Research Center has provided us. For instance, we usually find that the inverse of Angus Campbell's "surge and decline" model explains short-term landslides in the last century: they were produced by one-sided abstentions of one party's supporters, not by heavy ingestions of marginal and weakly socialized elements in the electorate. There is also genuine shock, a shock which I myself felt, when it is learned that mobilization and other indices of party activity increase backwards across time, as the level of formal education declines.[1] One could go on, but the examples simply abound even to the casual observer.

Even more striking, and to me much more so now than in 1965, is the extent to which the demobilization then described stands in stark contrast to longitudinal developments at the same chronological time in the history of European electoral politics. It was only natural to assume that profound system-wide influences were at work, influences which in their scope and thrust were virtually or wholly unique, among the cosmos of Western nations, to the United States. The description of these interlocked system changes, and of the universe of politics was alone enough to be a stumbling block to those—far more numerous in 1965 than they are today—who would apply the findings of contemporary survey research to all times and places. The explanatory scheme which was developed to deal with these phenomena was even more of a stumbling block, for it argued that the United States evolved after 1900 in such a way that political stability and elite insulation were bought at the price of partially liquidating political democracy.

Professor Rusk calls me to task for a failure to "operationalize" that explanatory scheme, and for modifying some of my hypotheses concerning the variables at work, particularly in the 1890's. I plead guilty to such charges—or rather, might strive for a *nolle prosequi* in the court of academic opinion: for no one knows better than the author of "The Changing Shape" how extremely complex the phenomena he sought to explain are in fact. Nevertheless, it is surely not out of place, contemplating such profound changes, to move from analytic decomposition of phenomena toward their synthetic integration. Such integration is very likely to go beyond data. Granted the current state of integrative efforts in political science, I can only add that happy is he who has as much solid data at the base of his inferences as are available to us in this subfield of study.

At the same time, the conflict which Professor Rusk purports to see between the original, economic-conflict hypothesis of the 1965 piece and the ethnocultural-conflict argument of 1970 and 1973 is one which I fail to perceive. Surely no one can doubt that causal explanations of complex change sequences in a political system must themselves, when fully elaborated, have a certain complexity. Let us review the matter this way. If there is any one thing which the Survey Research Center's work has taught us thoroughly, it is that voters mediate the world of political objects in terms of their immediate life experiences and group memberships. Such work has been invaluable in casting appropriate doubt on the tendency of earlier political historians to assume a public which is in the main *constantly* preoccupied with the issues of the day, which *constantly* pays attention to abstract doctrines and arguments which are quite remote from everyday life. Who today would seriously suppose a high level of mass voter consciousness about the intricacies of the tariff arguments made by political elites in the 1880's?

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[1] For a variety of reasons, one should beware of making too much of this decline. Certainly from a comparative point of view, the United States a century ago was conspicuous for its general literacy rate, and also for the extent and depth of interest among all classes of the (white) population in written communications. See Edgar W. Martin, *The Standard of Living in 1860* (Chicago: University of Chicago Press, 1942), pp. 295–342, 393–404. Moreover, one should take Professor Rusk's comment about a 20 per cent illiteracy rate in 1870 with a grain of salt. In the ex-Confederate states, the rate of those males 21 and over who could not write to the whole age cohort was indeed 46.5 per cent. But then we have two intervening legal-structural variables which explain most of this: the institution of black slavery and—subordinate to this—the prohibition which existed in many Southern states against teaching slaves to read or write. For the whole of the Free States, the proportion of adult males not able to write was 7.1 per cent, falling as low as 3.7 per cent in New Hampshire and 3.9 per cent in Maine. Literacy was extremely widely spread in the nineteenth-century white electorate, and seems to have been actively pursued, and noted by European visitors to these shores. *For the time*, this social indicator reveals, as do many others, an extremely high level of socioeconomic development.

So let us assume that, at the grass roots, acute group conflicts existed in the nineteenth century. Some—not all—of these were directly linked to the construction of major-party coalitions, compiled upwards from the congeries of local group conflicts toward a national summit. The evidence is very strong that many of the most politically salient of these conflicts turned upon subcultural antagonisms among well-defined categoric groups and their members, and that explicitly religious (or religio-political) perspectives formed the cognitive frame of reference for expressing these antagonisms. If we assume this, then it is hardly surprising that so little of the ethnocultural conflict at state and local levels should be manifested in national party platforms. Indeed, it may well be that issues such as the tariff were not only chosen by elites because they were interested in the outcome, but because they formed what we call today a safe set of "codewords." Being in favor of a protective tariff meant being, broadly, in favor of positive intervention by public authority across a wide range of "unsafe" issues which could be explicated more fully at the local level—for instance, in the conflicts between pietists and liturgicals over enactment of sumptuary, "legislating-morality" enactments. Finally, to the extent that tariffs were perceived at all at the mass level, it would be a safe bet that they were perceived as coalition-integrating "code words" covering many issues which were better left without discussion by national party leaders. So Professor Rusk's point that these local conflicts were curiously left out of national-level platforms becomes quite other than the curiosity which he thinks it is.

At the same time, acceptance of the proposition that such conflicts fueled much of nineteenth-century American electoral politics and mass voting behavior does not in any way deny that other issues grossly affected voters, especially during critical-realignment sequences.[2] Particularly in the crisis of 1854–60, it seems nearly certain that there was a notable *convergence* between local ethnocultural conflict issues and the national issues of slavery expansion and sectionalism: the one seems not merely to reinforce, but partially to explain the significance of the other. In the 1890's, the situation was probably more complex. But the basic point would surely be that *both* the shattering force of economic blows *and* changing ethnocultural party images were of immense significance in freeing a critically large minority of American voters for a permanent realignment in their voting behavior.

What might tie these strands together? First, the basic coalitional patterns of the 1894–96 realignment grew out of an economic conflict which was arrayed in geographical terms, i.e., between "colonial" and "metropole" regions of the vast American republic. Second, one among many reasons for Republican success nationwide, and in the vital "metropole" region, was the success of the McKinley-Hanna strategy of *secularizing* the Republican party, making it cosmopolitan and detaching it from identification with semi-agrarian religious perspectives which had become *passé* by the mid-1890's. The larger the "emerging Republican majority" became in North and West, the more attractive and successful this secularization would prove in augmenting it still further. Third, the Democratic party's base—ethnocultural as well as regional—significantly narrowed and, forced back upon a "colonial" clientele, became under Bryan's leadership markedly more Fundamentalist and ex-Confederate than it had been.

One of the most consistent charges leveled against that party by Republicans for the next forty years was its alleged lack of competence to govern; and this incompetence was linked by Herbert Croly[3] with Democratic incapacity to come to terms with the imperatives of high industrial capitalism. The point has merely been restated, with considerable supporting evidence, in David Burner's recent book, *The Politics of Provincialism* (New York: Knopf, 1967). Very broadly, some such composite explanation must come fairly close to the mark; it is very hard to see how, or in what sense, one of its aspects contradicts the other. And suppose it happens that there are a substantial number of people who live in industrial settings, or who are becoming linked to the elaborate division-of-labor world of industrialization. Suppose further that such people are not particularly involved in conflicts between "colonial" and "metropole" regions, or between two broad sets of religious perspectives which no longer relate to their life experiences. In such a case, they might well have two basic alternatives: to vote Socialist if a Socialist party exists, or to abstain. Both were manifest in the 1908–1914 period. By the time "normalcy" was achieved, abstention became paramount. The "party of nonvoters" was larger than all active voters put together in every election from 1918 through 1926;

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[2] The reader is referred to a review of several recent quantitative-history studies by me, "Quantitative History: Beyond the Correlation Coefficient," *Historical Methods Newsletter*, Vol. 4, pp. 62–66 (1971), for a more detailed discussion.

[3] *The Promise of American Life* (New York: Macmillan, 1909).

and not since 1910 has a majority of the potential electorate cast ballots in off-year congressional and state elections.

All of this is by now more than a twice-told tale. But it seems to relate directly to another of Professor Rusk's arguments: the contention that the legal-structural change theory he and Professor Converse have elaborated explains most of what there is to explain. If there is anything which my article here attempts to demonstrate as conclusively as possible, it is that such a theory does *not* explain most of what there is to explain. This is so, indeed, even by a very narrow construction of the argument—one which simply looks at the data with the intervening change variables taken as themselves not requiring explanation. Nor can one escape from this conclusion by referring to the "limited" nature of the data analysis. Of course, we all await the multivariate analysis based upon total universe recovery to which Professor Rusk alludes. Those of us who work in this area know only too well the immensity of preliminary data recovery and combination which this will require—to say nothing of the very real problem not merely of identifying the date and nominal scope of a legal change, but of measuring its effectiveness as concretely applied at varying points in time. As to this matter, I will make only two further points. First, the areas selected for analysis contained in the period studied far more than one-third of the total population of the United States. Second, V. I. Lenin is not the only psephologist with an intellectual grounding in political sociology who stressed the point that votes must at times not only be *counted* but *weighed*, that some kinds of political activity are simply more strategically important to the future evolution of a political system than others.[4] Considering not only the size but the strategic political and economic positions occupied by the four states analyzed here, a comparative study of legal and turnout changes within and among them can be amply and independently justified on such grounds. So, for that matter, can the study of critical realignments as decisively important phenomena be justified: for such realignments always involve the movements not of whole electorates, but of decisively large and strategically concentrated minorities.

But the most compelling reason for rejecting the legal-structural change theory—or more precisely, for subordinating it to its proper role in a more comprehensive set of propositions concerning the etiology of major electoral change—is that, from beginning to end, Professors Rusk and Converse have treated this intervention as though it were an uncaused cause, a kind of external intervention into the history of men. This is closely linked, one suspects, to their extreme aversion to a view of elite action, expressed in 1965, which could be read as verging on "elite conspiracy." In point of fact, the *deus ex machina* as an explanation should be left to phenomena which it does explain, such as the 1906 San Francisco earthquake. Otherwise, we are surely on much safer ground if we conclude that such changes are created by men who are more or less consciously pursuing goals through manipulation of the legal parameters of politics.[5] The case in the post-1890 South is too patent for argument, and has a richly documented literature. But the South is properly viewed as an extreme rather than a wholly deviant example of processes more generally and diffusely at work. Proximate motivations behind such actions may well have been couched in such non-conspiratorial terms as "purifying politics," "eliminating corruption" and the like. But a review of the record of the 1894 New York constitutional convention—not to mention developments in Alabama and North Carolina in the same period—yields clear evidence that a larger and very determined "search for order" dominated the work of the revisionists.

The basic model of this order, one to which politics was to be assimilated, was that of the *business corporation*, the most important and successful form of social organization ever developed in this country. Reception of this image by elites, and their transformation of the political institutions of the country in conformity to it, forms the shank of change during this period. Not only does it underlie, in my view, the legal-structural changes described by Professors Rusk and Converse, it also links such changes with the Supreme Court's 1890–1905 revolution in constitutional interpretation and with the drive toward nonpartisan local governments based on at-large elections. Above all, to the extent that this image of the business corporation came to dominate the intellectual world of the Metropole's middle and upper classes, it became very largely a consensual reception, one which found no organized ideological challenge among the overwhelming majority of politically active Americans. Thus it was that the uniquely American personal-registration statute was not only adopted all across the country, but remained very largely immune from serious academic or other criticism until very recently.

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[4] V. I. Lenin, "Vybory v Uchreditel'noe sobranie i diktatura proletariata," ("Elections to the Constituent Assembly and the Dictatorship of the Proletariat"), Dec. 16, 1919. Fifth Russian edition of *Collected Works (Sochineniye)*, Vol. 40, 1–24 (Moscow, 1958–1965).

[5] For an extremely suggestive, probably parallel case, see J. H. Plumb, *The Origins of Political Stability in England, 1675–1725* (Boston: Houghton Mifflin, 1967); and cf. Robert H. Wiebe, *The Search for Order, 1877–1920* (New York: Hill and Wang, 1967).

As I have said before, this antipartisan, corporate Utopia was nearly achieved in the 1920s, after some decades of development after the legal-structural changes which are supposed to explain so much. By that time, its near-achievement was the fruit of behavioral development in the electorate. Not until the great crash of 1929 was the corporate idol partially dethroned. Calvin Coolidge spoke not just for himself when he commented that the business of America was business; that a factory was a temple, and that he who worked there worshipped there. The point which my supposedly "conspiratorial" view of elite behavior between the 1890's and World War I sought to convey is that, under the given historical and sociological conditions of time and place, no viable alternative to this idolization of the corporation existed. I suppose that it is a truism of history that any search for political order which has achieved long-run success in its objectives has succeeded because it better corresponds in some way to the realities of social and economic power differentials in a given society than either the preceding disorders on one hand, or all imaginable alternative orders on the other. No doubt it is time to cease vain nostalgic heartburnings over what might have been after 1900 in American politics, for what might have been never happened after all. But it was certainly time in 1965 to point out a truth to my colleagues and others that a very high human price was paid for the deformation of democracy which dominated that era. An astonishingly large part of the profound political and constitutional crisis through which we are now living can be traced directly back to this deformation and its subsequent evolution; nowhere more so than in the internal structures of Congress and its linkages to the public through the electoral process. Let him who chooses to be "optimistic" about all this be so. My own effort, such as it has been, has not been to establish grounds for optimism or pessimism but to sense out the workings of a complex political system across time, to count costs, and to call things by what appear to be their right names.

One small analogic point about current electoral behavior needs to be made before I conclude. A basic thesis which underlay the 1965 article and all else which has followed is that a system tends to act as a system across time: changes in one measurable component of voting behavior can be by this time axiomatically postulated to be closely associated with changes in another. They are mutually interdependent. It is rather curious, in all this debate, that no one yet has dealt with post-1960 developments in our political system, or the arguments which have been made elsewhere about their implications. For reasons of space, I shall not attempt to present a full-scale documentation of the points I am about to make; but anyone who does the analysis on the file I will discuss will see for himself.

V. O. Key's classic *Southern Politics*[6] provides us with a simple profile of interrelated behavioral phenomena pertaining to that region, which shows the following: In the absence of party competition, turnout tends to be low. In the absence of stable party referents, electoral coalitions tend to be *ad hoc*, i.e., highly fluid from election to election and, within the same election, from office to office. When such a pattern exists, the probabilities are very high that "friends-and-neighbors" effects will become visible in election results: major candidates coming from a specified geographical area will enjoy disproportionate support from the voters of that area. One could go on, but this will suffice. We turn now to quite recent developments in the state of West Virginia—again, no doubt, a very limited selection, but one whose major features are visible very widely around the country. One reason for choosing this state is its historical record both of turnout and of very high levels of party-oriented voting behavior; and also that all of its major statewide offices since 1872 have been chosen in presidential years.

Analysis of these files leads to some unambiguous conclusions:

(1) No significant change has occurred in the distribution of party registration in West Virginia since 1956. Such change as there is has been in a glacially pro-Democratic direction.

(2) Since 1952 partisan decomposition has geometrically increased, as measured cross-sectionally across the array of offices for which the state at large is the constituency. Even leaving aside the increasingly discrete coalitional structures found in elections for federal offices, these differentials in outcomes have strongly increased (e.g., from a standard deviation of 0.98 in 1952 to one of 4.19 in 1972, with all contests in both years being two-party contests).

(3) While in 1940 a partisan intercorrelation of major-office voting reveals virtual identity in geographically partitioned coalitional structure, by 1972 severe decomposition has occurred. (Thus, county-level Presidential-Congressional correlation of per cent Democratic of the two-party [and total] vote in 1940 yields an r of +.988, and a gubernatorial-congressional r of +.994. By 1972 these figures had fallen to +.536 and +.503, respectively.)

(4) Increasing insulation of congressional incumbents has been detected by a number of

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[6] (New York: Knopf, 1949).

scholars as a major national characteristic of recent voting behavior. This phenomenon is also conspicuous in West Virginia. The last time any of the state's incumbent congressmen was defeated in a general election was in 1958. Examination of congressional elections in the First ("Panhandle") congressional district reveals that the breakaway toward dissociation of the congressional vote from voting for other offices can be traced to the 1960 election: Nixon received 46.8 per cent of the vote in the district while the second-term incumbent, Arch Moore (R), received 60.3 per cent. It should be noted, first, that this distribution occurred within district boundaries which had not been touched since 1916; and second, that—until 1960—this district had been one of the most durably competitive of any in the state or the country. By 1972, in an expanded district, we find McGovern winning 35.8 per cent of the district's vote and Robert Mollohan (D), the incumbent congressman, winning 69.4 per cent, a gap of 33.6 percentage points.

(5) With all this decomposition of the partisan intermediate between the voter and the candidates /offices on the ballot, one might well expect that sooner or later "friends-and-neighbors" effects might emerge. In 1972 they became generally visible, even for such relatively minor statewide offices as Attorney General. We may also examine one specific case, Mineral County. This county is the home of Harley Staggers (D), who has represented the Second Congressional District uninterruptedly since 1948. Here again, 1960 must be viewed as a "breakaway" year. While the mean party vote for all offices in this county reveals extremely close and long-lasting party competition (the range being between 42.1 per cent [1956] and 58.4 per cent [1964] Democratic of the two-party [and total] vote), Staggers moved into the noncompetitive range with 64.9 per cent in 1960, a lead of 16.8 per cent above the mean. By 1972, the mean for all offices in this county was 50.4 per cent Democratic; Staggers, receiving 79.1 per cent of the vote cast, led this mean by 28.7 per cent.

(6) One might, then, anticipate that whatever factors have been at work to reduce the intermediate influence of party on voting decisions might be closely linked in time with a decay in voting participation. Again, this is what we find, for both presidential and off-year elections. In 1972 the turnout rate for president fell to 64.9 per cent in West Virginia. One must go back prior to the re-admission of ex-Confederates to the state's electorate in 1870 to find a lower rate of participation. In the off-year contests, the decline in participation has been even more dramatic, reaching an all-time low of 40.3 per cent of the potential electorate in 1970. This, it should be noted, was an election with an important office (U.S. Senator) on the ballot; yet not even in the 1920's, or in the wartime election of 1942, were these depths reached. They contrast markedly with turnout rates such as 84.8 per cent in the 1890 off-year election, 84.0 per cent in 1894, or even the 65.7 per cent level reached in 1934. Very probably, the fact that the incumbent Senator, Robert C. Byrd (D) won with 77.6 per cent of the vote had some relationship to this massive voter apathy: unless one includes the uncontested 1863 and 1864 state elections, no precedent can be found for so sweeping a majority in a nominally partisan contest in this state.[7]

Now, all of this emergent behavioral pattern is obviously and closely linked in time with a major political crisis in the affairs of the American people. One of the most conspicuous aspects of this crisis has been a steep upward movement in anxiety and alienative responses to questions such as "People who are in charge of things don't care what I think." Obviously, a series of sample surveys of West Virginia's voters would be required to ascertain whether such an increase in alienation exists there, and if so whether and how it is linked to the size and behavior of the state's active electorate. But one can reach a less assertive position. First, the American electoral system is extraordinarily complex, reflecting as it does a complicated constitutional scheme involving federalism and the separation of powers. Second, as Key has pointed out, "party is the solvent of federalism," i.e., it permits integration of coalitions and mass demands (if any) across office-specific and geographical boundaries. Third, a decomposition of the relevance of party, for whatever reason, will be closely associated with the voter's quest for other cues to his voting decision. One of these cues may be the visibility of legislative incumbents; another, the chance that a major-party candidate comes from his home town. Fourth, the net effect of these developments will be a *radical reorganization* of American electoral politics along office-specific (from the voter's perspective: candidate-specific) lines. Fifth, this will immensely increase the conservative policy effects of the constitutional structure, very much along the lines hoped for by Madison in the Tenth Federalist. Key's solvent is itself dissolved. Sixth, while those voters who are still active will seek heterogeneous nonparty cues for their voting decisions, this reorganization will also serve to baffle and confuse a large number of citizens. The constitutional structure is immensely involuted, as every "a-bill-becomes-a-law" study duly reveals. When the simplifications provided by party evaporate, the voter is faced with a naked complexity which is sometimes unfathomable even to holders of Ph.D.'s in political science. Under such conditions, it would be only to be expected that participation rates would decline *pari passu* with increases in decompositional indices. On a smaller scale, this trend seems remarkably similar to those of the early twentieth century's "changing political universe."

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[7] The decay in presidential participation from 1960 through 1972 is one of the most significant aspects of recent American electoral politics. It cannot in any way be viewed as primarily due to the addition of the 18–20 age cohort to the electorate. It is easy enough to demonstrate that if as few as 35 per cent of this cohort voted in 1972, turnout in previously enfranchised age cohorts could not have been higher than 57.4 per cent. The range of turnout in 1972 among previously enfranchised age cohorts lay almost certainly between 56.5 per cent and 57.4 per cent. Moreover, if one excludes the eleven ex-Confederate states from analysis, turnout plunged in 1972 to levels very little above their all-time lows of 1920 and 1924. After more than fifty years of women's suffrage, it is highly unlikely that women can be blamed very much for this result.

It is somewhat refreshing, after all, to conclude that a system is behaving like a system. In its development there is a proper, and a properly subordinate, role for legal-structural change as an intervening variable. That proper role has yet to be worked out satisfactorily; certainly it has not been so worked out by the authors under review here. From another perspective, things are not at all so refreshing. As we are in the process of learning, the liquidation of party-in-the-electorate is associated with the disappearance of democracy, and *therefore* with the suppression of a vital element in the maintenance of political stability and legitimacy in the United States. In this connection, I cannot forbear to mention Gordon Strachan's Watergate testimony of July 23, 1973. He claims that a policy decision was made in the White House to use funds so as to avoid serious Republican contests to two classes of legislators: Democrats who supported the president's Vietnam war position, and another group of Democrats backed by organized labor, whose support Nixon sought for himself in 1972. The inferences which Senator Weicker made from this disclosure were precise. First, this ensured from the start that the Republican party could have no chance to win control of either house of Congress. Second, this reflected an aggrandizement of the president's personal power at the direct expense of the continuing electoral viability of his own party.

So it is that deliberate elite strategies may reinforce or accelerate trends toward the liquidation of party-in-the-electorate. Whatever the situation around 1900, the Watergate affair *in toto* and this particular episode in particular are useful reminders that conspiracies sometimes do exist. Sometimes, in fact, a theory of specific conspiracy best fits the ascertainable facts. Nor is there any very good reason to doubt that the decay of party-related voting behavior in our time has an intimate relationship—of proximity and interaction, if not of causality—with the crisis of constitution and legitimacy of which Watergate is currently the leading symptom. Who gains if party disappears and Madison's dream is finally realized, even in a shape he would never recognize? Is it really necessary, at long last, to ask?

By all means, therefore, let us have a complete replication of *The American Voter*, based both upon current surveys and upon careful longitudinal analysis of attitudinal and voting changes and consistency from the Eisenhower to the Nixon eras. This replication will serve the important scholarly purpose of bringing together in a single place the findings of many recent, scattered articles, and of reconciling, modifying or rejecting their arguments. It will not, alas, tell us anything conclusive about the nineteenth-century American electorate: the sociological structures of that era, and the political democracy which they nourished, have vanished. But it will tell us a lot about the intrusion of stress upon American citizens in today's world. One can confidently predict that the exercise will be worth doing both for what is discovered and for what needs to be recombined and rethought. We can continue at least to hope and to suppose that, by the time they appear, the findings of such a replication will be of more than historical interest for scholars and practitioners of American politics.

Social Change and Political Mobilization in the United States: 1870–1960*

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In the literature on political change, the achievement of mass participation is viewed as a crisis which must be passed if the process of political development is to continue.[1] Mass participation is seen as highly significant even when such participation is nothing more than a public ritual. Since citizen participation in the political process has been shown to correlate with macro-social characteristics in a variety of cultural contexts,[2] accurately specifying these social antecedents of the diffusion of mass political participation is a major concern of students of political change.[3] The widespread evidence of this correlation has provided a large measure of the empirical justification for viewing mass participation as a co-equal factor in the development syndrome. Neither this shared theoretical concern, nor the associated empirical research, have as yet produced any reliable contributions to the general theory of political development or a more limited theory of political mobilization.[4]

Using data drawn from the preceding century of American history, this paper will attempt to resolve the problems which have plagued previous research efforts and to develop a reliable causal model of the social bases of political mobilization in one society, the United States. Two factors motivated the choice of the United States as the object of research. First, the United States has undergone a very rapid process of economic, social, and political change during the past century. Second, the period under consideration may be reliably examined with data available in a number of published sources. It therefore presents the possibility of conducting, with sound data, a longitudinal examination of a society undergoing fundamental social and political redistributions.[5] The data points of the research are the states of the United States at each decennial census from 1870 to 1960.

A Critique of Previous Research

Previous research on the process of political mobilization and social change has been possible only by virtue of a fundamental, and erroneous, assumption. That assumption is quite simply that the process of change is itself unchanging. We have absolutely no empirical basis for such an assumption, and evidence will be shortly presented to show that the assumption is, in fact, incorrect.

It is impossible to utilize cross-national data to test hypotheses about development sequences without assuming an invariant process. It is only by making this assumption that an argument may be developed in which one society, exhibiting lower orders of magnitude on a variable, may be treated as a temporally posterior representative of a society exhibiting higher orders of magnitude on the same variable. We cannot use data from one point in time to make inferences about change through time without assuming that the form of change is a constant.[6]

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* The author would like to thank Professor Alfred B. Clubok of the University of Florida for reading and commenting on earlier versions of this research.

[1] For a review of approaches and research in the area see Myron Weiner, "Political Participation: Crisis of the Political Process" in *Crises and Sequences in Political Development,* ed. Leonard Binder (Princeton: Princeton University Press, 1971), pp. 159–204.

[2] Seymour Martin Lipset, "Some Social Requisites of Democracy: Economic Development and Political Legitimacy," *American Political Science Review,* 53 (March, 1959), 69–105; also Philips Cutright, "National Political Development: Measurement and Analysis" in *Politics and Social Life: An Introduction to Political Behavior,* ed. Nelson W. Polsby, Robert A. Dentler, and Paul A. Smith (Boston: Houghton Mifflin, 1963), pp. 771–780.

[3] For a general theoretical review of these linkages see Ioan Davies, *Social Mobility and Political Change* (New York: Praeger Publishers, 1970), pp. 11–46; Daniel Lerner, *The Passing of Traditional Society: Modernizing the Middle East* (Glencoe, Illinois: The Free Press, 1958), pp. 43–75; Donald J. McCrone and Charles F. Cnudde, "Toward a Communications Theory of Democratic Political Development: A Causal Model," *American Political Science Review,* 61 (March, 1967), 72–79.

[4] Samuel P. Huntington, "The Change to Change: Modernization, Development and Politics," *Comparative Politics,* 3 (April, 1971), 283–322.

[5] The data for this research were derived from the various reports of the U.S. Bureau of the Census, *U.S. Census of Population* (Washington, D.C.: G.P.O., 1872, 1883, 1895, 1901, 1913, 1922, 1933, 1943, 1952, 1962).

[6] For a similar critique see Lee Sigelman, "Modernization and the Political System: A Critique and Preliminary Empirical Analysis," *Sage Professional Papers in Comparative Politics,* 2 (1971), 12–13.

One author has recognized this difficulty and attempted to deal with it by treating longitudinal data from only one society.[7] This solution is only partially valid, for the author then proceeds to enter a series of data points, drawn from different points in time, into the same correlation analysis. To treat data drawn from 1790 in the same linear analysis as data drawn from 1960, again requires the assumption of an invariant process. Using longitudinal data we are faced not with the problem of establishing one society as a temporally posterior representative of another, but with the high probability that the correlations generated by the analysis are mere statistical artifacts. In essence, a causal model is a statement of the relationships obtaining among a set of variables derived from their covariation. As the magnitude and rate of particular changes increase or decrease, we would expect an alteration in the observed relations of variables, and thus, in the inferred pattern of the process itself. The pattern of relationship which obtains for an extended intertemporal analysis may, in fact, be nothing more than a bastardized combination of several patterns which prevailed during subsets of the time span under consideration. Assuming an invariant process may allow the researcher to ignore this possibility, but it does not eliminate it, and the question remains whether the assumption or the research results are correct.

In part, the assumption by researchers of an invariant process may be ascribed to the inclusion of that assumption in the argument by Lerner.[8] Most of the causal analysis of the mobilization process has referred to Lerner's work to justify the hypothesized pattern of relations.[9] Although Lerner treats the process as essentially circular, he nevertheless fails to deal with the problem of an alteration in the nature of the process. Yet, this alteration is implicit in the work of those authors concerned with "stages" or "crises" of development.[10] Lerner's failure to deal with the possibility of different patterns of change meant a theoretical position that invited researchers to employ methodology not capable of discerning sub-patterns of the process. The most appropriate methodology for a causal model of the development process is a design providing longitudinal aggregation of cross-sectional analyses derived from only one society.

In very general terms there is agreement on the major factors comprising the modernization process. Social mobilization is seen as one significant characteristic and is understood to be dependent upon major changes in the amount and form of wealth production.[11] Similarly, modernization is expected to entail increases in the level of individual adaptability, whether that adaptability is seen as "empathy,"[12] "innovation,"[13] "achievement orientation,"[14] or simply an acceptance of "modern"[15] values.

The process of modernization is often referred to as a "syndrome,"[16] indicating a general agreement on the high level of interdependence of factors entailed by the modernization process. Beyond this superficial agreement lie major differences in the expected time-order and significance of the elements of modernization. A number of alternative models are therefore possible as we pick and choose among contending theories. Empirical validation of the synthesized model however, is the only method of ultimately choosing the most acceptable

A Tentative Model

The argument upon which the following model will be based proceeds as follows: Increasing wealth leads to increasing social mobilization. Social mobilization can be broken into two major, interrelated processes—urbanization and mass communication—each more specific and more easily operationalized. In the following discussion, urbanization will be considered only in terms of intense demographic concentration. Industrialization, as it occurred under past levels of technology, required extreme demographic concentration. Throughout the history of industrialism, the growth of the cities has been primarily the result of stripping the hinterland of agrarian population. Urbanization did not result merely from natural population increase in pre-existing cities.

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[7] Gilbert Winham, "Political Development and Lerner's Theory: Further Test of a Causal Model," *American Political Science Review*, 64 (September, 1970), 810–818.

[8] Lerner, *The Passing of Traditional Society*.

[9] Including McCrone and Cnudde; Winham; and Arthur K. Smith, "Socioeconomic Development and Political Democracy: A Causal Analysis," *Midwest Journal of Political Science*, 13 (February, 1969), 95–125.

[10] Lucian W. Pye, *Aspects of Political Development* (New York: Little, Brown, 1966).

[11] Karl W. Deutsch, "Social Mobilization and Political Development," *American Political Science Review*, 55 (September, 1961), 493–514.

[12] Lerner, *The Passing of Traditional Society*.

[13] Everett E. Hagen, *On the Theory of Social Change: How Economic Growth Begins* (Homewood, Illinois: The Dorsey Press, 1962), pp. 12–16.

[14] David C. McClelland, *The Achieving Society* (Princeton: D. Van Nostrand, 1961) also David C. McClelland and David G. Winter, *Motivating Economic Achievement* (New York: The Free Press, 1969).

[15] Alex Inkeles, "Participant Citizenship in Six Developing Countries," *American Political Science Review*, 63 (December, 1969), 1120–1141.

[16] Pye, pp. 45–48.

Indeed, as Deutsch has noted, in industrializing societies, new cities sprang up as new industries were formed.[17] In a cyclical process, industrialization makes large scale urban populations possible, and increasing urban populations are necessary for further industrial expansion. The byproduct of this process is an ever-increasing number of mobilized individuals who, by their activities, will tend to reinforce both industrialization and urbanization. Urbanization necessitates the second process of social mobilization—mass communication. To create an urban *society*, mass communication is absolutely essential.

In earlier epochs, communication required literacy since the media were print. This has led some theorists to presume that mass literacy is necessary for mass communication.[18] There is, in reality, no reason for mass literacy to precede mass communication. Prior to the development of electronic media, as long as the nodal personality in a sociometric network was literate, the other persons in the net could effectively be considered part of the mass communications system.

A society cannot support urbanization without the large-scale community integration made possible by the process of mass communication. Demographic concentration can continue for some time, but the process of modernization will break down under the pressure and confusion of a nonintegrated environment. An extension of the cycle is apparent; industrialization leads to urbanization, which in turn is necessary for further industrialization. Yet, continued urbanization is not possible without the community integrating functions of mass communication.

Mass communication has a significant impact on the further development of mobilized individuals. Where urbanization acts as the first stage of social mobilization, mass communication acts as a second stage by extending the range of increased individual aspirations, making more individuals aware of currently available alternative life styles.

As the range of alternative roles and life styles is increased through the introduction of new forms of wealth production, the old social order is subjected to extreme stress and individuals are forced into new patterns of activity.[19] Increasing social mobilization will lead to pressure for higher individual adaptability. At this juncture, social stress becomes reflected in the governmental sphere. Individuals are faced with the unpleasant realization that their abilities are not commensurate with their aspirations. It has been argued that social mobilization destroys the individual's links with his previous social position. In order to exist in his new situation, the individual must find new forms of behavior in which to secure himself. He must, in short, take on new roles. Yet his ability, to fill new roles is contingent upon his level of general adaptability. If the rate of mobilization is slow, learning from experience will be sufficient to perpetuate modernization for a time. Repeated exposure to modern life styles may simply inure the individual to the harassments of modernity. The more rapid the original rate of mobilization, however, the more necessary an efficient form of education will be.[20] Insecurity and maladjustment are common products of repeated failures to cope in the face of an incomprehensible social order. As the number of maladjusted individuals increases, the ability of the remaining mobilized individuals to sustain the process of modernization decreases. Hence, the process of social mobilization creates a situation in which education becomes increasingly necessary. In terms of an entire society, mass formal schooling becomes necessary if the process of modernization is to continue.

Formal schooling provides the only method of increasing individual adaptability in the mass of the mobilized populace. As the rate of social change increases, governmental activity becomes unavoidable. Were it possible to generate mass education without state intervention, then the process of modernization would continue without the state. Mass education is a social service with no immediate return, and government activity is thus an intervening variable between the pressures resulting from mass mobilization and the increased need for education. Government activity is obviously necessary to the process of social modernization, yet it is not a logical link in the chain of causality. It is the *rate* of modernization which renders non-governmental solutions to the problem impossible.

This increase in the adaptability of the mobilized members of the society afforded by mass education insures the continuation of the process.

To develop a logically satisfying end to this chain, our comprehension of politics must compel our perception of political mobilization as a necessary response to the social changes described in the model. Such a perception may be assured only by employment of a delimitation of the concept "politics" which underscores its relation to social tension. The chain of events described in the model is tension producing; but in what sense does it necessitate political mobilization?

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[17] Karl W. Deutsch, *Nationalism and Social Communication: An Inquiry into the Foundations of Nationality* (New York: Wiley, 1953), pp. 97–126.

[18] See Lerner and also McCrone and Cnudde.

[19] Gerhard E. Lenski, *Power and Privilege: A Theory of Social Stratification* (New York: McGraw-Hill, 1966), pp. 31–32.

[20] In fact, much modernization research used the formal educational attainment of the populace as a measure of modernization, thus framing the argument in terms of education as an index of modernity instead of as a means to modernity. See Lerner and also Deutsch, "Social Mobilization and Political Development."

This research employed the following working definition of politics: *The attempted or actual use of the authority of the state to redistribute the life-chance matrix of the society to a configuration which is not legitimizable by traditional social norms.* This definition emphasizes the nonsubstitutable nature of political activity, and also meets the criticism that the definitions of Lasswell and Easton are also descriptions of the stratification system.[21]

Social processes of economic change and social mobilization disorient the norm structure of the previous stratification system, introducing new kinds of roles and valued goods. In large measure, the pre-existing norm structure contains referents neither for the appropriate *form* of a new distribution of valued goods, nor an appropriate *means* of agreeing upon one.[22] Therefore, political activity —recourse to the generalized decision processes of the state—becomes a necessary substitute for customary agreement. As the rate of change increases, there is correspondingly greater pressure to generalize and externalize decision processes.

Over an extended span of time the modernization process may continue only with the assumption by the state of responsibility for mass education. This assumption of responsibility is, in itself, a major political decision, although in terms of the mass the decision is undertaken by a small minority of the total society. Forces generated by the processes of social change may produce pressure for widespread education, but the political involvement of the mass of the populace is contingent upon the increasing adaptability fostered by education. Mass political mobilization serves to legitimize the new distribution of life chances, and also to provide an outlet for frustrations regarding perceived imbalances in the current distribution.[23]

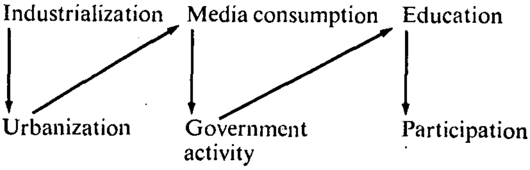

Figure 1. Proposed Sequence of Relationships

This hypothetical sequence completely reverses the literacy/media aspect of Lerner's argument.[24] Lerner's presentation is derived from the perspective of the individual, and is thus bound to the sequence by which an individual moves from lesser to greater empathic ability. From a more general perspective of social change and adaptation, however, it is realistic to argue that desire precedes ability, and that the wish generated by exposure to modern activity leads to pressure for a means to fulfill that wish.[25] As a first approximation, this research will test the sequence of change depicted in Figure 1.

It must be kept in mind that this pattern is not expected to hold for the entire century under consideration. In 1870, at the beginning of the period, the United States was basically an agrarian society, despite earlier changes prerequisite to rapid industrialization.[26] As a pattern of change, the model is expected to be empirically valid only for an early portion of the century, and subsequent alterations in the relationships of the factors will be identified as they occur.

Methodology

As noted earlier, the major objection to the methods of previous researchers was directed not at the process by which they identified their models, but with the methods they used to generate their base correlations. Either the correlations were drawn from a single cross-section, or they dealt with longitudinal data by ignoring possible changes in the process under study.

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[21] Lenski, p. 3ff.

[22] There is an interesting parallel here between Lenski's discussion of institutional evolution of stratification systems, and Thorstein Veblen's *The Theory of the Leisure Class* (New York: Viking Press, 1912), pp. 60–70. At both ends of Lenski's spectrum, hunting and gathering societies and mature industrial societies, we find relatively loose stratification systems. Veblen's discussion of conspicuous consumption suggests that in primitive societies and industrial societies, conspicuous consumption is a right, whereas in traditional societies (Lenski's agrarian societies) it is a privilege. In both forms of society where conspicuous consumption is a right of the mass, we find loose stratification systems and no perception of limited good. Given the absence of limited good, and the flexibility of the stratification systems, conspicuous consumption is an obvious and probably necessary means of identify definition. Veblen argues that conspicuous consumption is a means of maintaining reputation, a primary component of individual identity. David Riesman's discussion of the "inner-directed" and "other-directed" individual follows similar logic; see *The Lonely Crowd: A Study of the Changing American Character* (New Haven: Yale University Press, 1964). For a recent analysis of this interaction see Orrin Klapp, *Collective Search for Identity* (New York: Holt, Rinehart and Winston, 1969).

[23] Weiner, "Political Participation," and Inkeles, "Participant Citizenship," pp. 1123–1124.

[24] Lerner, *The Passing of Traditional Society.*

[25] Jesse F. Marquette, "Social Mobilization and the Philippine Political System," *Comparative Political Studies,* 4 (October, 1971), 339–347.

[26] Seymour Martin Lipset, *The First New Nation: The United States in Historical and Comparative Perspective* (New York: Basic Books, 1963); Walter W. Rostow, *The Stages of Economic Growth: A Non-Communist Manifesto* (Cambridge: Cambridge University Press, 1960).

To correct these deficiencies the following procedure was employed:

For each decennial census period a cross-sectional correlation matrix was generated using the states of the United States as data points. The data points used were those states which were so recognized at the time of the respective census; hence, certain southern states were not included during the period 1870 to 1880. Territories were added to the data set as they were admitted as states, beginning with the next following census period. For each pair of variables, the decennial correlations were then averaged over the total time span. Averaging was carried out by the use of an adjustment formula which controlled for the relative size of the correlations and the original number of data points on which they were based.[27] This method of averaging gives the final average correlation a reliability equal to the total number of data points rather than to the decennial base. Simon-Blalock causal analysis was then conducted on the aggregate correlation matrix for the entire time span under consideration.[28]

As expected, the model was not found appropriate for the entire century. The aggregate matrix for the entire period was then disaggregated into two periods by reference to the intertemporal alterations of relation between the base correlations. This dis-aggregation resulted in the identification of two models. One, corresponding to the proposed model, was identified for the period 1870 to 1910. The second, covering the period 1920 to 1960, was largely similar to the first, but some modifications appeared in the last stages.

After the two successive phases of the model were identified by cross-sectional analysis, a new set of correlation matrices was generated. These matrices were based on time lagged correlations; data from 1870 were correlated with data from 1880 and so on, through the entire period. The procedure outlined above was then repeated on this new set of matrices, and the existence of the two successive models previously identified was reconfirmed.

The operational indicators of the six concepts, and their component variables are reported in Table 1. Where relevant, the method of combining variables into indices was simple summing, all component variables having been computed on the same base.

Election data were averaged to a decennial base to give an aggregate participation index, Elections for 1868 and 1872 were averaged for the participation index for 1870, the procedure being repeated for the entire period.

Both procedures of testing the model were repeated substituting off-year Congressional election turnout for presidential turnout. The results are not reported in detail because of their redundancy; the analysis of these data confirmed the models identified with the presidential data.

This study uses voting in presidential elections as a proportion of the total population. This usage is dictated by the nature of the model under study. The model is designed to depict *political mobilization*, not simply *political participation*, and the measure of political mobilization is affected not only by whether an individual chooses to participate in the system, but also by whether the system will allow him to participate. Since at any point in time, legal modifications could alter the composition of a state's eligible electorate, measuring the percentage of eligible voters would not take into account the system's control over individual mobilization. In other words, an index based on the eligible electorate, by states, might reflect a state with highly restrictive eligibility (but high turnout of eligible voters) as highly mobilized, while another state with a broad base of eligibility (and the same number of voters) would appear far less mobilized. Such usage would not reflect both aspects of mobilization—the individual and the societal. Concerned with political mobilization, rather than simply political participation, it was necessary to use an index which tapped the totality of the society's involvement in political activity.[29]

Analysis

A correlation matrix composed of the averaged decennial correlations from 1870 to 1960 is presented in Table 2. The model and prediction equations derived from this matrix are presented in Table 3.

As might be expected, the model postulated earlier is clearly not identifiable for the entire period under study. The prediction equations indicate that the first four variables are related in the sequence predicted from the theoretical presentation, but the chain does not follow the expected sequence from media consumption to political mobilization.

To retest the model with subsets of the total

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[27] Truman L. Kelley, *Fundamentals of Statistics* (Cambridge: Harvard University Press, 1947), pp. 363–365.

[28] Hubert M. Blalock, Jr., *Causal Inferences in Non-Experimental Research* (Chapel Hill: University of North Carolina Press, 1964).

[29] For an excellent analysis of the misleading effect of changes in electoral laws and practices on the conclusions of historical patterns, see Philip E. Converse's critique of Walter Dean Burnham's "The Changing Shape of the American Political Universe," *American Political Science Review*, 54 (March, 1965), 7–28; Philip E. Converse, "Change in the American Electorate," in *The Human Meaning of Social Change*, ed. Angus Campbell and Philip E. Converse (New York: Russell Sage Foundation, 1972), pp. 263–337.

Table 1. Variables Included in Each Final Index with Years of Inclusion

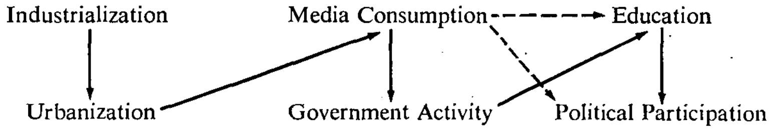
Variable	Index Components	Years
INDUSTRIALIZATION	Per capita value added in manufacturing	1870–1960
URBANIZATION	Percentage of population in cities larger than 2,500	1870–1960
	Percentage of population in cities larger than 25,000	1870–1960
MEDIA CONSUMPTION	Average daily newspaper circulation per 1,000 population	1870–1960
	Postal receipts, per capita	1870–1960
	Telephones per 1,000 population	1880–1960
	Radios per 1,000 population	1930–1960
GOVERNMENT ACTIVITY	Per capita state and local government expenditures	1870–1960
EDUCATION	Percentage of population which is literate	1870–1960
	Percentage of the school age population attending high school	1870–1960
	Percentage of the 19 to 24 age group enrolled in college	1910–1960
POLITICAL PARTICIPATION	Voter turnout in presidential elections as a percentage of total population; or	1870–1960
	Voter turnout in off year congressional elections as a percentage of total population	1870–1960

Table 2. Aggregate Correlation Matrix of the Six Indices for the Period 1870 to 1960

	Industrialization	Urbanization	Media	Government	Education	Participation
I	1.0000	0.8044	0.5861	0.4453	0.3218	0.2405
U		1.0000	0.7705	0.5709	0.3831	0.2661
M			1.0000	0.7319	0.5946	0.5140
G				1.0000	0.6554	0.5392
E					1.0000	0.7213
P						1.0000

Table 3. Model and Analysis of the Cross-Sectional Correlations: 1870–1960

1870–1960



Analysis
1870–1960

Actual − Expected = Difference

IM.U = IU × UM
.8044 × .7705 .5861 − .6197 = −.03

UG.M = UM × MG
.7705 × .7319 .5709 − .5639 = .01

IG.UM = IU × UM × MG
.8044 × .7705 × .7319 .4453 − .4563 = −.01

ME.G = MG × GE
.7319 × .6554 .5946 − .4797 = .11

GP.E = GE × EP
.6554 × .7213 .5392 − .4727 = .06

MP.EG = MG × GE × EP
.7319 × .6554 × .7213 .5140 − .3460 = .17

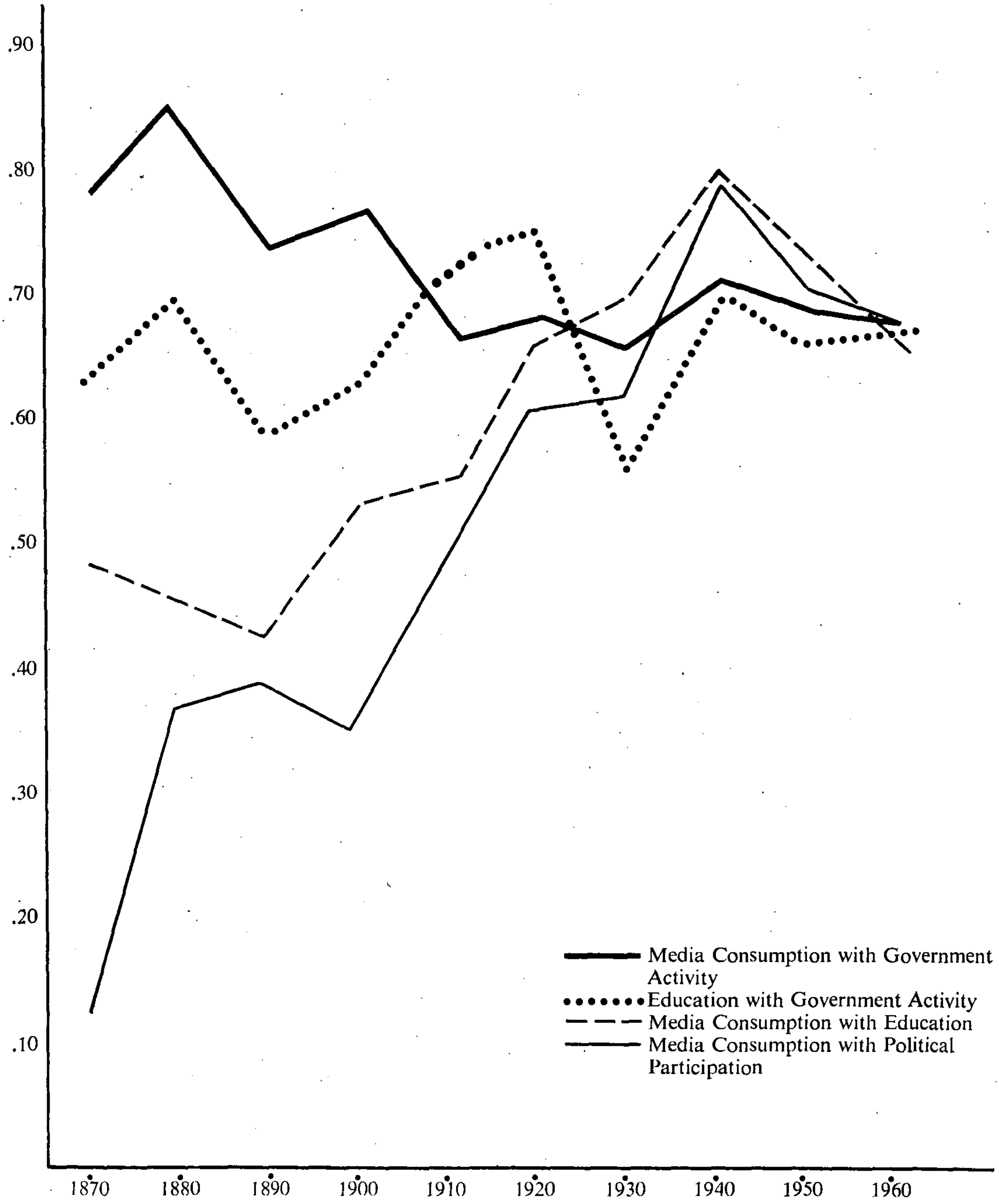


Figure 2. Plot of Correlation Coefficients of Media Consumption, Government Activity, Education, and Political Participation Versus Time: 1870 through 1960

period under study it is necessary to decompose the matrix in Table 2 into two or more submatrices. The significant question is the identification of the appropriate time periods on which to base the new correlations. It was decided that the best procedure was to resort to an examination of the intertemporal behavior of the relevant correlation coefficients.

Figure 2 is a plot of the correlations among media consumption, education, government activity and political mobilization by census years. The plot of these correlations indicates that at some time during the period 1915 to 1925, the magnitude of the correlations became very similar, and began to vary in much the same fashion. In particular, the track of the correlation between

Table 4. Aggregate Correlation Matrix of Six Indices for the Period, 1870 to 1910

	Industrialization	Urbanization	Media	Government	Education	Participation
I	1.0000	0.8875	0.6230	0.5635	0.4542	0.1204
U		1.0000	0.7670	0.6280	0.4349	0.1665
M			1.0000	0.7578	0.4986	0.3621
G				1.0000	0.6545	0.5075
E					1.0000	0.7160
P						1.0000

media consumption and political mobilization suggests an increasing direct relationship between these two variables. As illustrated in Table 3, the inability to partial out mobilization was one of the major difficulties with the test of the model over the entire century. On this basis it was decided to decompose the matrix for the entire century into two matrices covering the periods 1870 to 1910 and 1920 to 1960.[30]

The aggregate correlation matrix for the period 1870 to 1910 is presented in Table 4. Table 5 presents a model and set of prediction equations for the period much more gratifying to the original expectations. All prediction equations in Table 6 are well within the range of the minimum .10 cutoff suggested by Blalock.[31] The analysis in Table 5 provides support for the argument advanced earlier. Tables 6 and 7 present the correlation matrix, model and prediction equations for the time period 1920 to 1960. The model in Table 7 is a replication of the relationships found for the period 1870 to 1960. However, the size of the differences of the expected and actual correlations for the paths *Media Consumption—Government Activity—Education* and *Media Consumption—Government Activity—Education—Political Participation*, are much larger than the respective differences for the entire time-period test. This increase indicates that the model for the entire time period was unclear because of the altered nature of the relationships which prevailed during the second half of the period under study.

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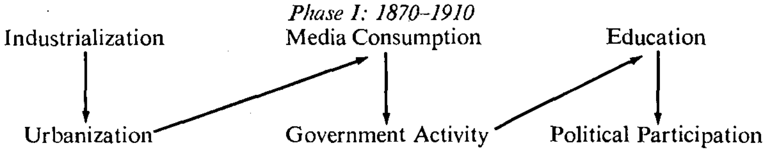
[30] Interestingly enough this empirical cutpoint corresponds closely to the transition from the "maturity" period to the "high mass consumption" period suggested by Rostow for the U.S., Rostow, pp. 75–76.

[31] Blalock, *Causal Inferences in Non-Experimental Research.*

Table 5. Model and Analysis of the Cross-Sectional Correlations: 1870–1910



Analysis
Phase I: 1870–1910

Actual − Expected = Difference

$IM.U = IU \times UM$
$.8875 \times .7670$ $.6230 - .6807 = -.05$

$UG.M = UM \times MG$
$.7670 \times .7578$ $.6280 - .5812 = .04$

$IG.UM = IU \times UM \times MG$
$.8875 \times .7670 \times .7578$ $.5635 - .5158 = .05$

$ME.G = MG \times GE$
$.7578 \times .6545$ $.4986 - .4959 = .002$

$GP.E = GE \times EP$
$.6545 \times .7160$ $.5075 - .4686 = .04$

$MP.EG = MG \times GE \times EP$
$.7578 \times .6545 \times .7160$ $.3621 - .3551 = .007$

Table 6. Aggregate Correlation Matrix of Cross-Sectional Correlations: 1920–1960

	Industrialization	Urbanization	Media	Government	Education	Participation
I	1.0000	0.7675	0.5626	0.3346	0.1850	0.3690
U		1.0000	0.7833	0.5140	0.3304	0.3772
M			1.0000	0.6882	0.6992	0.6876
G				1.0000	0.6636	0.5946
E					1.0000	0.7614
P						1.0000

Before attempting to deal with the lack of clarity in the second phase of the process, a reexamination is conducted by developing intertemporal correlation matrices for the two phases of the model. These time-lagged correlations enable an examination of the behavior of the model through time, and eliminate the possibility of mutual causation.

Tables 8 and 9 present the correlation matrix, model, and analysis for the period 1870–1910. The analysis in Table 10 replicates the conclusions which were derived from the cross-sectional analysis in Table 5. During the early period of major industrial and social change the model proceeds as expected.

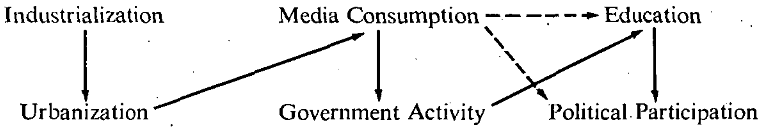
The data and analyses in Tables 10 and 11 also replicate the finding reached for the second period through the cross-sectional analysis. Again, unexpected direct relationships are observed among media development, education and participation Clarification of the causal priorities obtained during the period 1920–1960, is possible only by reference to the conceptual basis of the proposed model.

The original argument postulated a relationship between increasing available wealth and social mobilization. It was argued that industrial development necessitated media development. Urbanization was understood to be an intervening variable, primarily because of the characteristics of the productive process. Similarly, media development was expected to necessitate increased government activity which, in turn, fostered educational development. Government activity was understood to be a necessary intervening variable because of the extreme cost of developing an educational establishment.

Both urbanization and government activity, as

Table 7. Model and Analysis of the Cross-Sectional Correlations: 1920–1960

Phase II: 1920–1960



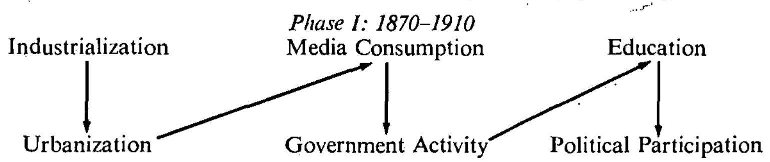
Analysis
Phase II: 1920–1960

			Actual — Expected = Difference
IM.U	=	IU × UM .7675 × .7833	.5626 — .6000 = —.04
UG.M	=	UM × MG .7833 × .6882	.5140 — .5286 = —.01
IG.UM	=	IU × UM × MG .7675 × .7833 × .6876	.3346 — .4100 = —.07
ME.G	=	MG × GE .6882 × .6636	.6992 — .4455 = .25
GP.E	=	GE × EP .6636 × .7614	.5946 — .5033 = .09
MP.EG	=	MG × GE × EP .6882 × .6636 × .7614	.6876 — .3391 = .32

Table 8. Aggregate Correlation Matrix of the Intertemporal Correlations: 1870 to 1910

	Industrialization	Urbanization	Media	Government	Education	Participation
I	1.0000	0.8805	0.6176	0.4935	0.3937	0.1677
U		1.0000	0.7414	0.5383	0.3855	0.1815
M			1.0000	0.6718	0.4715	0.3636
G				1.0000	0.6669	0.4926
E					1.0000	0.7613
P						1.0000

Table 9. Model and Analysis of Intertemporal Correlations: 1870–1910


Phase I: 1870–1910
Industrialization
Media Consumption
Education
Urbanization
Government Activity
Political Participation

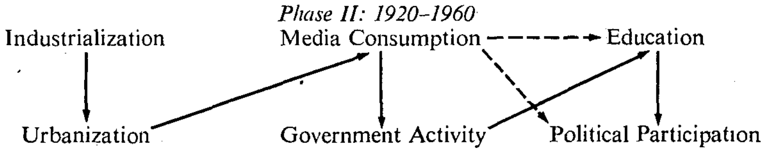
Analysis
Phase I: 1870–1910

		Actual — Expected = Difference
IM.U =	IU × UM .8805 × .7414	.6176 — .6528 = —.03
UG.M =	UM × MG .7414 × .6718	.5383 — .4981 = .04
IG.UM =	IU × UM × MG .8805 × .7414 × .6718	.4935 — .4386 = .05
MG.E =	MG × GE .6718 × .6669	.4715 — .4480 = .02
GP.E =	GE × EP .6669 × .7613	.4926 — .5077 = —.01
MP.EG =	MG × GE × EP .6718 × .6669 × .7613	.3636 — .3406 = .02

Table 10. Aggregate Correlation Matrix of the Intertemporal Correlations: 1920 to 1960

	Industrialization	Urbanization	Media	Government	Education	Participation
I	1.0000	0.7323	0.5941	0.3950	0.2084	0.4396
U		1.0000	0.7974	0.4710	0.3513	0.4482
M			1.0000	0.6223	0.6969	0.7060
G				1.0000	0.6637	0.6937
E					1.0000	0.7454
P						1.0000

Table 11. Model and Analysis of Intertemporal Correlations: 1920–1960



Analysis
Phase II: 1920–1960

		Actual	Expected	Difference
IM.U =	IU × UM .7323 × .7974	.5941	.5839	.01
UG.M =	UM × MG .7974 × .6223	.4710	.4962	−.02
IG.UM =	IU × UM × MG .7323 × .7974 × .6223	.3950	.3634	.03
ME.G =	MG × GE .6223 × .6637	.6969	.4130	.28
GP.E =	GP × EP .6637 × .7454	.6937	.4947	.20
MP.EG =	MG × GE × EP .6223 × .6637 × .7454	.7060	.3079	.39

they relate to political mobilization, may be understood as infrastructures. In the early phases of the development process, the social costs of urbanization, and the development of an educational plant must be taken into consideration. It is however possible that, as suggested by Deutsch,[32] there exists a threshold effect in terms of the establishment of basic infrastructures. While it is obvious that urbanization and governmental expenditures for education continue, in time the threshold effect may render them *causally* insignificant.

In this view the general argument for the process of political mobilization remains the same. Increasing wealth production causes social mobilization, which necessitates increasing individual adaptability if the process of mobilization and wealth production are to continue. Increasing individual adaptability makes political mobilization possible as a generalized response to the social dislocations inherent in the earlier stages of the process. Given the expected threshold for the urban and educational infrastructures, we are now discussing a four-variable not a six-variable model. The expected sequence of the four variable model is *Industrialization—Media Consumption—Education—Political Participation.*[33]

Since the division of the process into two phases has twice been confirmed, the final portion of the research employs somewhat different methodology, focusing on the examination of the process of political mobilization as an example of change through time. In this section all states will be entered into one analysis for each of the periods 1870–1910 and 1920–1960.[34] This results in two correlation matrices based on 212 and 240 cases, respectively.

Tables 12 and 13 deal with the new correlations for the period 1870 to 1910. The prediction equations in Table 13 again indicate a satisfactory solution for the model in six variables. The model identified through this technique is a replication of the model generated through the use of cross-sectional and intertemporal correlation bases.

Finally, we may now examine the contention that urbanization and government activity are functioning as infrastructures by examining the data and model presented in Tables 14 and 15. Clearly the expectations were correct, the model is now reduced to the proposed four-variable case. This four-variable model is much closer to a direct representation of the processes of interest than the six variable model, which, necessarily, contained indices of developing infrastructures.

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[32] Deutsch, "Social Mobilization and Political Development," pp. 496–497.

[33] For a similar model see Marquette, "Social Mobilization and the Philippine Political System," p. 340.

[34] Reported correlations are based on untransformed data, although all correlations were checked by logarithmic transformations since some of the growth rates were slightly curvilinear.

Table 12. Total Data Point Correlation Matrix: 1870–1910

	Industrialization	Urbanization	Media	Government	Education	Participation
I	1.0000	0.8871	0.6053	0.5777	0.4861	0.1201
U		1.0000	0.6873	0.6060	0.4581	0.1767
M			1.0000	0.8192	0.6881	0.3378
G				1.0000	0.7542	0.4841
E					1.0000	0.5315
P						1.0000

Discussion

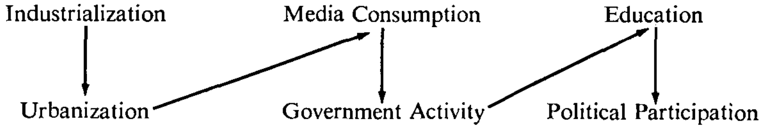
At the outset of this research it was suggested that the identification of a longitudinal model of the process of political mobilization would be a useful contribution to the study of political change. A model of this process for a one hundred year period of American history has been identified. Yet that model is merely a description of a set of relationships between variables and does not, by itself, constitute an explanation.

The original depiction of the proposed model was motivated by an implicit theory of change. In essence, that theory is based on a reformulation and synthesis of the social process and identity formation approaches to change.[35] To provide an explanation for the process represented by the model it is necessary to make that theory explicit.

Some of the most imaginative treatments of change have proceeded from the concept of social tension. Basically tension has been understood as a result of an imbalanced relationship between certain culturally established desires and the available means of satisfying them. In its original formulation this treatment was confined to a discussion of different alienated responses to such imbalance. In *Social Theory and Social Structure*, Merton advanced a paradigm of alienation based on a comparison of the two variables, cultural goals and institutionalized means.[36] Although not so treated by Merton, to maintain consistency

Table 13. Model and Analysis of Total Data Point Correlations: 1870–1910

Phase I: 1870–1910



Analysis
Phase I: 1870–1910

		Actual — Expected = Difference
IM.U =	IU × UM .8871 × .6873	.6053 — .6097 = —.004
UG.M =	UM × MG .6873 × .8192	.6060 — .5630 = .04
IG.UM =	IU × UM × MG .8871 × .6873 × .8192	.5777 — .4995 = .08
ME.G =	MG × GE .8192 × .7542	.6881 — .6178 = .07
GP.E =	GP × EP .7542 × .5315	.4841 — .4009 = .08
MP.EG =	MG × GE × EP .8192 × .7542 × .5315	.3378 — .3284 = .009

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[35] Huntington, "The Change to Change."

[36] Robert K. Merton, *Social Theory and Social Structure* (Glencoe: The Free Press, 1949), pp. 125–149.

Table 14. Total Data Point Correlation Matrix: 1920–1960

	Industrialization	Media	Education	Participation
I	1.0000	0.7355	0.5701	0.3384
M		1.0000	0.8330	0.6141
E			1.0000	0.6729
P				1.0000

with later authors, this relationship may be expressed as a simple ratio:

$$\text{Dissatisfaction} = \frac{\text{Cultural Goals}}{\text{Institutionalized Means}}$$

Merton's discussion of this relationship is oriented toward the identification of modes of adaptation, and he consequently pays little attention to the development of imbalances throughout society.

In later research, Lerner expanded this earlier conception and defined the phenomenon as one which occurred " . . . when many people in a society want far more than they can hope to get."[37] Lerner's representation of this ratio is:

$$\text{Satisfaction} = \frac{\text{Achievement}}{\text{Aspiration}}$$

Lerner and Deutsch have attempted to explain the ramification of this response pattern throughout a society by relying on the destructive impact of social processes on pre-existing social structures.[38]

In *Political Order in Changing Societies*, Huntington[39] extrapolated this ratio into a three-tiered representation of political change:

a) Social Frustration

$$= \frac{\text{Social Mobilization}}{\text{Economic Development}}$$

b) Political Participation

$$= \frac{\text{Social Frustration}}{\text{Mobility Opportunities}}$$

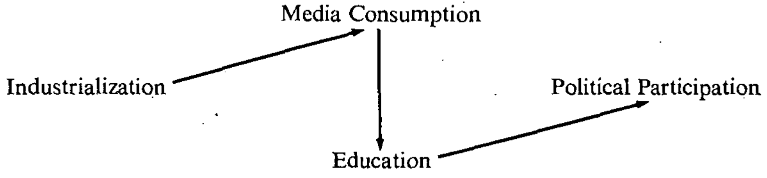
c) Political Instability

$$= \frac{\text{Political Participation}}{\text{Political Institutionalization}}$$

This presentation is an appealing description of what seems to occur within each of the three realms. The first two ratios are clearly an adequate alternative representation of the model identified in this research. As an explanation, however, this system fails on two counts: (1) it is merely asserted that individuals will be frustrated without any attempt to account for the mechanism by which they are actually frustrated; and (2) no ex-

Table 15. Model and Analysis of Total Data Point Correlations: 1920–1960

Phase II: 1920–1960



Analysis
Phase II: 1920–1960

			Actual − Expected = Difference
IE.M	=	IM × ME .7355 × .8330	.5701 − .6126 = −.04
MP.E	=	ME × EP .8330 × .6729	.6141 − .5605 = .05
IP.ME	=	IM × ME × EP .7355 × .8330 × .6729	.3384 − .4122 = −.07

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[37] Daniel Lerner, "Toward a Communication Theory of Modernization: A Set of Considerations," in *Communications and Political Development*, ed. Lucian W. Pye (Princeton: Princeton University Press, 1963), p. 333.

[38] Lerner, "The Passing of Traditional Society," pp. 43–75; "Communications Systems and Social Systems: A Statistical Exploration in History and Policy," *Behavioral Science*, 2 (October, 1957), 266–75; and Deutsch, "Social Mobilization and Political Development."

[39] Samuel P. Huntington, *Political Order in Changing Societies* (New Haven: Yale University Press, 1968), p. 55.

planation is given for the transformation of change from one level to the next. Both Lerner and Huntington are attempting reductive explanations of change, even though their primary attention is directed toward social processes. Explanation of social phenomena by reference to psychological characteristics requires a link between the two levels of explanation. Lerner's and Huntington's explanations are not compelling because such a link is never established. In short, the criticism that political theories of change lack composition laws still stands.[40] An adequate reductive explanation of political change can be advanced only if the original conception of the psychological attributes of the individual contains the seeds of the explanation of the extension into social process.

Both Pye [41] and Hagen[42] have attempted to deal with change and responses to change as they originate in a particular psychosocial process—identity definition. Neither has produced theories which are reliable descriptions of the process of social change. In general, any psychological explanation which predicts a particular behavior pattern or a particular personality type is open to question.[43] Similar personality types may manifest different behaviors in response to the same situation, and as a developmental process not all individuals subjected to the same environment develop the same personality type. For instance, Holt and Turner point out that Pye's analysis, as applied to Germany, results in diametrically opposite conclusions to those derived by Pye in the Burmese case.[44]

Hagen bases his theory of change on the concept of status deprivation, and predicts that over time this will result in the development of an "innovative" personality.[45] In essence, Hagen's argument reduces to one category of Merton's paradigm of alienation. Status deprivation produces an imbalance in the ratio of goals/means, but in Merton's paradigm, innovation is only one possible response.[46]

As an explanation of social change, the most serious failure of Hagen's argument lies in its reliance on the occurrence of a particular event, status deprivation, as the causal basis for his explanation. Although he treats the development of an innovative personality as a *process*, his explanation in its entirety is not process oriented. This lack of process orientation forces Hagen to argue that economic modernization results from the existence of a particular group of individuals, whose forefathers, as a group, were subjected to severe status deprivation. He is not able to provide any explanation of the manner in which the existence of this group ramifies through the entire society to produce a self-sustaining process of change. These two examples indicate that an explanation of social change which relies on the development of particular personality types is likely to be extremely limited.

Emphasis on a personality type undoubtedly flows from the same orientation which produced the "great dichotomy."[47] If tradition and modernity are distinctly different, then it is not unreasonable to expect basic differences in "traditional" and "modern" men. Recent criticism of this position has, in fact, cast doubt on the argument that the nature of change is subject to some fundamental dichotomization.[48] It is equally plausible to argue that there is no difference, and that the process of social change is inextricably linked to the *process* of individual identity definition. Process is emphasized to reiterate the concern with the manner in which individuals develop personal identities, not with the particular identities which develop.[49] What follows is an attempt at a partial explanation of that linkage, using a very limited number of factors.

We may begin by examining Erikson's definition of the process of identity formation.

> . . . in psychological terms, identity formation employs a process of simultaneous reflection and observation, a process taking place on all levels of mental functioning, by which the individual judges himself in the light of what he perceives to be the way in which others judge him in comparison to themselves and to a typology significant to them; while he judges their way of judging him in the light of how he perceives himself

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[40] Robert T. Holt and John E. Turner, *The Political Basis of Economic Development: An Exploration in Comparative Political Analysis* (Princeton: D. Van Nostrand Company, 1966).

[41] Lucian W. Pye, *Politics, Personality and Nation-Building: Burma's Search for Identity* (New Haven: Yale University Press, 1962).

[42] Hagen, *On the Theory of Social Change*, pp. 123–160.

[43] Holt and Turner, p. 31.

[44] *Ibid.*, p. 30.

[45] Hagen, *On the Theory of Social Change*, pp. 141–143.

[46] Merton, pp. 134–140.

[47] Huntington. "The Change to Change," pp. 287–298.

[48] Reinhard Bendix, "Tradition and Modernity Reconsidered" *Comparative Studies in Society and History*, 9 (April, 1967), 313–14. See also Lloyd I. Rudolph and Susanne Hoeber Rudolph, *The Modernity of Tradition: Political Development in India* (Chicago: The University of Chicago Press, 1967), pp. 3–14.

[49] It may be useful to make a distinction between rational anxiety, uncertainty and concern produced by a rapidly changing social situation, and irrational anxiety, uncertainty and concern produced by individual misperception of cues understood by the majority of individuals. Our concern focuses on the effect of rational anxiety on the identity definition process, as this anxiety results from and is reflected in rapid social change.

in comparison to them and to the types that have become relevant to him.[50]

There are two significant aspects of Erikson's definition; the interactive process of mutual judgment, and the typology which establishes the context of that judgment. Mutual judgment implies a mutual conception of the relevant typologies employed, and at least to some extent, mutuality of the typologies themselves. The more alien the typology employed by a "significant other," the more likely judgment is to proceed in terms of our own typology.

In Erikson's argument, this typology includes a number of attributes, perhaps the most significant being the "cultural consolidation" developed in response to the dominant techniques of the society.

> In every technology and every historical period there are types of individuals who (properly brought up) can combine the dominant techniques with their identity development, and *become* what they *do*. Independently of minor superiorities or inferiorities, they can settle on that *cultural consolidation* which secures them what joint verification and what transitory salvation lies in doing things together and in doing them right . . . . The point is that only such consolidation offers the coordinates for the range of a period's identity formations . . . [51]

The psychoanalytic concern with the development of the self, the individual's sense of "who he is," is inextricably linked to a broader conception of "what he is." For the individual, who he is, and what he is, are in the eyes of his judges, largely inseparable.[52] Identity formation proceeds as an internalization of the judgment of "what he is" in the eyes of others. To combine psychoanalytic and sociological terminology, the individual's conception of self is bounded by the range of life-chances available to him.

The process of social mobilization is often described in terms of the roles available to the individual, and the destruction of old roles and their normative supports.[53] A general theoretical difficulty with social mobilization as a concept is the consistent implication that there is a movement "from" traditional "to" modern behavior patterns. In terms of our concern, the implication is that a more "modern," and relatively stable, identity is waiting for the mobilized individual.[54] It is also agreed, however, that the possibilities for individual identity definition are alterable by virtue of changes in the range of life chances available to him. A not so obvious corollary is that an increase in the rate of change in the spectrum of life chances available will result in a change in the very nature of the process of identity definition.[55] What I am suggesting is that the possibilities for a "cultural consolidation" decrease as the rate of change in the spectrum of life-chances accelerates.

The existence of a typology, or cultural consolidation, would give the individual a certain independence in the process of identity formation. Since the individual is not only judging himself as others judge him, but also judging their judgment in terms of the typology, he must accept or reject their view of him depending upon their comparative standing in terms of the typology.[56]

In a rapidly industrializing society, however, economic development results in a hasty multiplication and differentiation of available roles. As the importance of new roles increases, the utility and meaning of older roles disappears. With the erosion of these older roles, the individual loses any temporally extended anchor for personal identity. In the absence of past referents, identity definition more and more entails comparison of present-oriented symbols and signs, without the filtering effect provided by reference to a previously internalized cultural consolidation. As the rate of change in the number and range of life-chances increases, the ability of the individual to discriminate among the judgments of others decreases, because of a lack of standards by which to judge the judges.[57]

In short, rather than presaging the existence of a "modern" cultural consolidation, social mobilization actually renders the possibility of such a consolidation nil. Instead, the value of external symbols increases. Unable to judge in terms of an agreed typology, individuals are compelled more and more to judge in terms of more easily visible signs of status, such as place of residence, group affiliations and forms of leisure. As the significance of the earlier cultural consolidation recedes, the identity definition process is increasingly externalized, precipitating the individual into increasing competition. He is compelled to compete for the external symbols because the external symbols now constitute the basis for discriminating the judgments of others, and the basis for comparison of self to others.[58]

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[50] Erik H. Erikson, *Identity: Youth and Crisis* (New York: Norton, 1968), p. 22.

[51] *Ibid.*, pp. 31–32.

[52] Talcott Parsons has suggested that Freud's later work also demonstrated increasing concern with social stress on the development of the ego. See Parsons, "Social Structure and the Development of Personality: Freud's Contribution to the Integration of Psychology and Sociology," *Psychiatry*, 21 (1958), 321–340.

[53] Deutsch, "Social Mobilization and Political Development," p. 496.

[54] *Ibid.*, pp. 496–498; Lerner, *The Passing of Traditional Society;* Inkeles, "Participant Citizenship."

[55] Dahrendorf has argued that the changing nature of identity referents suggested by Riesman was actually argued much earlier by Tocqueville; see "Democracy without Liberty: An Essay on Other-Directed Man," in *Culture and Social Character: The Work of David Riesman Reviewed*, ed. Seymour Martin Lipset and Leo Lowenthal (New York: Free Press, 1961), pp. 175–206.

[56] Erikson, pp. 22–24.

[57] Klapp (*Collective Search for Identity*, pp. 73–116) pursues this analysis in terms of not only physical symbols but also patterns of leisure, movement membership, etc.

What occurs then may be understood as a transformation of meaning. Under a pre-existing cultural consolidation, certain symbols were understood to attach to particular roles. As the previous cultural consolidation melts under the pressure of accelerating economic change, the meaning system relating status and symbols is reversed. Instead of status implying symbols, symbols come to imply status. Possession of the appropriate symbols becomes the most significant aspect of the quest for identity.[59]

Obviously, the meaning system of any society may be drastically altered by many forms of social disruption such as war or trade expansion. As sources of social change, however, war, conquest, or trade expansion must at some point cease, and at that time a new cultural consolidation is possible.[60] Industrialization, on the other hand, is not only a source of social change, it is also a continuing productive process which reinforces itself. The changes produced by industrialization render the old cultural consolidation obsolete, and identity formation becomes externalized. Individual identity definition by reference to external symbols increases, and possession of the symbols is most easily achieved by involvement in the productive process. Involvement of greater numbers of individuals in the process further decreases the utility of the previous cultural consolidation for those individuals not yet involved, and the cycle continues. Not only does the dominant mode of identity definition become externalized, it also becomes constantly shifting, as the basis of comparison, the agreed valuation of symbols, constantly shifts. As a technological-economic process, industrialism produces a steady flow of new roles and new symbols, increasing the tentativeness of the individual's identity.[61]

Since the meaning imputed to a particular set of symbols is no longer derived from an accepted cultural consolidation, the shared meaning must be based on current agreement, and is open to change simply because some portion of the populace chooses to disagree. Social sanctions which would have been available to penalize a deviant individual or group under the old cultural consolidation are no longer useful in an effort to compel agreement. This uncertainty of valuation also increases the uncertainty of the individual's identity definition, since he is confronted not only with a flow of new roles and symbols, but also with unreliable valuations of older roles and symbols.[62] This tentative quality of individual identity has, in turn, both personal and social consequences.

Our earlier discussion of the various ratios detailing social tension may be reviewed with a different emphasis. In effect, the ratios in the form:

$$\text{Alienation} = \frac{\text{Institutionalized means}}{\text{Cultural Goals}}$$

may be comprehended as partial descriptions of the stratification system. For our purpose, the most significant aspects of a stratification system are the distribution of valued goods, the "proper" form of social mobility, and the normative justification for that definition of propriety. In a strictly economic sense, the process of industrialization operates directly on two elements of the stratification system: the extant distribution of life chances, and the form of social mobility. Increasing economic productivity offers alternatives to each of these elements. As the number of alternatives increases, changes in the normative supports are necessary. An increasing rate of change in the number of alternative life chances however, results in an increasing lag in the relationship between the extant distribution and its normative justification.

A norm is a synthetic link between act and consequence.[63] Formation of a synthesis by custom becomes more difficult as the rate of expansion of available acts increases. Custom implies shared experience, sentiment, and meaning, but the rate of change in the number and kind of available roles makes such sharing impossible. In this context the significance of media development appears more clearly. Media growth is significant not only for its functional utility in the transmission of "facts," but for the generalization of meaning. Mass media enable widespread sharing of the meaning to be attached to new symbols and roles.

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[58] Riesman pursues a similar argument in *The Lonely Crowd,* pp. 11–36.

[59] The changing use of symbols had been paralleled by changes in the myth system underlying the symbols; see Richard Weiss, *The American Myth of Success: From Horatio Alger to Norman Vincent Peale* (New York: Basic Books, 1969).

[60] The ultimate cessation of other sources of change was the explicit reason behind Hagen's postulation of a personality type—the innovative individual. Hagen, *Theory of Social Change,* pp. 12–16.

[61] In our literature the problem is perhaps best illustrated by Babbitt's encounter with a cigar-lighter as a new means of self-definition. Sinclair Lewis, *Babbitt* (New York: Harcourt, Brace, 1922), pp. 52–57.

[62] Such uncertainty is one of the major results of competitive consumption. Veblen, *The Theory of the Leisure Class;* and Lenski, *Power and Privilege.*

[63] Emile Durkheim's formulation of a norm most clearly illustrates the process of translation from social to political legitimation. See *Sociology and Philosophy,* D. F. Pocock, trans. (Glencoe, Illinois: The Free Press, 1953), pp. 40–46.

The identity definition process creates pressure for some justification for the distribution of newly available symbols, but the process of norm formation is not able to respond with the necessary rapidity. Hence there is an externalization of the process of sanctioning the newly developing distribution from a customary synthesis to a legal synthesis.[64] In short, the legitimation of a distribution of newly available life chances is impossible by customary means, and the process of legitimation is politicized.

In this environment education serves two distinct functions; it enables the individual to compete for possession of significant symbols within the new productive process, and it predisposes the individual to a belief that he is competent—at least to a minimal degree—to influence the political system. The motive force for participation comes from a need to protect new distributions of valued goods, or to acquire access and protect new modes of acquiring valued goods.

I am, of course, not arguing that individuals perceive the political process as a means of protecting their modes of identity definition. Anxiety over identity as it inheres in the possession of status symbols and mobility opportunities however, is the primary motivation for intense politicization of conflict over possession of those symbols and opportunities, and this anxiety persists long after there need be any objective concern for the means of subsistence. It therefore makes sense to assert, as does Inkeles, that participant citizenship—political mobilization—is not just a characteristic of democracy, but is, in fact, compelled by the process of "modernization" itself.[65]

Conclusion

In his massive review of the state of political change theory, Huntington suggested that a major gap in that effort was the lack of any model for the process of change in an industrialized, western nation. This research has provided a first attempt to fill that gap.

A model of the process of mobilization in the United States is perhaps most significant because it is, in a sense, unique.[66] In a society lacking any traditionally legitimate stratification system, we may observe the effect of the process of economic change in its rawest form. It is therefore unlikely that many other societies will follow the same path as the United States, but therein lies the utility of using the United States as a model. If this is, in fact, the impact of economic development in a society lacking strong traditional structures, we may attempt to develop adequate models of change in other societies by understanding them as a conjunction of a general process and particular structures. This research has, in a limited way, attempted to clarify the nature of that general process.

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[64] Max Weber, *From Max Weber: Essays in Sociology*, ed. H. H. Gerth and C. W. Mills (New York: Oxford University Press, 1946), pp. 224–229.

[65] Inkeles, "Participant Citizenship," pp. 1122–1123.

[66] Louis Hartz, *The Liberal Tradition in America: An Interpretation of American Political Thought Since the Revolution* (New York: Harcourt, Brace, 1955); Alexis De Tocqueville, *Democracy in America* (New York: Edward Walker, 1847).

Institutionalization, the Ombudsman, and Bureaucracy*

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An Authority-Based Conception of Institutionalization

This article focuses upon one principal aspect of the process of institutionalization, upon the political structure's relationship with its environment. The other major thrust of the institutionalization literature is upon internal structural integration. Nelson Polsby exemplifies this thrust in his study of the institutionalization of the House of Representatives in which he states: "Over the life span of this institution, it has become perceptibly more bounded, more complex, and more universalistic and automatic in its internal decision making."[1] Additionally, of Samuel Huntington's four criteria of institutionalization, two are internal: complexity and coherence.[2] Such measures of organizations' internal structures and capabilities clearly are relevant, particularly since large, intrinsically important structures including legislatures, political parties, bureaucracies, and armies are the usual bodies that are analyzed. These two criteria, however, constitute only a limited perspective for the analysis of the process of institutionalization. Organization-environmental relationships are the focus of Huntington's two other criteria: adaptability (a function of chronological age, generational leadership succession, and ability to take on new functions while discarding obsolete ones) and autonomy ("the development of political organizations and procedures that are not simply expressions of the interests of particular social groups"[3]). If when in addition to internal complexity and coherence, organizations attain high degrees of adaptability and autonomy in the face of environmental challenges, then they are said to be institutionalized.

Implicit in these criteria of institutionalization are various definitions of the phenomenon. The most influential and common intradisciplinary definition is sociologist Philip Selznick's statement that to institutionalize is "to *infuse with value* beyond the technical requirements of the task at hand."[4] Among those studying institutionalization, political scientists are most likely to know Huntington's definition: "Institutionalization is the process by which organizations acquire value and stability."[5] Notions of institutionalization based upon stability and value obviously are useful for many purposes. They are, however, insufficiently political in conception for this paper.

I view the organization (the incipient institution) as a behaving entity or actor in an environment populated with other potentially competitive actors.[6] Not only must an organization *defend* itself against its environment (Huntington's adaptability and autonomy), but also it must have an *offensive* capability. It must carry out its mission, a programmatic goal more demanding than mere survival, as well as having an impact upon that environment.[7] *Institutionalization is a process*

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* This article is revised from a part of a larger work, "The International Transfer of Political Institutions: A Behavioral Analysis of the New Zealand Ombudsman" (Ph.D. dissertation, Tulane University, 1970), which was supported by a Fulbright Fellowship to New Zealand. My greatest debt is to Sir Guy Powles, the New Zealand Ombudsman, who has contributed so much to his own investigation. Thanks are also due to Laurie B. Brown for his assistance with coding and to William B. Gwyn, Stanley V. Anderson, Ralph Braibanti, Karl A. Friedmann, Susan Hill, Robert L. Lineberry, Hugh G. MacNiven, John E. Moore, Charldean Newell, and Alan J. Wyner for their useful comments on earlier drafts and to Oliver Benson and Samuel A. Kirkpatrick for their methodological advice.

[1] Nelson W. Polsby, "The Institutionalization of the U. S. House of Representatives," *American Political Science Review*, 62 (March, 1968), 145.

[2] Samuel P. Huntington, *Political Order in Changing Societies* (New Haven: Yale University Press, 1969), p. 12.

[3] *Ibid.*, p. 20. It should be noted that Polsby's first criterion, well-boundedness or environmental differentiation, is not strictly internal; it is related to Huntington's environmental concept of autonomy.

[4] Philip Selznick, *Leadership in Administration: A Sociological Interpretation* (New York: Harper and Row, 1957), p. 17; Selznick's italics. On page 40 Selznick elaborates and indicates that organizations which are infused with value become "prized not as tools alone but as sources of direct personal gratification and vehicles of group integrity."

[5] Huntington, *Political Order*, p. 12; he cites Selznick at p. 15.

[6] As S. N. Eisenstadt has observed (*Essays on Comparative Institutions*, New York: John Wiley, 1965, p. 187): "From the very beginning, any bureaucratic organization is put in what may be called a power situation in relation to its environment and has to generate processes of power on its own behalf."

[7] This perspective is generally consistent with the "institution building" strand of literature, whose major theoretician has been Milton Esman. *Influence*, the extent to which the organization affects decisions in its functional area and its ability to enlarge its sphere of action, and *impact* or spread-effect, meaning the degree to which its values and actions be-

that occurs over time in which the organization creates authority relationships vis-à-vis the environmental actors. That is, the organization attains a level of internal structural integration sufficient for the performance of its other functions; it manages to cope with any depredations from the environment; and through the exercise of whatever resources of power and influence are available, it succeeds in establishing itself as an authority figure.[8]

This is not to imply that the successful organization, the institution, exercises anything like sovereignty over its environment. Minimally, it does mean that the legitimacy of its existence is acknowledged. Furthermore, the institution has created the expectation among the environment's members that its actions and demands will fall within the boundaries of a shared normative framework, so that predispositions toward at least negotiated compliance are established. Institutionalization, thus defined, can be measured in various ways. The approach taken here is to examine the behavioral interactions, linkages, or exchanges,[9] between an organization—in this case the New Zealand ombudsman—and the actors in its environment—primarily the bureaucratic actors. From those indicators, inferences are drawn concerning the strength of authority relationships and, hence, of political institutionalization.

The Ombudsman: Candidate for Institutionalization

Both internationally and in the United States, attention increasingly focuses upon the originally Swedish institution of ombudsman as a bureaucratic control mechanism.[10] There is even a worldwide ombudsman movement[11] which has reached such proportions that it often has been labeled as ombudsmania. A key to understanding the ombudsman's popularity is its relative simplicity; "grievance-man," "mediator," and "citizens' defender" are commonly used and not inaccurate synonyms. The 1972 edition of the *Encyclopaedia Britannica*, which offers a more structural definition, states that an ombudsman "is a legislative commissioner for the investigation of citizens'

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come normative within the environment, are the labels given the two major criteria. Milton J. Esman and Hans C. Blaise, *Institution Building Research: The Guiding Concepts* (Pittsburgh: University of Pittsburgh, Graduate School of Public and International Affairs, 1966), pp. 6–7.

That paper was a product of the Inter-University Research Program in Institution Building (Indiana, Michigan State, Pittsburgh, and Syracuse Universities are the program's members). Most of its papers have been distributed in mimeographed form, but two collections have been published: Joseph W. Eaton, ed., *Institution Building and Development: From Concepts to Application* (Beverly Hills: Sage Publications, 1972); and D. Woods Thomas, Harry R. Potter, William L. Miller, and Adrian F. Aveni, eds., *Institution Building: A Model for Applied Social Change* (Cambridge, Mass.: Schenkman Publishing Co., 1972). Esman's indebtedness to Selznick's conception of institutionalization is acknowledged in "The Elements of Institution Building," in Eaton, p. 28.

[8] The present focus upon the organization's offensive capability parallels Ralph Braibanti's discussion of "permeative dynamism" and the "propelled rediffusion of norms from one institution to others." "External Inducement of Political-Administrative Development: An Institutional Study," in Ralph Braibanti, ed., *Political and Administrative Development* (Durham, N.C.: Duke University Press, 1969), p. 55.

[9] Esman discusses linkages and transactions in "The Elements of Institution Building," pp. 28–34. For an interpretation of institutionalization as an exchange process see Eisenstadt, *Essays on Comparative Institutions*, chapter 1, part 1.

[10] On the national level the ombudsman has been transferred not only throughout Scandinavia and to New Zealand, but also to Great Britain, Guyana, Tanzania, Northern Ireland, Israel, Mauritius, France, and Fiji. New Constitutions in Bangladesh, Greece, Pakistan, and the Philippines provide for the office. Also, West Germany has a military ombudsman, and Canada has a "Commissioner of Official Languages." On the sub-national level, three American states (Hawaii, Nebraska, and Iowa), six Canadian provinces (Alberta, New Brunswick, Quebec, Manitoba, Nova Scotia, and Saskatchewan), and three Australian states (Western Australia, South Australia, and Victoria) have ombudsmen. Such cities as Zurich, Switzerland; Jerusalem and Tel Aviv, Israel; Dayton, Ohio; Seattle, Washington; San Jose, California; and Kansas City, Missouri have municipal ombudsmen. At least one hundred ombudsmen exist on the campuses of American colleges and universities.

Merely keeping track of the plethora of ombudsman proposals has become a major undertaking. The Ombudsman Committee of the American Bar Association, which has taken on that chore, reported in 1973 news about ombudsmen of various types in ninety different non-American jurisdictions, mainly national ones. In addition to several proposals at the American federal level, by 1973 ombudsman bills had been introduced into the legislatures of all but eight of the American states. Bernard Frank, "American Bar Association Section of Administrative Law, Ombudsman Committee Development Report, July 1, 1972–June 30, 1973," Annual Reports of Committees: Section of Administrative Law, American Bar Association Vol. 10 (1973).

[11] See Larry B. Hill, "International Transfer of the Ombudsman," in Richard L. Merritt, ed., *Communication in International Politics* (Urbana: University of Illinois Press, 1972), pp. 295–317. The volume edited by Donald C. Rowat, *The Ombudsman: Citizens' Defender* (Toronto: University of Toronto Press, and London: Allen and Unwin, 1965), has been an important document in the ombudsman movement. Two other such documents are the papers presented in 1967 at Columbia University's American Assembly, which are collected in *Ombudsmen for American Government?* (Englewood Cliffs, N.J.: Prentice-Hall, 1968), ed. Stanley V. Anderson; and the papers in "The Ombudsman or Citizens' Defender: A Modern Institution," ed. by Roy V. Peel, *The Annals,* 377 (May, 1968).

complaints of bureaucratic abuse."[12] In developing a more comprehensive definition, I have isolated several defining characteristics:[13] the classical ombudsman is (1) legally established, (2) functionally autonomous, (3) external to the administration, (4) operationally independent of both the legislature and the executive, (5) specialist, (6) expert, (7) nonpartisan, (8) normatively universalistic, (9) client-centered but not anti-administration, and (10) both popularly accessible and visible. The institution's mission is to generate *complaints against government administration*, to use its extensive powers of investigation in performing a postdecision administrative audit, to form judgments which criticize or vindicate administrators, and to report publicly its findings and recommendations but not to change administrative decisions. Indeed, one of the institution's most interesting puzzles is its apparent effectiveness despite minimal coercive capabilities.

The extensive popular and (mainly normative) academic interest[14] establishes the ombudsman as an appropriate subject of policy analysis, but little empirical research yet has been undertaken. Such research often has been discouraged by the new ombudsmen's legal secrecy requirements and by their sometimes excessively cautious estimations of the sensitivity of materials in their files. This article is the first empirically based, extended treatment of the inner workings of an ombudsman as political actor.[15] In 1962 the New Zealand official became the first extra-Scandinavian ombudsman, and his success provided the initial evidence that the institution was not dependent upon a uniquely Scandinavian constellation of cultural and political factors. Since then, the New Zealand institution has become the primary model for the world's newer ombudsmen. Because Sir Guy Powles has become the model ombudsman and because of his relative longevity in office, which provides necessary historical perspective, it is appropriate to select this first Anglo-Saxon ombudsman for detailed study. Concluding comparisons also are drawn with other national ombudsmen and with the Hawaiian official.

The New Zealand materials were collected during nineteen months of fieldwork in the ombudsman's office. This article's data were generated from a content analysis of a sample of his cases. The sample was chronologically based and divided into nine time periods. Between September, 1962, and March, 1969, 4,280 complaints were lodged. Eleven per cent of the cases, 450 complaints, constitutes the sample, which tests revealed was highly representative of the universe (see footnote 21).

New Zealand's Parliamentary Commissioner (Ombudsman) Act, which demarcates the organization's formal boundaries and specifically places the major government departments under its jurisdiction, provides a legal foundation for the office's authority.[16] Within these boundaries, the ombudsman is given extensive latitude to carry out investigations of any departmental decision, recommendation, act, or omission "relating to a matter of administration." Most important, he is guaranteed access to departmental files and records. The Act's specification of the ombudsman's domain appears hyperbolic: he may investigate where the action complained of appears to be illegal, mistaken, unreasonable, unjust, oppressive, discriminatory, or even "wrong." Although the ombudsman cannot change administrative decisions, the moral force behind his recommendations gives him sufficient authority so that potentially he could disrupt the administrative process. Alternatively, through timidity or being co-opted by the administration, the role of New Zealand's ombudsman could atrophy.

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[12] Stanley V. Anderson, "Ombudsman," *Encyclopaedia Britannica*, 1972, XVI, 960.

[13] For an explication of the following conception of the ombudsman, see Larry B. Hill, *Ombudsmen, Bureaucracy, and Democracy* (New York: Oxford University Press, forthcoming 1975), chapter 1. See also Donald C. Rowat's rather legalistic listing of the ombudsman's "essential features." "Preface to Second Edition," in *The Ombudsman: Citizens' Defender*, rev. ed., ed. D. C. Rowat (Toronto: University of Toronto Press, 1968), p. xxiv; and Stanley V. Anderson's functionally based "essential characteristics" in *Ombudsman Papers: American Experience and Proposals* (Berkeley: University of California, Institute of Governmental Studies, 1969), pp. 3–4.

[14] A perusal of even the titles of the rapidly expanding ombudsman literature clearly reveals its normative thrust. Counting only the items from scholarly or semi-scholarly sources, Stanley Anderson's 1969 bibliography contains nearly 250 listings, and it now is quite incomplete. See *Ombudsman Papers*, pp. 381–407. The most influential early popularizing article was Henry J. Abraham, "A Peoples' Watchdog Against Abuse of Power," *Public Administration Review* 20 (Summer, 1960), 152–57.

[15] The inaccessibility of files for extended research does not, of course, mean that no valuable work can be done. The ombudsmen themselves provide useful data in their annual reports, and they often have cooperated with scholars by granting interviews and sometimes providing additional statistical data. The classic comparative study remains Walter Gellhorn's *Ombudsmen and Others: Citizens' Protectors in Nine Countries* (Cambridge, Mass.: Harvard University Press, 1966). Britain's Parliamentary Commissioner, who took office after Gellhorn's book was completed, is analyzed in William B. Gwyn, "The British PCA: 'Ombudsman or Ombudsmouse?'," *Journal of Politics* 35 (February, 1973), 45–69.

The University of California's Ombudsman Activities Project, sponsored by the Office of Economic Opportunity (Stanley V. Anderson Principal Investigator), which has provided technical assistance to the new American ombudsmen, is not hampered by such research limitations. See especially John E. Moore's forthcoming monograph on the Hawaiian ombudsman.

[16] Complaints relating to the armed services, those within the jurisdiction of a court or an administrative tribunal, and complaints against the Public Trustee and the Crown Law Office are specifically excluded. Implicitly excluded are complaints against Cabinet ministers' decisions (although departmental recommendations to them are included), complaints against government corporations, and most complaints against local authorities. Inmates of institutions may submit sealed complaints. For overviews of the New Zealand ombudsman see Gellhorn, Chapter 3; and Larry B. Hill, "The New Zealand Ombudsman's Authority System," *Political Science* 20 (September, 1968), 40–51.

In pursuing the topic of the ombudsman's institutionalization through a sociometric analysis of his interaction with bureaucracy over time, this article investigates four questions: How extensive is the ombudsman's interaction with the bureaucracy? How consequential for the government department is the threat posed by complaints? What demands does the ombudsman make upon the agency? How cooperative is the department in responding? Their answers also will illuminate the public policy question of what kind of governmental device an ombudsman actually is.

Extensiveness of Ombudsman-Departmental Interaction

Because a common objection to the ombudsman institution is that it would create a large additional workload for already overburdened agencies, a preliminary step in interpreting the relationship between the ombudsman and the agencies was to measure the frequency and mode of their interaction. After receiving a complaint, the initiative lies with the ombudsman. He can have no impact upon the bureaucracy unless he decides to investigate; thereupon he must make important strategic decisions about how to conduct the investigation and about whom to involve.

Targets of Complaint. Table 1 indicates that more than three-quarters of the ombudsman's complaints processed through the 1973 reporting period were against fourteen major agencies of government. Only five departments (Treasury, External Affairs, Defence, Internal Affairs, and Agriculture) which traditionally are considered major are missing from that list. However important the functions performed by them are, they do not regularly come into contact with large numbers of individual citizens (New Zealand farmers are not subsidized). The major targets of complaint are the primary institutions of New Zealand's highly developed welfare state, the institutions which most often interact with citizens.[17] Although it is impossible to determine how many complaints "should" have been made against particular departments during a decade, the numbers actually lodged seem quite small. The multifarious activities of all of these departments require tens of thousands of yearly contacts with clients of one type or another; each transaction is a potential complaint to the ombudsman.

Incidence of Investigation. According to Table 1, the ombudsman has investigated about three-fifths of the complaints within his jurisdiction. One might speculate that as he gained experience he would investigate fewer and fewer complaints, but according to the data in his reports, this has not occurred. Although there have been some fluctuations in the annual proportions of cases investigated, the deviations are minor and there is no secular trend. When investigation is defined slightly differently as I do in the sample of cases, however, the incidence of investigation *has* fallen somewhat from 72 per cent in the first time period to 56, 50, 58, 32, 38, 44, 46, and for the last period 46. This definition is a behavioral one; if the Ombudsman queried the department about the case, then it was investigated. He sometimes tentatively began an investigation and then discontinued it for various reasons, but nevertheless labelled the case "not investigated." It is because my definition of investigation and the Ombudsman's are not exactly the same that comparisons of the actual complaints with the sample are not made in Table 1. The definitions, however, are quite similar, and 62.8 per cent ($n = 221$) of the sample cases were investigated compared with 60.1 per cent for the actual cases as of 1969. Since interaction with departments took place only on those cases which were investigated, 221 becomes the n for subsequent discussion of the findings from the sample.

As expected, cases against the major complaint departments constitute a predominant proportion of those which the ombudsman investigated. Social Welfare's incidence of complaints investigated is artificially high because it has no separate appeal agency, whereas Inland Revenue's is low due to the Tax Court's displacement of the ombudsman's jurisdiction. Otherwise there are no particular surprises in the departmental percentages of complaints which the ombudsman has deemed worthy of investigation. Certainly, the fact that—excepting Social Welfare's special circumstances—over a decade the ombudsman investigated a total of no more than 340 cases against any one department indicates in a gross

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[17] New Zealand's percentage of working age population employed by its government agencies and enterprises is the world's highest. See Bruce Russett, ed., *World Handbook of Political and Social Indicators* (New Haven: Yale University Press, 1964), p. 70.

Table 1. Departmental Targets of Complaint and Investigation: 1 October 1962 to 31 March 1973

Department	Complaints Processed		Complaints Investigated		Target's Per Cent of Processed Complaints Investigated
	n	%[a]	*n*	%[b]	
Total Major Investigative Targets	5,360	78.4	3,234	81.3	60.3
Social Welfare	1,154	16.9	837	21.0	72.5
Education	581	8.5	340	8.5	58.5
Justice	543	7.9	286	7.2	52.7
Inland Revenue	432	6.3	168	4.2	38.9
State Services Commission	342	5.0	242	6.1	68.8
Post Office	313	4.6	188	4.7	60.1
Health	308	4.5	183	4.6	59.4
Police	290	4.2	147	3.7	50.7
Labour	286	4.2	158	4.0	55.2
Customs	279	4.1	174	4.4	62.4
State Advances	262	3.8	169	4.2	64.5
Works	240	3.5	123	3.1	51.3
Railways	171	2.5	107	2.7	62.6
Government Superannuation Board	159	2.3	112	2.8	70.4
Total Other Targets	1,476	21.6	745	18.7	50.5
Total	6,836	100	3,979	100	58.2

Compiled from *Report of the Ombudsman for the Year Ended 31 March 1973* (Wellington: Government Printer, 1973), Appendix B. One hundred sixty-three complaints still under action at the end of the year and 1,746 complaints against agencies outside of the ombudsman's jurisdiction are excluded.

[a] Means target's per cent of total complaints processed.

[b] Means target's per cent of total complaints investigated.

quantitative sense that the burden of his investigations has not been overpowering.

Interaction Through Correspondence. If the ombudsman's number of cases investigated does not appear to be very burdensome even for the major investigative targets, how extensive have the investigations of the individual cases been? Nearly all of his interactions with departments on particular cases have been through correspondence which he initiated. There was a long-range tendency for him to write more than a single letter, which was all he wrote in 21 per cent of the investigations; but the most common situation (36 per cent), one that remained relatively stable over time, was that in which he wrote two letters. The mean was 2.8 letters. In only 13 per cent of the cases were five or more letters sent, so the instances of a heavy input from the ombudsman were few. But such cases (nine or more ombudsman-letters were sent in 3 per cent of the investigations) could be expected to have a considerable impact upon the departments.

Although letters do vary greatly in length and in the effort that goes into them, the fact that in nearly half (48 per cent) of the cases the department wrote only a single letter in response to the ombudsman indicates that agencies may not regard responding to him as onerous. The mean number of departmental replies was 2.1 letters to the ombudsman. Truly extensive correspondence involving five or more letters from the departments was required in only 7 per cent of the investigations, and departmental responses per case exceeded nine only 1 per cent of the time. These facts confirm that departments have not been heavily burdened by the ombudsman's investigations. Also, they support an impression gained from reading the letters that departments have prepared carefully their initial response so as to preclude a request for fuller information. No secular shifts in the number of departmental responses have occurred.

Other Interaction. The ombudsman held personal interviews—all initiated by his office—with departmental officers in just 7 per cent of the cases. Most of them were with permanent heads, and all but one conference was held at the ombudsman's office. In only 13 per cent of the investigations did the ombudsman's office telephone the department. Most calls were by the ombudsman himself, primarily to department heads. There are notes of telephone conversations that were initiated by the department in 6 per cent of the cases; the real figure is probably somewhat higher.

Furthermore, despite the ombudsman's very extensive power to investigate departmental premises, in only 3 per cent of the cases did he exercise it. The circumstances then at issue usually were a construction site or disputed piece of property. Knowing of the ombudsman's legal powers, some administrators have held a fearful image of him as a crusading figure who barges into agencies looking for secret files; this image is incorrect. Although it is clear that the ombudsman's communications with the departments by means other than correspondence have been quantitatively infrequent, this is not to say that they are necessarily unimportant.

Not only was the interaction initiated by the department with the ombudsman limited, but also interaction with other actors in the course of dealing with a complaint was rare. Perhaps surprisingly, governmental officials other than the ombudsman and departments very seldom participated. Cabinet Ministers played some role in the investigation of only 5 per cent of the cases; Members of Parliament were implicated in 8 per cent.[18] Other government departments seldom participated in investigations, and there was no pattern of departments consistently being doubly involved in cases. In fact, four was the most times any given department interacted with any other; in just 4 per cent of the cases were three departments involved; and four departments were concerned in a single investigation only twice.

Pervasiveness of Complaint's Threat

Writers sometimes have provided examples of the kinds of cases which ombudsmen investigate, but these usually rather dramatic stories may be quite unrepresentative of the general run of cases.[19] Determining how great a threat those complaints which the ombudsman decides to investigate pose to agencies is central to an understanding of their experience with him.

Extent of Departmental Discretion. One matter vital to an assessment of the ombudsman-departmental relationship is whether or not the agency had been free to make a contrary decision. It emerged from the analysis that 4 per cent of the complaints were really not against a decision of a government department. An additional 1 per cent pertained to a decision made by a Minister rather than a department (New Zealand does not work very hard at maintaining the British convention that all decisions are made by Ministers). Eight per cent concerned the department's duty to enforce a law or regulation; many statutes allow no discretion—as in the computation of several taxes. Slightly more complaints really were against government policies which the departments were forced to implement. Except as they may have convinced Cabinet to adopt the policies, they cannot be held responsible. An additional 16 per cent fell into the less formal category of departmental procedures or policies, which normally were at least tacitly approved by government. The complaints arose over their content, their justice, etc.; whether or not the complainant met the established criteria was not at issue.

Only about one-third of the complaints investigated concerned matters on which the department had made discretionary decisions. The administration of any welfare state requires that huge numbers of such decisions be made—about matters such as whether Applicant *X* meets the qualification rules for Benefit *Y*—and it is not surprising that they should be the largest single occasion for complaint. Almost another third of the complaints were lumped into a miscellaneous category; often a departmental omission or failure to act was alleged. In perhaps a majority of that category of cases it was not within the province of the department to have acted differently. The finding which emerges from this analysis of the extent of the departments' discretion is that—either because they did not make the decision or for various legal or procedural reasons—in probably no more than *half* of the total complaints investigated could the department reasonably have acted to forestall the complaint. Knowing this, departments would not have been particularly concerned about the other half of the cases.

Depth of Threat. The complaints which the ombudsman forwarded did not often seriously threaten departments; in 74 per cent of the total sample only an individual case was involved. For example, questions about such matters as the interpretation of a citizen's qualification to receive a benefit would be at issue. Even these mundane matters, however, were potentially costly; 53 per cent of these cases made some financial claim upon the department. Broader matters were raised by the remaining 26 per cent; these included issues such as whether or not, according to a proper interpretation of the Act, a particular subclass of people should be entitled to a benefit.[20] Some of these cases raised for the department the specter of being forced to amend an Act or of reallocating resources. Indeed, 58 per cent of these cases involved financial claims. It was obvious from a reading of the departmental responses that such possibilities—especially prospective Treasury raids—evoked feelings of trepidation and even indignation. Although the ombudsman's complaints did not usually threaten departments beyond a single case, they have raised more far-reaching matters often enough to keep agencies acutely aware of his existence.

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[18] Larry B. Hill, *Parliament and the Ombudsman in New Zealand* (Norman: University of Oklahoma, Bureau of Government Research, 1974), pp. 90–103.

[19] For analyses of the complainants and their complaints, see Larry B. Hill, "Complaining to the Ombudsman as an Urban Phenomenon: An Analysis of the New Zealand Ombudsman's Clients," *Urban Affairs Quarterly* 8 (September, 1972), 123–27; and Larry B. Hill, "Socio-Psychological Dimensions of Complaints to Ombudsmen: A New Zealand Analysis" (paper presented at the meeting of the American Political Science Association, Chicago, Illinois), September 7–11, 1971.

Locus of Decision Making. Fifty-three per cent of the complaints investigated concerned decisions originally made at the across-the-counter level. Since the ombudsman's complaint is routed to the department head, this meant that it often was necessary for a local branch office of the department to become involved in the internal investigation. Sometimes files were sent to the head office, sometimes reports were sent, and sometimes conferences were held. A dramatic increase has occurred in the complaints made against local office decisions, while those made by the head office have decreased. This has been due to complainants deciding to skip an internal appeal to the head office as the ombudsman's existence as an alternative appeals agent became more widely known.

Despite increased work in communication, the fact that complaints were increasingly against a local office rather than headquarters actually may have tended to reduce the level of ombudsman-departmental conflict, for less threat was involved. It is easier for an agency's chief policy-makers to consider reversing a subordinate's decision than one of their own.

Focus of Responsibility. Although the ombudsman sometimes has been depicted as the scourge of civil servants, the complainants normally conceived of the department as a corporate body and focused their ire upon it. Individual employees were accused or challenged for mistakes, misbehavior, etc. in only 7 per cent of the investigations. In none of them was the matter ever carried far enough for the accused civil servant to ask to state his defense to the ombudsman as the Act provides (Section 15 [3]). What usually happened in those cases was that the department pointed out to both the complainant and the ombudsman that what had been interpreted as the capricious action of an individual civil servant was actually an action in pursuance of the law or governmental policy, etc., so that the focus of the complaint was shifted to the department as an impersonal entity. Since the ombudsman's complaints usually are potential threats to the agency as a whole rather than to individual civil servants, agencies must be quite sensitive to them.

Nature of the Ombudsman's Demands

Of course, it is the ombudsman's own actions which are crucial in this relationship. That he has little coercive ability is clearly established, but he does have discretionary power about how he shall allocate his energies and define his role. Is he, as some have supposed, a modern Don Quixote with his lance ever poised for tilts at bureaucratic windmills?

Amount of Information Provided. In nearly half of the cases the ombudsman began the investigation by writing to the department giving an extensive précis of the complainant's allegations, including long quotations from his letter, and the ombudsman's preliminary legal investigations. This practice has remained stable, but that of sending the department a copy of the client's letter, which was done in about one-third of the cases, has greatly increased. It may be that complainants have come to make fewer contentious—and potentially legally actionable—statements in their letters, so that the ombudsman has felt less hesitation in sending copies to departments; or it may be that he has decided more often than previously that it is sounder strategy or administration to send a copy. The office has no copying machine, so copies have to be individually typed. In the remaining cases the ombudsman forwarded only a bare sketch of the complaint; that procedure virtually has stopped. It was becoming usual for the department complained against to be given nearly as much information about the complaint as the ombudsman possessed. Under such circumstances, departments would have no fear that the ombudsman could be holding out information on the complaint in an attempt to trap them.

Action Demanded. Aside from the ultimate power of recommendations to Parliament, the ombudsman's most important potential threat over government departments is that he may examine any of their files. He exercised this option, however, in only one-fifth of the complaints. The ombudsman asked for the files *alone* in about half of those cases (but sometimes the files were accompanied with an unsolicited report). These were usually cases in which he was only interested in determining a factual matter that could be found easily in the file, and the practice usually should be interpreted as an attempt to relieve the department of the burden of preparing a report on an obvious matter. In the other half of the cases in which the ombudsman demanded the department's files he also required them to submit a report. Only twice did he originally ask for a report and then subsequently also ask for the files.

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[20] In about one-third of those cases the ombudsman added importance to the investigation to the extent of asking the department for an explanation of their policy.

In more than three-quarters of the cases investigated the ombudsman was satisfied merely to demand a report from the department concerned. (Even though they were not requested to do so, in 4 per cent of the cases the departments sent the relevant files in addition.) Thus, he generally has been satisfied with the quality of the reports received. It could have been predicted that in a first flush of crusading zeal the ombudsman might often have demanded to see the files, and that as he gained confidence in the departments' dependability, he would then tend to rely more heavily on their reports. This has not been the case, and the only trend discernible over time is that a miscellaneous category composed of situations in which interviews and telephone calls were requested, has vanished.

Ombudsman's Impact. At the conclusion of his investigation the ombudsman formally labels cases as indicated in Table 2.[21] Although three-quarters of the sample's complaints which were designated "not justified" and "discontinued" were investigated, the ombudsman's chief impact upon the departments was in those cases listed as "rectified" and "recommendation made." Adding those two categories together (10.8 per cent plus 5.0 per cent) and using the ombudsman's terminology, we find that almost 16 per cent of the sample cases investigated were "justified." Analysis revealed no important changes over time, aside from a trend for somewhat more complaints to be "withdrawn." Even among the complaints designated as "rectified," in only one-fourth of the cases did the department change a decision because of overt pressure from the ombudsman. In those cases the department bowed not to his moral suasion but to his political demands. The making of a recommendation is the ombudsman's most potent sanction. It is not surprising that, as indicated in the Table, it is seldom used and then only as a last resort. Thus, the ombudsman's quantitative coercive impact upon departments has been relatively small.

Departmental Reaction

The nature of the ombudsman institution makes it more probable that government departments will be acted upon than initiate action. This does not, however, mean that they are passive. The bureaucracy might use "sandbagging" or other obstructive strategies to increase the difficulty of the ombudsman's job; thus, an analysis of the department's reactions to the ombudsman's investigations is essential to an understanding of their relationship.

Speed of Departmental Reply. Departments generally have accorded the ombudsman's complaints high priority. *On almost one-third of them the ombudsman received a substantive reply within one week.* (Interim replies merely acknowledging receipt of the complaint were not counted.) Nearly as many more were received in the second week, and less than one-tenth took more than a month. Taking into account the large volume of paper work that government departments must process, the common occurrence of crises, the fact that reports from subsidiary offices were so often required, and the complex nature of many complaints, the replies seem quite prompt. Also, there is a trend for complaints to be dealt with more quickly over time. As further evidence of the alacrity of departmental response, in only 7 per cent of the complaints did the ombudsman send a prompter to the department; and in only one case was it necessary to send a second prompter.

Contested Jurisdiction. There is a high rate of agreement between the ombudsman and the departments upon his jurisdictional boundaries. In only three cases did the department contest his jurisdiction. In two of them the department convinced the ombudsman that he had no jurisdiction, and in the other he convinced them that he did. But in none of them did the department carry its disagreement to the Crown Law Office for a legal opinion to buttress its claim as the Act provides that it may. Measured by this important criterion, the relations between the ombudsman and departments generally have been amicable. Another obstacle that departments could raise would be an attempt to hide behind their Minister's authority. In only one case in the sample, however, did they involve their Minister in a case

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[21] Table 2, which deals with a most important variable, offers an opportunity for a statistical test of the sample (the sample figures are best compared with those for 1968 in parenthesis in the Table). When the frequencies for each classification which were observed in the sample are compared with the frequencies expected—because they represent the actual classifications—and the standard one-sample (or $1 \times n$) chi-square was applied, it was determined that the differences were not significant. According to the .05 criterion, the sample is representative of the universe. (one-sample $X^2 = 3.98$, d.f. $= 5$, $p. > .50$).

Table 2. Classification of Ombudsman's Decisions: 1 October 1962 to 31 March 1973

Classification	Per Cent of Actual Complaints $n=8{,}582$[a]	($n=4{,}414$)[b]	Per Cent of Complaints in Sample $n=450$	Per Cent of Sample Complaints Investigated $n=221$
Not Justified	36.6	(37.5)	38.7	61.1
Declined	32.9	(36.3)	34.4	5.6
Discontinued	12.9	(8.8)	8.4	13.1
Withdrawn	7.8	(8.5)	10.7	4.3
Rectified	7.6	(6.4)	5.3	10.8
Recommendation Made	1.5	(2.3)	2.4	5.0
Justified—No Recommendation[c]	.6	(—)	—	—
Total	100	(100)	100	100

Compiled from *Report of the Ombudsman For the Year Ended 31 March 1973* (Wellington: Government Printer, 1973), Appendices B and C.

[a] One hundred sixty-three complaints still under investigation at the end of the year and 1,746 complaints outside of the ombudsman's jurisdiction are excluded from this total.

[b] This column reports cumulative figures as of the 1969 *Report*, which is the reporting period closest to the one covered in the sample.

[c] Classification used for the first time in 1967.

when neither the ombudsman nor the complainant previously had done so. This is further evidence that the departments did not view the ombudsman as a dangerous enemy who should be opposed at every turn.

Disputed Facts. An important insight into the departments' perceptions of the ombudsman's complainants can be gained from another finding. In only 19 per cent of the investigations did they dispute a fact given to the ombudsman by the complainant. It is interesting that essential facts were at issue in half of the cases in the first time period, so that the initial conclusions of departments about the ombudsman's complainants were that they were often not truthful. Of course, it might be that the department was wrong and that it was defending its position by attacking the complainant's integrity. Its contentions usually were substantiated, however, with documentary evidence that convinced the ombudsman and the coder. Some of the facts in contention were easily verifiable, such as a date of application, but others were less so, such as a verbal promise from an officer. Thus, in 80 per cent of the cases, disputes between the department and the ombudsman were on grounds other than the facts of the complaint.

Rectification. As alluded to above, a common departmental reaction to the ombudsman's investigation was to rectify the complaint. The analysis revealed that in more than one-third of the cases which the ombudsman categorized as "discontinued" the department was already in the process of correcting the matter; the ombudsman felt his role was insufficient to warrant classifying the case as "rectified." Usually departments would have corrected a mistake, etc. had it been brought directly to their attention.

Turning to the 10.8 per cent of the sample's investigated cases categorized as "rectified," at the time of complaint the department itself was in various stages of rectifying more than one-fourth of them. In an additional half the department readily righted the matter when informed of it by the ombudsman. Thus, reversals stimulated by the ombudsman's insistence accounted for only about a quarter of the "rectified" cases.[22]

Evaluation and Comparisons

Although organizations that meet specified criteria are given the label of institution, I recommend with S. N. Eisenstadt that rather than conceiving of the phenomenon as an end point we should "talk about the *process of institutionalization* and look on it as a process of continuous crystallization of different types of norms and frameworks."[23] As Huntington has reminded us, political structures decay perhaps more commonly than they develop.[24] Because political organizations are vulnerable to so many environmental and internal vicissitudes, we cannot with confidence examine Organization *X* or *Y* at Time *Z* and certify it as truly institutionalized in the sense that it is vulcanized against further change. Nevertheless, this review of the New Zealand ombudsman's interaction with the bureaucratic actors in his environment elicits the *provisional* judgment that according to the theoretical criteria specified above, the ombudsman has become an institution. That is, not only has the ombudsman developed levels of internal structural integrity and environmental coping mechanisms adequate to the challenge,[25] but also he performs his mission well and has established himself with the environmental actors as an authority figure. A review of our findings on the policy questions posed in the introduction will assist in evaluating the New Zealand ombudsman's institutionalization. Included with this review are some brief comparative observations on the Swedish, Finnish, Danish, Norwegian, British, and Hawaiian ombudsmen.[26]

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[22] An important recent development is that increasing numbers of complaints are "rectified." Whereas in earlier years only 60 to 80 per cent of the total "justified" complaints were "rectified," the figure has been over 90 per cent since 1971.

[23] Eisenstadt, *Essays on Comparative Institutions*, p. 32.

[24] Samuel P. Huntington, "Political Development and Political Decay," *World Politics* 17 (April, 1965), 386–430.

First, the ombudsman is on target, but his interaction with the bureaucracy is not highly extensive. In keeping with the aims of many advocates, ombudsmen have become institutions for the resolution of citizens' quarrels with welfare state agencies. Even the original Swedish ombudsman (an institution that was in 1970 made tripartite) now has the resolution of such conflicts as his principal role, whereas formerly he was preoccupied with the judicial system. The volume of complaints which ombudsmen investigate does not create a consequential burden for administrative agencies; few departments would receive more than one complaint per month. The New Zealand finding that almost all interaction took place through correspondence is, however, not universal. A large proportion of the Hawaiian ombudsman's investigations utilize the telephone; feeling that letter inquiries generate "paperwork," some agencies have requested this form of interaction whenever possible. The Swedish and Norwegian ombudsmen also use the telephone much more often than their New Zealand counterpart does. Conversely, Britain's Parliamentary Commissioner very often sends a staff member to the accused agency for conferences and examination of documents. These differences are stylistic; in no case are departments inundated by work from the ombudsmen. Many bureaucratic officials may be aware of the ombudsman's legal authority, but he cannot build an actual authority relationship without interaction with them. On the other hand, the decision to involve a department in the investigation of a case must be considered carefully, for the ombudsman's credibility is at stake in each investigation.

Second, the finding that the New Zealand complaints seldom were fundamentally threatening to the department is common. Differences in legal systems and in the ombudsmen's jurisdictions make precise comparisons difficult, but it appears to be general that the bureaucracy would have had the option of deciding in favor of the citizen in only about half of the cases investigated. Also, complaints seldom have repercussions beyond a single, individual case; but the focus of complaints is almost always the department and not an individual civil servant. This is less so in Sweden where tightly drafted laws hold civil servants strictly accountable for administrative actions. The finding that complaints from the ombudsman are unlikely to pose a serious threat is positive for his authority, because a perception of threat would be likely to produce attempts at cover up rather than cooperation.

Third, ombudsmen invariably have been punctilious to avoid the disruption of the administrative process; they have treaded softly and usually have fully informed departments about details of the complaint. The Danish ombudsman normally sends the actual letter of complaint—without making a copy—and has experienced no adverse effects. The Hawaiian official even enjoys that ubiquitous American administrative accoutrement, the Xerox machine. It is not unusual for the Danish ombudsman to request the departmental files, and the other ombudsmen seem to do so more often than in New Zealand; especially in the beginning, the Hawaiian ombudsman frequently asked for the documents. But ombudsmen have typically found that they can depend upon the administrators to such an extent that their option of calling for documents seldom is exercised. It is especially difficult to compare the ombudsmen's disposition of cases, but their ability to obtain a favorable outcome for their client in about one-tenth of the cases (but in a somewhat higher per cent of the complaints investigated) is roughly comparable. Reflecting his traditional status and power to prosecute, the Swedish ombudsman is the most likely to use coercion; whereas the Finnish official—who is legally almost as powerful—seems least likely to do so. Coercion normally is avoided. The ombudsman has little real power, and his judicious use of the limited resource has been important to the office's institutionalization.

Fourth, bureaucrats have not obstructed the ombudsman. It is clear that the New Zealand official gets service that is usually twice as fast as his brethren (Finland's is the slowest; there it may take from two to three months for a reply), but cooperation is the norm. Even though an ombudsman's jurisdiction often is debatable, it is almost never contested. Denmark's is the clearest instance of bureaucratic tolerance, for the ombudsman has no legal jurisdiction unless internal appeals through the Minister have been exhausted; practically, the ombudsman simply forwards such complaints and they are almost always dealt with as if they were actionable. Willing rectification of clear mistakes as in New Zealand is also internationally normative; such cooperation is an important index of the ombudsman's institutionalization.

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[25] These matters are treated in Hill, "The International Transfer of Political Institutions," chapter VII.

[26] The data on the European ombudsmen were gathered during the summer of 1971, and the Hawaiian materials were obtained the following summer. These impressionistic observations are elaborated upon in Hill, *Ombudsmen, Bureaucracy, and Democracy.*

In comparing the various ombudsmen one is struck by how similar they are, but their environments are also similar.[27] Often the ironic comment is made that ombudsmen exist where they are least needed, and it is true that standards of public administration are quite high in all of these jurisdictions. One might even say that they enjoy an administrative civic culture. We understand the institutionalization phenomenon only imperfectly, but it might be hypothesized that the classical ombudsman could not succeed in an environment which lacked this infrastructure. Certainly, one might suspect that a corrupt bureaucracy would be unlikely to cooperate fully in its own arraignment.

Probably the congruence between the norms of the New Zealand political-administrative environment and the ombudsman's putative norms goes far to explain a salient finding: there were few changes over time in ombudsman-environmental relationships. Perhaps the most important authority enhancing development is the departments' increasing willingness to rectify complaints so that the ombudsman does not have to make formal recommendations. Although the relationships have not changed in critical ways, time may yet be an important variable. The sharing of stable interaction patterns, which seem to have been reasonably mutually satisfactory, probably has tended to create expectations of further similar experiences. The creation of such expectations enhances the ombudsman's authority.[28]

It may appear from the foregoing empirical analysis that the ombudsman accomplishes little. Certainly, he has not had a profound impact upon the workings of public administration. This may dishearten advocates, who have envisioned him as Don Quixote on a charging steed. But critics may be mollified; the ombudsman has not tilted at windmills, nor, to mix metaphors, has he thrown sand in the administrative gears. His accomplishments are not flashy, but in addition to his substantive and psychological public impact, he has affected administration in subtle ways. The ombudsman is not an administrative panacea; it is probable that those who will be most disappointed with ombudsman experiments are those who expect too much.[29]

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[27] See Hill, "International Transfer of the Ombudsman," pp. 309–312.

[28] See Larry B. Hill, "Affect and Interaction in an Ambiguous Authority Relationship: New Zealand's Bureaucrats and the Ombudsman." *Journal of Comparative Administration* 4 (May, 1972), 35–58; and Kent M. Weeks, "Public Servants in the New Zealand Ombudsman System," *Public Administration Review* 20 (November/December, 1969), 633–638.

[29] Charles S. Ascher's caveat is well taken; "The Grievance Man or Ombudsmania?" *Public Administration Review*, 27 (June, 1967), 175.

Hegel's Critique of Liberal Theories of Rights*

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In "Abstract Right," the first part of the *Philosophy of Right*, Hegel develops the rights of the person to life, liberty, and property; his right to self-defense; and his right to the wide-ranging exercise of his arbitrary freedom or free self-determination. Furthermore, in abstract rights Hegel finds the bases for what is made concrete in civil society: a capitalistic economic order ("the system of needs") and a liberal administration of justice—public, egalitarian, and characterized by due process. In "Abstract Right"—and in "Civil Society," where the concrete manifestations of abstract rights are discussed—Hegel holds, in their fullness, the central liberal positions.[1]

The Critique of the Content of Rights

But "Abstract Right" contains, usually implicitly, a critique of much liberal theory. Hegel's critique falls into two distinct categories. One major category of Hegel's critique has to do with the content of abstract rights or, as much liberal theory calls them, natural rights. Unlike most liberals, Hegel does not treat the state immediately after the discussion of the rights of man; rather, he takes a long route—through morality, the family, and civil society—before he comes to the state.

Both Hegel and Locke, for instance, explain the rights of men, and then turn to a consideration of punishment. Hegel next examines morality,[2] and then the family[3] and civil society,[4] before he finally gets to the state.[5] Locke, on the other hand, proceeds quickly to a discussion of the state.[6] Thus, Locke jumps from the natural man with his arbitrary will to the civil state which makes laws but otherwise lets man's arbitrary will have full play.[7] In other words, Locke generally assumes (or ignores) all those institutions and ways by which the natural man (or the natural child) becomes a civilized man capable of functioning well in the civil society of the *Second Treatise*.[8] But Hegel is surely correct in his argument that men plucked from the state of nature would not be able to maintain or govern a Lockean state.[9] In *On Liberty*, usually considered the classic defense of arbitrary freedom or individual freedom from social and political control, Mill is explicit in assuming—and then ignoring—the institutions and attitudes which make men civilized. *On Liberty* simply applies to the few advanced European countries where civilization can be assumed; other countries are lucky if they are ruled by a Charlemagne or the East India Company.[10]

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* I should like to thank Thomas L. Pangle and P. Gordon B. Stillman for their helpful comments on earlier drafts of this article.

[1] The best discussion of the part "Abstract Right" is Joachim Ritter, "Person und Eigentum," in his *Metaphysik und Politik* (Frankfurt am Main: Suhrkamp, 1969) and (in French) as "Personne et Propriété selon Hegel," in *Archives de Philosophie,* 31, no. 2 (Apr.-June, 1967), 179–201. Ritter does not deal with the section on wrong and its annulment, for which see my "Hegel's Idea of Punishment," *Journal of the History of Philosophy* (forthcoming, 1975).

[2] G. W. F. Hegel, *The Philosophy of Right,* trans. T. M. Knox (Oxford: Clarendon Press, 1942; corrected ed., 1945), secs. 105–140. In citations to the *Philosophy of Right,* where the material cited is from the main text of the section, the section number alone is given; where it is from the "remarks" Hegel added to the text, the section number is followed by "R"; where it is from the "additions" which later editors appended to posthumous editions by collating student lecture notes, the section number is followed by "A."

[3] *Ibid.,* secs. 158–181.

[4] *Ibid.,* secs. 182–256.

[5] *Ibid.,* secs. 260–329.

[6] John Locke, *Two Treatises of Government,* revised edition, ed. Peter Laslett (New York: Mentor Books, 1963), *Second Treatise,* secs. 4–22. (Hereafter cited as *Second Treatise.*) See also Thomas Hobbes, *Leviathan,* intro. Richard S. Peters (New York: Collier Books, 1962), Part I, esp. chap. 14 and Part II, esp. chap. 17.

[7] Locke, *Second Treatise,* sec. 22. Interpretations of Locke on the question of freedom are numerous; the interpretation given is a literal—some would say superficial—one, in which the only "development" that the individual's freedom undergoes as the individual matures is to become sophisticated arbitrary freedom—for Locke's rationality is that of the "Industrious and Rational" (sec. 34) in the sense of economic rationality, where the arbitrary free will is calculating enough to postpone an immediate satisfaction in order to obtain a larger future satisfaction. See also Adam Smith's praise of "natural liberty" in *The Wealth of Nations,* ed. Edwin Cannan (London: University Paperbacks, Methuen, 1904; reprint ed., 1961), I, 157, 344; II, 37–38, 208.

[8] The case is similar for Hobbes, who excludes education from the text proper of *Leviathan,* relegating it to the "Review, and Conclusion," pp. 503–511.

[9] See esp. G. W. F. Hegel, *Philosophy of Mind,* trans. William Wallace (Oxford: Clarendon Press, 1894), sec. 502R, and, specifically in reference to Hobbes, G. W. F. Hegel, *Lectures on the History of Philosophy,* trans. E. S. Haldane and Frances H. Simson (London: Routledge & Kegan Paul, 1896; reprint ed., 1955), III, 317–319.

[10] John Stuart Mill, *On Liberty* (New York: Library of Liberal Arts, 1956), chap. I, p. 14. In fairness to Mill and in the interests of accuracy, it must be noted that his other works consider morality,

Hegel, on the other hand, spends much—it might be said, all—of the *Philosophy of Right* dealing not only with the political order and arbitrary freedom, but also with those various attitudes (like morality) and those various institutions (like the family and the pluralistic group[11]) by which men become fully developed men who have freedom, culture, and individuality, men who can give reasoned consent to a reasonable political order, men who are mature enough to be able to exercise their freedom. At the level of formal rights, then, for Hegel there is implicitly a serious defect, of narrowness; abstract rights alone cannot form the basis for the state; more is needed, and the sections between "Abstract Right" and the "State" describe those attitudes and institutions not explicitly present in abstract rights.

For Hegel, a basic inadequacy of abstract rights, and the reason for their narrowness, is to be found in the definition of the person. A theory of rights does not deal with man; it deals with a mere abstraction from man: the person, who is defined essentially in terms of his arbitrary free will and thus his capacity for rights.[12] Consequently, for the person, morality is irrelevant; persons may do whatever they have a right to do, regardless of morality. Indeed, not only morality, but also a whole range of human concerns—about social relations, human feelings,[13] culture, welfare, and individual development—are excluded from rights. Therefore, the person exercising his rights is not an especially attractive human, at least for Hegel:

> To have no interest except in one's formal right may be pure obstinacy, often a fitting accompaniment of a cold heart and restricted sympathies. It is uncultured people who insist most on their rights, while noble minds look on other aspects of the thing.[14]

Similarly, for Hegel the contractual relation is not the only legitimate human relation; yet the person, as the subject of rights, can only exercise his rights and can only relate to others by contract.[15] Hegel insists that neither marriage nor the state can be a contract, in part because of the strict definition of contract: its object must be "a single external thing."[16] Thus, for Hegel marriage and the state are areas of life in which contract and its concomitants—like the *Versachlichung* (literally translated, "thingification") of human relations[17] and the political theory of possessive individualism[18]—do not prevail, where feeling and community may have scope, and where human, not merely personal, characteristics exist.[19]

The person is but an abstract and narrow man; and the attenuation carries over into many areas. For instance, persons are not individuals with distinctive individual traits which set them apart from each other and give them individual value; persons are not individuated. One person differs from another simply because of external differences: color of hair, height, birthplace, impressions on his *tabula rasa* mind—in short, differences over which the person has no control and which are basically the products of nature or chance.[20] Consequently, the resultant differentiation among persons is one of eccentricity[21] and idiosyncrasy rather than of substance. For fully developed men in society, there are substantial differentiations—of class and occupation, for instance. While persons can only be eccentrics, men in society are individuals.

Further, even in regard to the free will, that important area which the person shares with the man, the person is limited. For the person is, in an important sense, not free, even though he has

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the family, the economic order, and the state (the topics of the *Philosophy of Right*); only by abstracting *On Liberty* from Mill's total work can he be considered a pure defender of arbitrary freedom or "freedom from." This illegitimate abstraction has been made by some contemporary defenders of "freedom from"; for an effective, indeed devastating criticism of their position, see the excellent but overlooked article by R. N. Berki, "Political Freedom and Hegelian Metaphysics," *Political Studies*, 16, no. 3 (Sept., 1968), 365–383.

[11] Hegel, *Philosophy of Right*, secs. 255 and 255R.

[12] *Ibid.*, secs. 35–59.

[13] Hegel's handwritten marginal note [*Randbemerkung*] to sec. 126 in his own copy of G. W. F. Hegel, *Grundlinien der Philosophie des Rechts*, fourth edition, ed. Johannes Hoffmeister (Hamburg: Felix Meiner, 1955), p. 393.

[14] Hegel, *Philosophy of Right*, sec. 37A.

[15] Thus, most legitimate human relations not based on passions (like the "concord of natural lust") are contract relations in Hobbes's *Leviathan* and Locke's *Second Treatise*. Even the relation of parent and child is a contract; see Hobbes, *Leviathan*, chap. 20, p. 152.

[16] Hegel, *Philosophy of Right*, sec. 75.

[17] *Ibid.*, sec. 40; Ritter, "Person und Eigentum," sec. 7; and, among others, Karl Marx, *The Manifesto of the Communist Party* (New York: Monthly Review Press, 1968), Part I, pp. 5–6, and Edmund Burke, *Reflections on the Revolution in France* (New York: Library of Liberal Arts, 1955), p. 86.

[18] See C. B. Macpherson, *The Political Theory of Possessive Individualism* (Oxford: Oxford University Press, 1962).

[19] Hegel's distinction here, between contractual areas of life (like civil society [*die bürgerliche Gesellschaft*]) and noncontractual or communal areas of life, is the precursor of, among others, Ferdinand Tönnies, *Community and Society* [*Gemeinschaft und Gesellschaft*], trans. and ed. Charles P. Loomis (New York: Harper & Row, 1957).

[20] See G. R. G. Mure, "Foreword," in Frederick G. Weiss, *Hegel's Critique of Aristotle's Philosophy of Mind* (The Hague: Martinus Nijhoff, 1969), pp. xiii–xiv.

[21] Thus, J. S. Mill warns, "that so few now dare to be eccentric marks the chief danger of the time" (Mill, *On Liberty*, chap. III, p. 82).

formal rights and formal freedom. He has only an "arbitrary will"; it wills whatever it wants, but its contents, from which it chooses, are given.

> If you stop at the consideration that, having an arbitrary will, a man can will this or that, then of course his freedom consists in that ability. But if you keep firmly in view that the content of his willing is a given one, then he is determined thereby and in that respect at all events is free no longer.[22]

The arbitrary will is able to choose among impulses, desires, passions, and the like; but the passions or impulses among which it chooses are a content over which it has no control. The Hegelian arbitrary will is thus quite similar to freedom as conceptualized by many liberals, like Locke and Hobbes. For them,[23] freedom and determination are compatible; as long as the individual has self-determination, it does not matter that the content of his choice is given. Locke agrees with Hobbes's reasoning that

> *liberty* and *necessity* are consistent . . . in the actions which men voluntarily do: which, because they proceed from their will, proceed from *liberty;* and yet, because every act of man's will, and every desire, and inclination proceedeth from some cause, and that from another cause, in a continual chain, whose first link is the hand of God the first of all causes, proceed from *necessity.*[24]

Locke also concurs in Hobbes's further argument that

> fear and liberty are consistent; as when a man throweth his goods into the sea for *fear* the ship should sink, he doth it nevertheless very willingly, and may refuse to do it if he will: it is therefore the action of one that was *free.*[25]

For Hegel, the self-determination of the arbitrary will is an essential element of freedom; but this freedom of the person is only one element in the full definition of freedom. Another element, for instance, is control over the contents of the choice, both in the sense that the contents be at least in part determined by the individual and not by an alien causal chain and in the sense that at least one of the choices be an alternative that is desirable and rational under any circumstances. (Thus, while Hobbes and other liberals see liberty as consistent with both fear and a restricted choice, Hegel regards a choice among unappealing alternatives as only limited freedom.[26] It is not true freedom, Hegel thinks, for a traveler to have to choose between losing his goods and death by water, for a subject to have to choose between living under an oppressive tyranny and a flight that would lose him his family and property, or for Hobbes to be given the choice, in England during the Civil War, only of fighting for the Roundheads or fighting for the Cavaliers—and not allowed the alternatives of flight or peace suitable for a man "born twins" with fear[27] and desirous of civil order.) The person of abstract rights, like the man of natural rights, has freedom in the sense of the arbitrary will or of self-determination; but the content of his choice is given, and thus in significant ways he is not free.

Like the will's freedom, political freedom is defined as severely limited by those theorists who proceed from natural rights directly to the state. When the state is built solely on formal rights and natural or abstract men, political obligation is based on a consent, given long ago or sometimes given only tacitly.[28] And obedience to specific laws is based, not on the validity or the rightness of the law, but on prudential considerations of the potential penalities for disobedience.[29] On the other hand, where the laws do not reach, the citizen can do anything, and can follow his natural impulses and arbitrary will wherever they lead him.[30] In the state based on the formal rights of natural or abstract men, who exercise tacit consent, prudential obedience, and full arbitrary freedom outside the law, the citizen has little scope for moral decisions and for questions of rightness, and little area in which he must or can develop or educate himself; the education that does occur is either education that makes the citizen more "Industrious and Rational," i.e., more eager to work and better able to calculate, than he was, or education that is peripheral to life's central concerns.[31] In general, political freedom—in the sense of making responsible choices and of having an environment in which the citizen must or will wish to improve and educate himself—is simply narrow in scope.[32]

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[22] Hegel, *Philosophy of Right,* sec. 15A.

[23] Among others, James Mill agrees with Hobbes and Locke; see John Stuart Mill, *Autobiography* (London: Oxford University Press, 1924), p. 91. J. S. Mill poses the dichotomy that allows for the coexistence of political (or social) freedom and philosophical determinism at the beginning of *On Liberty;* see Mill, *On Liberty,* chap. I, p. 3.

[24] Hobbes, *Leviathan,* chap. 21, p. 160; see also John Locke, *An Essay Concerning Human Understanding* (New York: Dover, 1894; reprint ed., 1959), Book II, chap. 21, esp. secs. 14, 27, and 29.

[25] Hobbes, *Leviathan,* chap. 21, pp. 159–160; see also Locke, *An Essay Concerning Human Understanding,* Book II, chaps. 21 and 27.

[26] Hegel here agrees with Aristotle; see Aristotle *Nicomachean Ethics* 3.

[27] Of himself Hobbes wrote, "Fear and I were born twins" (quoted in Peters, "Introduction," in Hobbes, *Leviathan,* p. 7).

[28] See Locke, *Second Treatise,* secs. 73, 98, 117, and 119; Hobbes, *Leviathan,* chap. 18, p. 136.

[29] See, e.g., Hobbes, *Leviathan,* chap. 17, p. 192; Elie Halévy, *The Growth of Philosophic Radicalism* (Boston: Beacon Press, 1955), esp. pp. 487–488.

[30] Hobbes, *Leviathan,* chap. 30, pp. 255–256; Locke, *Second Treatise,* secs. 22 and 57.

[31] For education in such characteristics and traits as virtue, liberality, and steadfastness is, in a sense, external to the essential and logical structure of the *Leviathan,* and therefore is described, not in the text proper of the *Leviathan,* but in the appended dedicatory and concluding material; see Hobbes,

Even in theory, political freedom is thus limited and meager in states built on the abstract or natural man and his rights. In historical practice, however, political conditions based on the arbitrary or self-determining wills of abstractly equal persons can produce terrifying results, as was demonstrated by the French Revolution, which aimed at full liberty and equality but produced the Reign of Terror. For arbitrary freedom in political practice is the desire to reduce all limitations on itself to nought, to be fully self-determining.[33] In society, unlimited self-determination faces institutions and social differentiations that provide frameworks and modes for choice. But absolute freedom feels such guides as limits; it desires and acts to eliminate them; and the eventual result of its action is "the destruction of the whole subsisting social order":[34]

> In this absolute freedom all social ranks or classes, which are the component spiritual factors into which the whole is differentiated, are effaced and annulled; the individual consciousness that belonged to any such group and exercised its will and found its fulfillment there, has removed the barriers confining it.[35]

With the attempt to realize absolute freedom comes the terror of destruction of the social order.

The demand for absolute freedom and the social destruction accompanying its attempted actualization are concurrent with the demand for absolute equality. Absolute equality is as ferocious as absolute freedom; from the removing of all social barriers and the attempted establishing of absolute equality follows "the elimination of individuals who are objects of suspicion to any social order, and the annihilation of any organization which tries to rise again from the ruins."[36] Absolute equality assumes that equality is defined as complete sameness and complete undifferentiation; to absolute equality, any difference, any quirk, any distinction implies inequality and must be eliminated. Since any organization or institution implies difference and distinction, no institutions—not even government—are permitted by absolute equality.[37] Everyone must be the same and have the same will: this dictum of absolute equality the government tries to enforce, by executing those who are different, even if they are different only because someone suspects them of difference; similarly the people tries to enforce the dictum against the government, deposing it when it seems to be setting itself up as different in status or in policy from the people, and thus the people rapidly overturns kings, constitutions, and parties.

The result of absolute freedom and equality, for Hegel, is simply negation; fully developed negative freedom "cannot arrive at a positive accomplishment of anything, . . . either in the shape of laws and universal regulations of conscious freedom, or of deeds and works of active freedom."[38] Any institution that develops to guide individuals, any government that tries to make any particular policy, any distinctions based on merit or worth: all are precluded by absolute freedom and equality. Whenever any such differentiation is developed, absolute freedom desires it destroyed. Consequently, "universal freedom can thus produce neither a positive achievement nor a deed; there is left for it only negative action; it is merely the rage and fury of destruction."[39]

Even without the extreme (but logically derivative) absolute freedom and terror, abstract or natural rights as the bases of a political state are, for Hegel, abstract and narrow. The person, that abstraction from man, is only a narrow—and amoral, unfeeling, and static—portion of man; he is not an individual, though he may be an eccentric; he has the arbitrary freedom of self-determination without control over the content of his choices; and his political freedom is both limited and limiting. A political order based on the abstract or natural rights of man is thus based on only a restricted portion of man; a political theory based only on abstract or natural man thus leaves unconsidered and indeed places beyond consideration questions of human morality, affective human relations, human culture and cultivation, and the development of freedom. In short, a world populated solely by abstract or natural persons with abstract or natural rights is a limited, inhuman, and inhumane world.

Because of the basic inhumanity of a world

Leviathan, "Dedicatory Epistle," p. 5, and "Review, and Conclusion," pp. 503–511; see also Locke, *Second Treatise,* esp. secs. 34 and 64.

[32] The denigration of consent and obligation, and the limiting of the scope of political freedom, form two basic elements of the depoliticization inherent in much liberalism; see Sheldon S. Wolin, *Politics and Vision* (Boston: Little, Brown, 1960), chaps. 9 and 10.

[33] Hegel, *Philosophy of Right,* sec. 5R.

[34] *Ibid.,* sec. 5R.

[35] G. W. F. Hegel, *Phenomenology of Mind,* trans. J. B. Baillie (New York: Harper Torchbooks, Harper & Row, 1910; reprint ed., 1967), pp. 601–602.

[36] *Ibid.,* pp. 605–606, and Hegel, *Philosophy of Right,* secs. 5R and 5A.

[37] Hegel, *Philosophy of Right,* sec. 5A and Hegel, *Phenomenology of Mind,* pp. 605–606.

[38] Hegel, *Phenomenology of Mind,* p. 603.

[39] *Ibid.,* p. 604; see also Hegel, *Philosophy of Right,* sec. 5R. Many current interpretations follow Hegel's; see, e.g., Alfred Cobban, *In Search of Humanity* (New York: George Braziller, 1960), esp. p. 210.

populated by abstract persons, Hegel's logic drives the *Philosophy of Right* to consider more aspects of man than solely "personhood." The limited world of persons or natural men is, with Hegel, not the final word, but rather is superseded until Hegel's political philosophy deals with men and individuals, with morality and education and culture, and with freedom in its full sense and the social institutions that produce and maintain it.

The Critique of the Liberal Formulation of Rights

The other major category of Hegel's critique centers on the crucial theoretical errors inherent in the concept of natural rights.[40] The first crucial error of the natural rights theories is that men are led almost inevitably to social contracts, for the natural man's only legitimate interpersonal political relation is contract, and thus the only way that natural men can relate, to form families and civil society, is to contract. Nonetheless, it is probable that some liberal contract theorists were not entirely pleased with the implications of their contract theories.[41] Locke, for instance, clearly tries to hedge both the marriage and social contract with noncontractual limits so that the full implications of contract would not be developed. Thus, while asserting a popular right to revolution whenever the government does not act as it was bound by the social contract, Locke emphasizes the "slowness and aversion in the People to quit their old Constitutions"[42] and the people's willingness to tolerate "*great mistakes* in the ruling part, many wrong and inconvenient Laws, and all the *slips* of humane frailty . . . without mutiny or murmur."[43] He also sees his doctrine as, in practice, "*the best fence against Rebellion*."[44] Similarly, marriage is a contract which, at the same time "draws with it mutual Support, and Assistance, and a Common Interest too, as necessary . . . to . . . Care, and Affection [and] . . . to their common Offspring."[45] Locke is frequently regarded as "judicious" in doctrine and phrasing; Hegel, on the other hand, rather than adopt the Lockean hedging, is simply clear: marriage and the state are not in essence contracts.[46]

The second crucial theoretical error is to see natural or autonomous man as giving up his right to self-defense (or to punish) to the state in return for the benefits of society.[47] For, Hegel argues, at the level of the prepolitical, where there are no positive laws nor magistrates, the annulment of crime takes the form of revenge, not punishment.[48] Thus, the abstract right (and the natural right) to coerce is always revenge,[49] and individuals in a prepolitical condition have the right to revenge, not the right to punish. Clearly, then, it is a misconception to argue that men in the state of nature have a right to punish which they can transfer to the sovereign by compact. They cannot give up the right to punish, because they do not possess it; all they can give up is the right to revenge. Therefore, the liberal contract state gains, not the right to punish, but the right to revenge. As Hegel points out, however, no act of revenge is justified, because the agent is interested, arbitrary, and lacking in authority, and because any act of revenge can start an infinite chain of revenge.[50] Therefore, the liberal state has the right to revenge, a right which it ought not exercise in any specific case and a right which, if exercised, may well start a cycle of revenge and alienate some of the state's citizens for life or, indeed, for generations.[51] For Hegel, therefore, the assertion that the state obtains from its citizens the right to punish is incorrect; all that the state can obtain from natural (or abstract) men by compact is their right of revenge to annul crime in an undisinterested way to which the criminal rarely consents. To use the social contract, or the alienation of the right to punish, as the basis for the state is to commit a theoretical error.[52]

It might be noted that one possible liberal rejoinder would be to argue that the right of revenge of private individuals, when it is given to and accepted by the state, becomes the right of punishment. But this argument contains within itself an interesting presupposition that actually runs counter to the liberal position. For if the state, by accepting rights from individuals, can transform or even redefine the individuals' rights—which are the bases and origin of the state—then the state must have some existence independent of the rights granted it; that is, the state must have some basis on which or from which it is able to transform or redefine rights. This basis cannot be obtained from the contracting or consenting individuals, because the ability to transform revenge to punishment is not an ability of the individual; he cannot give to the state something he does not have himself. Consequently, since the basis from which the state redefines revenge to be punishment derives not from individuals, it must rest in the state, and therefore the state does have some existence and content independent of those individuals from whose alienated rights alone the state is supposedly composed. A liberal rejoinder, then, can end up showing what it does not want to prove: that the origin and basis of the state is not solely derived from its citizens.

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[40] Hegel's critique of the conceptualizations of natural rights is equally applicable as a critique of some liberals' equivalent of natural rights, the free and autonomous prepolitical man.

[41] Implications like the *Versachlichung* of human relations, possessive individualism, and the impermanence and easy dissolubility implied in contractual marriage and state; see note 17, above; note 18, above; Hegel, *Philosophy of Right,* secs. 75A, 161A, and 258A; and Emile Durkheim, *The Division of Labor in Society* (New York: Macmillan, 1933), pp. 203–204.

[42] Locke, *Second Treatise,* sec. 223.

[43] *Ibid.*, sec. 225.

[44] *Ibid.*, sec. 226.

[45] *Ibid.*, sec. 78.

[46] Hegel, *Philosophy of Right,* secs. 75, 75R, and 75A. J. S. Mill denies only implicitly that the state is a contract; see Mill, *Considerations on Representative Government* (New York: Library of Liberal Arts, 1958), chap. 1, pp. 6–11.

[47] As in, e.g., Locke, *Second Treatise,* secs. 13 and 88.

[48] Hegel, *Philosophy of Right,* secs. 102–103. This is a not uncommon distinction: see K. G. Armstrong, "The Retributivist Hits Back," *Mind,* 70, No. 280 (Oct. 1961), 487–488; and T. H. Green, *Lectures on the Principles of Political Obligation* (Ann Arbor, Michigan: University of Michigan Press, 1967), sec. 178.

[49] As in Hobbes, *Leviathan,* chap. 28.

[50] Hegel, *Philosophy of Right,* secs. 102, 102A, and 220; Stillman, "Hegel's Idea of Punishment," sec. I.

[51] It might be inferred that Hobbes and Locke, by seeing the right of punishment as a prepolitical right, do not regard as theoretically central the distinctions between an interested and a disinterested judge and between a consenting and a disaffected criminal. For T. H. Green (sec. 178), they also fail to understand rights correctly.

[52] Green (sec. 178) makes similar and related (and Hegelian) arguments along this line.

The final crucial theoretical error inherent in theories of natural rights is the conclusion that the state must limit man's natural freedom and autonomy. This error stems not only from the state of nature hypothesis. It also derives from the tendency of modern and liberal political philosophers to begin political reflection with a consideration of the "I" or "Of Man," as have moderns from Descartes and Hobbes,[53] and to develop this "I" or "man" to his full concreteness, humanity, and social behavior before a discussion of society, so that, for instance, the Hobbesian or the Lockean "man" has the same characteristics within civil society as outside it, in the state of nature as in the civil state. One result is, of course, that this completely civilized and human man, developed in a logical abstraction from society, must necessarily be limited in his rights as soon as he is put in a social context.

At best, this usual liberal point of view is slightly depressing: all men's rights and liberties must be limited. At worst, it is dangerous: if liberties must be limited for security or utility, the scope of limitation may be unlimited, as, for instance, in a "closed commercial state," or in extreme forms of pragmatism.[54] And, on the surface, it is paradoxical: for men join states in order to protect their life, liberty, and property, and yet immediately upon joining find their property and liberty limited, and—in times of war or in case of crime—their life endangered.

Hegel has a threefold answer to the problem that society must limit the rights of autonomous man. In the first place, the "I" or "man" that forms the first determination of modern and liberal political philosophy is not and cannot be a fully developed human being. If he is abstracted from civil society, those characteristics of his which are social must be omitted, and indeed any characteristics which man outside of society would have must be added. It is an invalid logical sleight of hand which abstracts a fully socialized man from society and proclaims him therefore an autonomous man whose characteristics are the opening determination of a political theory. Before Hegel, Rousseau recognized the logical problems of the prepolitical autonomous man;[55] Rousseau adopts a hypothetical historical approach to try to eliminate the problem.[56] Seeing the same problem, Hegel adopts the usual liberal approach of developing the rights of men in a prepolitical state. Hegel's prepolitical state is, however, not a Rousseauian natural state prior in time, but an abstract state which is prior only in terms of logic. In Hegel's prepolitical state, as in that of all liberals, the fully socialized human is abstracted from society; but Hegel's formulation is distinctive in also abstracting the prepolitical man from his socialized behavior and attitudes.[57] Thus, the Hegelian "I" or "man" of abstract right is simply an abstract "I" or abstract "man": a person.

The prepolitical man, the liberal's natural man, is not a full individual but merely an abstract person: Hegel's logic is accurate. And, by considering the abstract person rather than the autonomous man in the prepolitical condition, Hegel makes it clear at least that the prepolitical condition is a realm not only of greater freedom (from laws and other governmental limits) in some ways but also of lesser freedom and of lesser humanity than the social state: for the sphere of abstract or natural rights contains not only no governmental limitations but also, among other aspects, no morality, no feelings, no individuation, and an arbitrary freedom that is itself rather limited. It is clear in the Hegelian formulation that, while the prepolitical person may be limited when put in a concrete social context, he will also gain some desirable and human characteristics when he is put in a social context. What most liberals see simply as a limitation, Hegel's abstraction shows as also a development.

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[53] René Descartes, *Meditations on First Philosophy,* in *Descartes: Philosophical Writings,* trans. Norman Kemp Smith (New York: The Modern Library, 1958), esp. "Meditation II," pp. 182–187; Hobbes, *Leviathan,* Part I.

[54] Reinhold Aris, *History of Political Thought in Germany* (London: George Allen & Unwin, 1936), pp. 125–135; John Dewey, *Liberalism and Social Action* (New York: Capricorn Books, 1935; reprint ed., 1963).

[55] Jean-Jacques Rousseau, *The First and Second Discourses,* ed. Roger D. Masters (New York: St. Martin's, 1964), *Second Discourse,* "Preface," pp. 91–93.

[56] *Ibid.,* "Preface," p. 97.

[57] Hegel's abstract man, the person, is abstracted from his social contexts but not from his historical development; the person lacks socialized characteristics but is nonetheless an abstraction from contemporary man, and thus the person claims for himself the rights that a contemporary man claims. In contrast, Rousseau's primitive man in the *Second Discourse,* when logically isolated from society, loses all his socialized and historically-developed characteristics ("Preface," pp. 91–97); Hobbes's natural man, when logically isolated from society, retains all his socialized and historically-developed characteristics (Hobbes, *Leviathan,* chap. 13).

Hegel's second answer is that the limits imposed in society on the rights of prepolitical autonomous or abstract men are actually prepolitical limits as well. For the existence of the prepolitical annulment of crime makes quite clear that, purely at the level of rights, individuals are limited by rights. Thus, the progress from the prepolitical to the political is not the shift from autonomy and freedom to legal limits; rather, it is the shift from rights as limits to laws as limits.[58]

Hegel's third answer to the mistaken liberal conceptualization of the prepolitical autonomous man with rights and liberties is simply that only in civil society and the state do rights and liberties exist in an actualized and concrete form; therefore, the state, instead of limiting rights, is in fact the precondition for their existence.[59] Without the state and political order, rights are unsure; only with society do rights exist actually and securely. Indeed, Hegel's three points together indicate the necessity of state and society for individual rights and liberties.

Conclusion

In sum, Hegel cogently criticizes natural rights theories for their narrownesses and for their theoretical inadequacies. Yet his criticisms are made in the context of his asserting abstract rights and thus do not lead to a rejection of the rights themselves, but merely of their previous formulations. Therefore, the criticisms point the way to later portions of the *Philosophy of Right*—to Hegel's treatment of morality, educative institutions, institutions that are not contractual—just as Hegel's assertion of abstract rights points the way to his exposition of civil society and other later sections of the text in which he discusses the concrete actualizations of rights.

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[58] The shift is desirable, of course, because the enforcement of rights is uncertain, that of laws fairly sure. Locke and Hegel agree here; see Locke, *Second Treatise,* secs. 13 and 22–23, and Hegel, *Philosophy of Right,* secs. 103 and 217.

[59] Hegel's argument is similar to Hobbes's here; see Hobbes, *Leviathan,* chap. 17, and Hegel, *Philosophy of Right,* secs. 142 and 260. For Hobbes, however, rights exist in the state of nature and war (*Leviathan,* chap. 14, pp. 103–104), whereas for Hegel rights are brought into existence only after centuries of civilization's cultural, social, spiritual, and political development (*Philosophy of Right,* secs. 341–360).

Party Coalitions in Multiparty Parliaments: A Game-Theoretic Analysis*

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Since the days of A. Lawrence Lowell, students of parliamentary democracy have argued that durable cabinets require majority party government. As Lowell writes:

> It is an axiom in politics that, except under very peculiar circumstances, coalition ministries are short lived compared with homogeneous ones . . . .
> A cabinet which depends for its existence on the votes of the Chamber can pursue a consistent policy with firmness and effect only when it can rely for support on a compact and faithful majority; and therefore the parliamentary system will give the country a strong and efficient government only in case the majority consists of a single party.[1]

Following the publication of Lowell's book, *Government and Parties in Continental Europe* (1896), every generation of political scientists has witnessed the recitation and reaffirmation of Lowell's thesis. The list of advocates includes Lord Bryce (*Modern Democracies*, 1921), Harold Laski (*Parliamentary Government in England*, 1938), F. A. Hermens, *Democracy or Anarchy* (1941) and Maurice Duverger (*Political Parties*, 1951).[2] A major theme found in all of these classics is summarized concisely by Duverger: "Multipartism weakens the government in a parliamentary regime. . . . The absense of a majority party makes it necessary to form heterogeneous . . . cabinets based on a coalition, or else minority cabinets." As a consequence, "cabinet collapses which are exceptional and rare under the two-party system become normal and frequent and are scarcely mitigated by the fact that the same men are often to be found in different ministerial combinations."[3]

In our own generation, Lowell's thesis is reflected most clearly in the work of Jean Blondel. In "Party Systems and Patterns of Government in Western Democracies" (1968), Blondel writes that the "duration (of cabinet governments) is unquestionably influenced by the type of party system prevailing in the country." According to Blondel:

> Two factors . . . appear to contribute to the relative duration of governments in parliamentary systems: the fact that the system is a straight clash between two parties and the fact that one party government prevails. The two factors are quite clearly connected: one-party governments are not possible without at least one large party, except on a temporary basis. On balance, . . . it does seem that one-party government . . . is the factor contributing most decisively to the stability of governments.

In contrast to one-party governments, Blondel concludes that "Coalition, whether small or large, appears directly antagonistic to stable government . . . "[4] Blondel's index of stable government is cabinet durability.

The empirical evidence presented in these works has been of two different types. Scholars from Bryce to Duverger supported Lowell's thesis by comparing the unstable experience of France or Weimar Germany to the stable government of the United Kingdom, implying that the former were typical multiparty regimes whereas the latter was the typical majority party system. Blondel made a major advancement by expanding the number of countries examined; he analyzed 17 Western parliamentary democracies between 1946 and 1966, focusing on the average differences in cabinet durability between party systems. Blondel's analysis indicated that (1) majority party systems produce more durable cabinets on the average than multiparty cabinets and (2) single party governments are more durable than coalition governments.[5]

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* Throughout the preparation of this study William H. Flanigan, Edwin Fogelman and W. Phillips Shively provided a constant source of counsel and encouragement. In addition, during the formulation of these ideas I benefited from stimulating discussions with numerous faculty members and graduate colleagues at the University of Minnesota and, at a later stage, I received helpful critiques from Norman Frolich, Joe Oppenheimer, John Pierce, Robert Putnam, Kenneth Shepsle and Oran Young. For a more extensive development of the analysis presented in this article, see Lawrence C. Dodd, *Party Coalitions and Parliamentary Government* (Princeton University Press: forthcoming, 1975).

[1] A. Lawrence Lowell, *Governments and Parties in Continental Europe* (Boston: The Riverside Press, 1896), I, 70, 73; see also Lowell, *The Governments of France, Italy and Germany* (Cambridge: Harvard University Press, 1914), pp. 70–74.

[2] James Bryce, *Modern Democracies* (New York: Macmillan Company, 1921), I and II, 121–122, 347–348; Harold J. Laski, *Parliamentary Government in England* (New York: The Viking Press, 1938), pp. 56–57; Ferdinand A. Hermens, *Democracy or Anarchy?* (Notre Dame: University of Notre Dame Press, 1941), pp. 16–17, 68–69, 72–74; and Maurice Duverger, *Political Parties* (London: Methuen Press, 1951).

[3] Duverger, pp. 407–408.

[4] Jean Blondel, "Party Systems and Patterns of Government in Western Democracies," *Canadian Journal of Political Science*, 1 (June, 1968), 198–199.

These works clearly build on Lowell's analysis; they uncover patterns that support Lowell's predictions. Nevertheless, critical problems remain before Lowell's thesis can be accepted. First, these studies rely on broad contrasts of central tendencies between majority party systems and multiparty systems. They fail to examine systematically the variation around central tendencies. Second, Lowell's followers have offered no real theoretical justification for his thesis. They have repeated his axiomatic assertion that coalitions are nondurable, failing to note that this axiom, if genuinely true, would reduce political analysis to chaos, since political associations (particularly political parties) are all coalitions of some sort. Concern with Lowell's thesis has proceeded largely at the inductive level with each new generation offering new empirical evidence that presumably supports his thesis.[6] The thesis has yet to be subsumed within a coherent theory. Thus we are left with two questions. Is it really true that durable cabinets do not exist in multiparty parliaments? Is Lowell's axiom that coalitions are nondurable really axiomatic?

Based on analysis of the countries and time periods listed in Table 1, the answer to each question is a firm no. Lowell rests his thesis on the assertion that multiparty parliaments cannot produce durable cabinets; only majority party parliaments produce durable cabinets. From the perspective of central tendencies, these assertions seem correct. For the countries and time periods listed in Table 1, one party majority cabinets persisted in office for approximately 48 months on the average; cabinets in multiparty parliaments lasted 26 months on the average. As Table 2 indicates, however, the analysis of central tendencies is hiding a great degree of variation in cabinet durability among multiparty parliaments.

Table 2 presents patterns for all multiparty parliaments of the countries and time periods in Table 1. As throughout this study, (1) multiparty parliament refers to any parliament with no majority party; majority party parliament refers to any parliament with a majority party; (2) a cabinet is durable so long as there is no change in the parties within the cabinet. As Table 2 indicates, there is wide variation in the durability of cabinets formed in multiparty parliaments, with 23 per cent of all *cabinets* lasting 40 months or longer. In 34 per cent of the multiparty *parliaments*, the shortest cabinet lasted at least 40 months; the percentage would be higher if we included those parliaments in which no new cabinet formed but during which an old cabinet continued. And of those governments that were coalitions, approximately 22 per cent lasted 40 months or longer. These patterns indicate that while the average durability of cabinets may be low for multiparty parliaments, a large number of durable cabinets exist and are concentrated in certain specific parliaments rather than spread evenly throughout all parliaments. Finally, Lowell was simply wrong in his assertion that coalitions are necessarily transient. Even where periods of wartime are omitted, some coalitions are quite durable.

Clearly, the validity of Lowell's thesis must be challenged. Multiparty parliaments do not appear as detrimental as the scholarly consensus of the past 70 years would suggest; party coalitions are not necessarily antagonistic to durable government. A re-examination of the impact of party systems on cabinet durability is in order. Why are some coalitions durable whereas others are not? Why do some multiparty parliaments produce durable cabinets whereas others do not? What impact does the party system have on cabinet durability?

The answers to these questions lie in the complex world of multiparty parliaments. Politics in this world has been characterized quite aptly by Maurice Duverger as a "parliamentary game"; the object of the game is to form and control the government; in the game, a durable government "must find support from a coalition of associated parties: their alliance is always uneasy, and intrigues are perpetually being hatched in the lobbies of parliament to break up the existing combination and replace it with a new one."[7] Some intrigues succeed and a cabinet falls; other intrigues fail and a cabinet persists. The explanation derives from understanding the parliamentary game.

The purpose of the present study is to provide an explanation of cabinet durability based on a game-theoretic analysis of party coalitions in multiparty parliaments. The thesis of the study can be summarized as follows:

(1) Cabinet durability in multiparty parliaments is determined by the coalitional status of the cabinet in power. Minimum winning cabinets will be quite durable; oversized and undersized cabinets will be more transient.

(2) The coalitional status of the cabinet that forms in a parliament is determined by the bargaining conditions that exist in parliament. Two relevant conditions are: (1) information certainty; (2) the *a*

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[5] Jean Blondel, *An Introduction to Comparative Government* (New York: Praeger Publishers, 1969), p. 342.

[6] For a recent study in this vein see Michael Taylor and V. M. Herman, "Party Systems and Governmental Stability," *American Political Science Review*, 65 (March, 1971), 28–37.

[7] Duverger, p. 400.

Table 1. Countries and Time Periods Investigated

Country	Time Period	Country	Time Period
Australia	1919–1940, 1945–1972	Great Britain	1919–1940, 1945–1972
Austria	1919–1932, 1945–1972	Iceland	1924–1939, 1945–1972
Belgium	1919–1939, 1946–1972	Ireland	1945–1972
Canada	1918–1940, 1945–1972	Italy	1946–1972
Denmark	1920–1940, 1945–1972	Luxembourg	1926–1940, 1946–1972
Finland	1918–1940, 1945–1972	Netherlands	1918–1940, 1946–1972
France	1918–1940, 1945–1958	New Zealand	1918–1940, 1945–1972
Germany	1918–1932, 1949–1972	Norway	1920–1935, 1945–1972
		Sweden	1920–1939, 1945–1972

priori willingness of parties to bargain. As these conditions vary within a parliament or between parliaments, the coalitional status of the cabinets will vary.

(3) The bargaining conditions that exist within a parliament are influenced by the nature of the parliamentary party system. Three relevant party system characteristics are fractionalization, instability and cleavage conflict.

The clear inference of the thesis is that the parliamentary party system does affect cabinet durability. In this sense the thesis is an extension of Lowell's emphasis on the role of the parliamentary party system. The thesis departs from Lowell's position, however, in three major ways: (1) cabinets within multiparty parliaments are viewed as subject to wide variation in durability; (2) party coalitions are viewed as a potential source of durability as well as transiency; (3) the impact of party systems on cabinet durability is a much more complex and multivariate process than Lowell and his followers indicate.

The remainder of this study analyzes the thesis in more detail. The first two sections present the theoretical explication of the thesis and an empirical interpretation of its conceptual elements. The next section presents initial tests of hypotheses drawn from the theory. The data bank employed in these tests covers all multiparty parliaments in the countries and time periods listed in Table 1 and includes a list of the cabinets that formed in the parliaments, the parties that held ministerial seats, a list of all parties in the parliament and the legislative strength of these parties in the parliament.[8] The concluding section discusses the implications of the study.

A Theory of Cabinet Formation and Maintenance

The distinguishing characteristic of parliamen-

Table 2. Cabinet Durability in Peacetime Multiparty Parliaments: A Frequency Count, 1918–1972

Cabinet Durability (in months)	All Cabinets	Shortest Cabinet Per Parliament[a]	Coalition Cabinets
50 or more	28 (12.5%)	24 (18.9%)	25 (14.1%)
40 to 49	23 (10.3%)	20 (15.7%)	15 (08.5%)
30 to 39	22 (09.9%)	14 (11.0%)	12 (06.8%)
20 to 29	37 (16.6%)	15 (11.8%)	27 (15.3%)
10 to 19	45 (20.2%)	15 (11.8%)	40 (22.6%)
0 to 9	68 (30.5%)	39 (30.7%)	58 (32.8%)
Total	223 (100%)	127 (100%)	177 (100%)

[a] This count includes only those parliaments in which a new cabinet was formed.

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[8] The cabinet data analyzed in this study were collected under the auspices of the Minnesota Historical Data Archives. The Archives are directed by William H. Flanigan and Edwin Fogelman under a grant from the Ford Foundation. The data were collected by Illa Miscoll, currently at the University of Heidelberg (Germany), and by the author. I am grateful to Flanigan and Fogelman for allowing me access to the data, and I owe a huge debt to Illa Miscoll for her patience and precision. The original sources from which the cabinet data were collected include literally hundreds of scholarly works and newspapers that cannot be cited here. Every attempt was made to verify all data by at least two sources. And, while most of our research was conducted prior to its availability, a useful work for our verification process was Klaus von Beyme, *Die parlamentarischen Regierungssysteme in Europa* (Munchen: R. Piper and Co., 1970), pp. 901–967.

The party system data were gathered by the author, relying basically on a series of specialized country documents as well as *Keesing's Contemporary Archives* (London: Keesing's Publication, 1945–1972); Dolf Sternberger and Dieter Nohlen, *Die Wahl der Parlamente* (Berlin: Walter De Gruyter and Co., 1969); Stein Rokkan and Jean Meyriat, *International Guide to Electoral Statistics* (The Hague: Mouton, 1969).

tary democracy is that the chief executive and his cabinet are dependent upon parliament for their installation and maintenance.[9] Within the life of any parliament, numerous cabinet governments can be installed and deposed through party negotiation.[10] There are no general legal limitations to the nature and type of cabinets that are formed. The parties in a cabinet may control a bare majority of parliamentary seats, virtually all seats, or virtually no seats. The distribution of ministerial positions within a cabinet may be proportional to legislative strength in parliament, reflect ideological or emotional biases, or be totally disproportionate to legislative seats.[11]

In the following theory,[12] parliamentary parties are treated as the basic actors. This convention is followed to simplify and clarify the presentation. Clearly, parliamentary parties are not homogeneous and monolithic entities but are themselves coalitions of factions held together by leaders through the use of a variety of payoffs.[13] In addition, some members of parliament may not belong to parliamentary parties, or members of various parties may belong to supra party organizations that cut across various parties. These factors can influence cabinet formation and maintenance. They are complications that can be added after a simple framework has been constructed and tested. The present goal is to construct an explanatory framework and to examine its predictive utility.

The Basic Framework. Let us assume that political parties enter parliament in a quest for governmental power. This quest originates at the electoral level as the parties seek popular support for their candidates. At the electoral stage, each party articulates a political program designed, at least in part, to attract a political following so as to maximize the party's legislative strength within parliament. The quest for power culminates, in a narrow sense, as a party or set of parties forms the cabinet and successfully maintains control of the cabinet for a time sufficient to enact and administer their legislative program.[14] In a parliament with no majority party, the quest by each party for secure participation in and control of the cabinet becomes a search, in Middleton's terms, for "a coalition of natural allies."[15] Immediately upon the conclusion of the electoral stage, "Parties . . . (begin) trying to discern the physiognomy of the assembly, and particularly to forecast the majority—or the various possible majorities—which it is capable of producing. Groups are rarely content simply to represent shades of electoral opinion. They look forward to taking an active part in government. . . . Hence the importance of the calculation of the visible and latent tendencies of the newly elected assembly."[16]

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[9] On the nature of parliamentary democracy see Michael Ameller, *Parliaments: A Comparative Study* (London: Cossell and Company, Ltd., 1966); C. M. F. Campion and D. W. S. Liddendale, *European Parliamentary Procedure: A Comparative Handbook* (London: George Allen and Unwin, Ltd., 1953); Carl J. Friedrich, *Constitutional Government and Democracy* (Boston: Ginn and Company, 1950), pp. 222–237, 296–386.

[10] For discussion of party behavior in the various countries, see Walter Rice Sharp, *The Government of the French Republic* (New York: D. Van Nostrand Co., Inc., 1938), pp. 73–77; Sven Groennings, "Patterns, Strategies and Payoffs in Norwegian Coalition Formation," in *The Study of Coalition Behavior*, ed. Sven Groennings, E. W. Kelly, and Michael Leiserson (New York: Holt, Rinehart and Winston, Inc., 1970); Peter H. Merkl, "Coalition Politics in West Germany," in Groennings et al., *The Study of Coalition Behavior;* Herbert P. Secher, "Coalition Government: The Case of the Second Austrian Republic," *American Political Science Review*, 52 (September, 1958), 791–808; Duncan MacRae, Jr., *Parliament, Parties and Society in France, 1946–1958* (New York: St. Martin's Press, 1967); Giorgio Galli and Alfonso Prandi, *Patterns of Political Participation in Italy* (New Haven: Yale University Press, 1970); Dankwart K. Rustow, *The Politics of Compromise: A Study of Parties and Cabinet Government in Sweden* (Princeton, N.J.: Princeton University Press, 1956). See also the various essays in Robert A. Dahl, ed., *Political Oppositions in Western Democracies* (New Haven: Yale University Press, 1966). For more general discussions, see Duverger, *Political Parties*, pp. 281–351; Sven Groennings, "Notes Toward Theories of Coalition Behavior in Multiparty Systems: Formation and Maintenance," in Groennings et al., *The Study of Coalition Behavior.*

[11] This study will not attempt to include in the analysis the influence of any procedures specific to particular parliaments although such procedures can certainly have a vital influence on cabinet formation and maintenance. For examples, see Gerhard Loewenberg, *Parliament in the German Political System* (Ithaca, N.Y.: Cornell University Press, 1966). A systematic cross-national analysis of the nature and consequences of parliamentary procedure is clearly needed by analysts of cabinet behavior. For a recent study of the actual distribution of ministerial positions among cabinet parties see Eric C. Browne and Mark N. Franklin, "Aspects of Coalition Payoffs in European Parliamentary Democracies," *American Political Science Review*, 68 (June, 1973), 453–469.

[12] Essentially, the thesis results from the convergence of two schools of analysis. The first school is that represented by Lowell, Blondel, Taylor-Herman, Giovanni Sartori, and others; this school emphasizes the influence of the parliamentary party system on cabinet durability. The second school includes von Neumann and Morgenstern, Riker, Leiserson, Axelrod, Browne, Groennings, and others; some of these scholars were instrumental in developing game-theoretic models, while others have emphasized the potential utility of game-theoretic models in the study of multiparty parliaments. The present study synthesizes the theses of these two schools into a broader theory of cabinet formation and maintenance in multiparty settings. See Jean Blondel, "Party Systems"; A. Lawrence Lowell, *Governments and Parties in Continental Europe;* Taylor and Herman, "Party Systems and Governmental Stability"; Giovanni Sartori, "European Political Parties: The Case of Polarized Pluralism," in *Political Parties and Political Development*, ed. Joseph LaPalombara and Myron Weiner (Princeton, N.J.: Princeton University Press, 1966); John von Neumann and Oskar Morgenstern, *The Theory of Games and Economic Behavior* (Princeton: Princeton University Press, 1944); R. Duncan Luce and Howard Raiffa, *Games and Decisions* (New York: John Wiley, 1957); William H. Riker, *The Theory of Political Coalitions* (New Haven: Yale University Press, 1962); Michael Leiserson, "Factions and Coalitions in One-Party Japan: An Interpretation Based on the Theory of Games," *American Political Science Review*, 62 (September, 1968) 770–787; Robert Axelrod, *Conflict of Interest* (Chicago: Markham, 1970); Eric C. Browne, "Testing Theories of Coalition Formation in the European Context," *Comparative Political Studies*, 3 (January, 1971), 1–15; see also the various articles in Groennings et al., *The Study of Coalition Behavior.*

[13] See Duverger, *Political Parties*, pp. 61–132; Leon D. Epstein, *Political Parties in Western Democracies* (New York: Praeger Pub., 1967), pp. 98–129; Frank J. Sorauf, *Party Politics in America* (Boston: Little, Brown and Co., 1968), pp. 82–92. At a more theoretical level see particularly Norman Frolich, Joe A. Oppenheimer and Oran R. Young, *Political Leadership and Collective Goods* (Princeton, N.J.: Princeton University Press, 1971).

In this calculation, the central motivation is the desire by each party to enter a durable cabinet and to maximize the significant positions that it will hold within the cabinet. As representative organizations mandated by their various constituencies to enact legislative programs, each party could seek to do no less. This motivation dictates that, in immediate negotiations, each party opposes every other party. Each party seeks its own maximum ministerial advantage: each parliamentary party behaves as an actor in a conflictual coalitional game. The resources that a party brings to the game are its legislative votes. The desired payoff, in an immediate sense, is one or more ministerial positions in the cabinet. In the game, each party seeks to enter a coalition of parties that will have a reliable majority with which to install and sustain the cabinet. At the same time, each party seeks to enter a coalition through which it can maximize its own ministerial status. This involves fairly complex calculations.

Each party must estimate not only which party coalitions can win but also (1) which coalitions are possible in the sense that each coalition partner would view the coalition as a desirable one, and (2) which coalition or coalitions will be favorable to it in terms of ministerial seat payoffs. This calculation is complicated because some ministerial positions are more desirable than others, so that the value of payoffs received in each potential coalition are not calculable strictly in terms of the number of ministerial seats obtained. Owing to the intricacy and complexity of these calculations, parliamentary parties can be overwhelmed in coalition negotiations. To avoid this result, parties employ a basic criterion in all calculations and negotiations: they seek first and foremost to enter a minimum winning coalition. That is, they seek to enter a coalition of parties that (1) has a reliable majority, yet (2) contains no party in the coalition that is unnecessary to majority status.

The minimum winning criteria provides a baseline against which all possible coalitions can be compared by the parties. If a coalition is not of minimum winning status, it is either (1) not of winning status or (2) contains some unnecessary party within it that could be removed from the cabinet with its ministerial payoffs distributed among the other coalition partners. The former coalition is undesirable to a party because it would not guarantee victory in the cabinet formation and maintenance process. The latter coalition is undesirable to a party because it would waste desirable payoffs unnecessarily, denying to the party the maximum ministerial status that it could receive if only the necessary parties were included in the coalition. Seen in this perspective, the search for natural parliamentary allies is a search for partners with whom to form a minimum winning coalition. In varying coalitional conditions parties also may bring other considerations to bear such as the programmatic perspective of their potential coalition partners.

In the effort to enter a minimum winning coalition of parties, as actors in any conflictual coalitional game, parliamentary parties are constrained by a number of parameters. First, events of overriding seriousness may threaten the existence of the parliamentary system, challenging the security of the coalitional game itself. In such a situation, most clearly seen in wartime, the motivation for parties to maximize their participation in the cabinet may be overridden by the desire to save the game. Second, parties are constrained by explicit and implicit rules of the parliamentary game. An explicit rule is that a majority of 50 per cent plus one of legislative votes is the level at which a coalition assures itself winning status. An implicit rule of the game is that the cabinet formation process cannot last indefinitely. The longer the coalition bargaining process lasts, the greater the pressure becomes for some cabinet to be formed. Eventually, if no majority solution can be reached, then that minority cabinet will be installed that can attain the largest plurality of votes. Such a cabinet will persist only until a larger coalition arises that is willing to take over cabinet responsibility or until parliament is dissolved.

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[14] These are general assumptions shared with numerous analysts. See for example Anthony Downs, *An Economic Theory of Democracy* (New York: Harper and Row, 1957), pp. 21–35, 142–163; Gunnar Sjoblom, *Party Strategies in a Multiparty System* (Sweden: Lund, 1968), pp. 68–95, 250–279.

[15] W. L. Middleton, *The French Political System* (London: Ernest Benn, Ltd., 1932), p. 154.

[16] Middleton, pp. 107–108.

Finally, parties are constrained in their calculations and negotiations by the bargaining conditions that prevail within parliament. As noted earlier, the quest for governmental power in a multiparty parliament can be conceived as a search by each parliamentary party for partners with which to form a minimum winning coalition. The calculations and negotiations in this search are intricate and complex. First, parties must estimate which of the possible party coalitions possess a reliable parliamentary majority and, among these coalitions, which ones achieve minimum winning status. These calculations require that parliamentary parties possess reliable information as to the voting strength of the various parliamentary parties. Second, as they negotiate parties must realistically estimate which parties are available to them as potential coalition partners. To do this, they must know the implicit as well as explicit commitments made by parties to one another. Third, if parties are to enter coalitions, they must be willing to negotiate with other parties, "to form coalitions and to compromise."[17] They must be willing, at least in principle, to enter into cabinets with other parliamentary parties.

These bargaining conditions can be subsumed under two categories: (1) information certainty, and (2) the generalized *a priori* willingness of parties to bargain.

(1) Following Riker,[18] information certainty entails two elements:
   (a) *complete information as to weights*—exists in a parliamentary setting when all parties know precisely the weight (that is, the reliable number of legislative votes) controlled by all parliamentary parties.
   (b) *perfect information as to prior moves*—exists in the bargaining arena when each party knows all of the moves (that is, offers, bargains, counter-offers, etc.) made among all of the various parties in coalition negotiations.

(2) A generalized *a priori* willingness of parties to bargain exists when all parties are willing to consider entering a cabinet coalition with any other party. In other words, there are no *a priori* constraints which circumscribe or inhibit the negotiation and coalition formation between any two parties.

If we assume (1) that most coalition bargaining between parties occurs under conditions unconstrained by extraordinary events such as war and (2) that the rules of the game specified earlier are indeed operative, then the critical factors in cabinet formation are the bargaining conditions that characterize a parliament.

Cabinet Coalitional Status and the Willingness of Parties to Bargain. If parties in parliament possess a fairly generalized willingness to bargain among themselves, majority or winning coalitions should always form. In other words, so long as there are no serious *a priori* constraints on parties which make them hesitant to negotiate or strike bargains, we can expect that parties can reach agreement on some majority that will govern. By contrast, if parliamentary parties are highly constrained in their willingness to bargain among themselves, there should be a tendency toward minority cabinets. The central question here is whether there is a set of parliamentary parties (1) that possess a majority and (2) each party in the set is willing to negotiate with all other parties in the set. In a parliament in which every party is willing to bargain with every other party, numerous majority sets exist. However, the more specific each party is in specifying the party or parties with which it will bargain, the greater the possibility becomes that there is no majority set of parties willing to bargain. Consequently, the less generalized the willingness of parties to bargain among themselves, the greater the possibility of a minority cabinet forming.

Figure 1 depicts the resulting relationship between the generalized *a priori* willingness of parties to bargain and cabinet coalitional status. In this graph the y axis indicates the coalitional status of the cabinet that forms in a parliament. This status can be of three general types:

(a) *greater than minimum winning status* (>*mwc*).
Cabinets in this category consist of at least one

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[17] This point is made clearly by Bracher:

> The cabinet crises which plagued the German Republic throughout its life were the direct consequences of the nature of German political parties, their unwillingness to form coalitions and to compromise, their rigid ideological stance, their preoccupation with prestige, and their authoritarian tradition.
> This was the price paid for the partly forced, partly voluntary rejection of parliament and parties during the monarchy, a policy whose negative aspects were pinpointed very clearly by Max Weber . . . (who) insisted that the absence of a two-party system was not the obstacle: "Far more important is another difficulty: parliamentary government is possible only if the largest parties of the parliament are in principle *on the whole* ready to take over the responsible conduct of state . . ."

See Karl Dietrich Bracher, *The German Dictatorship: The Origin, Structure and Effects of National Socialism* (New York: Praeger Publishers, 1970), p. 78.

[18] Riker, *The Theory of Political Coalitions*, pp. 77–89.

party that could be subtracted from the coalition and the cabinet's majority status sustained; the greater the number of extra parties and the greater the weight (legislative voting strength) of these parties, the more oversized a cabinet coalition is.

(b) *minimum winning status* (*mwc*).

Cabinets in this category are (1) of winning or majority status and (2) do not contain any party that could be subtracted and winning (or reliable majority) status sustained.

(c) *less than minimum winning status* (<*mwc*)

These cabinets do not possess a reliable parliamentary majority; the greater the number and weight of the parties that would be required to make the coalition of minimum winning status, the greater the deviation of these cabinets from minimum winning status.

The *x* axis in this graph represents the generalized *a priori* willingness of parties to bargain among themselves. In conditions of extremely low constraints, every party is willing to bargain with every other party; in conditions of extremely high constraints, no party is willing to bargain with any other party.

The checkered space in Figure 1 specifies the relationship between cabinet coalitional status and the willingness of parties to bargain. Oversized (>*mwc*) cabinets form in conditions of low to moderate bargaining constraints. Undersized (<*mwc*) cabinets form in conditions of moderate to high bargaining constraints. Minimum winning cabinets form in a wide range of bargaining conditions: the main factor (from the standpoint of bargaining constraints) will be whether there is at least one majority set of parties that are willing to bargain. A great deal of variation in cabinet coalitional status cannot be explained by variation in the generalized *a priori* willingness of parties to bargain. The willingness of parties to bargain determines the general range in which the coalitional status of a cabinet should fall. Within this range the coalitional status of the cabinet that forms can vary; the critical element in this variation is the degree of information certainty that exists within the parliamentary bargaining arena.

Cabinet Coalitional Status and Information Certainty. In parliaments that experience low to moderate constraints on party bargaining, variation in information certainty produces variation between minimum winning and oversized cabinet status. If parties can estimate precisely the strength of all other parties and the moves being made by all other parties, they will act to form minimum winning coalitions. If parties are unsure of the reliable votes possessed by each other, however, or if they are unsure of each other's reliability as coalition partners, then parties will compensate for this uncertainty by entering into oversized coalitions. A coalition of parties will form that is larger than would appear nominally necessary to the coalition's majority status. The greater the information uncertainty, the more oversized these coalitions will be.

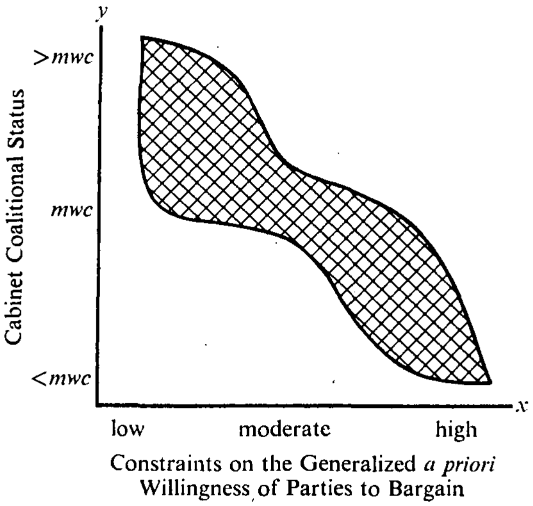


Figure 1. The Predicted Relationship Between the Willingness of Parties to Bargain and Cabinet Coalitional Status

By contrast, in parliaments experiencing moderate to high constraints in party bargaining, information uncertainty creates or augments a tendency toward undersized cabinets. This follows if we consider the nature of calculations and negotiations in such parliaments. On the one hand, parties want to enter minimum winning coalitions. When minimum winning cabinets are not possible because of the constrained nature of party bargaining, parties will seek to be included in the largest possible coalition. In this way they maximize the security of the cabinet by making it as difficult as possible for a larger coalition to arise. On the other hand, in highly constrained settings parties have *a priori* reasons to avoid serving in a cabinet with certain specific parties (say party A), while they may be more willing to serve with other parties (say party B). Party B and party C may or may not be willing to serve in cabinets together. Faced with a situation like this, complete information as to weights (reliable legislative votes) is needed if parties are to calculate the size of potential coalitions; perfect information about prior moves is needed by parties to guard against the possibility of being drawn into a cabinet with an undesirable partner.

In a situation in which information certainty is possessed by parties, each party will realize that it cannot secretly maneuver another party into an undesirable coalition. In addition parties can clearly calculate which party coalitions are possible and desirable. Consequently, within the coalitional status range predetermined by the willingness of parties to bargain, minimum winning status will be approached as information certainty is approached. As information uncertainty increases in a parliament experiencing moderate to high constraints on party bargaining, parties will tend to form smaller cabinets than would be maximally desirable within the existing constraints. In such a situation, because of lack of perfect information, each party (A) will be hesitant to bargain with otherwise acceptable parties (B) out of fear that, in order to create a minimum winning coalition, their acceptable parties may attempt to maneuver party A into a coalition with an undesirable party (C). In addition, lack of certain information makes it difficult for parties to calculate reliably which coalition would be of the maximally desirable size; calculations and negotiations would thus take more time, and under the time constraints that exist on parliamentary bargaining, parties would be forced to settle for coalitions that might be suboptimal rather than enter coalitions that might be undesirable for *a priori* reasons.

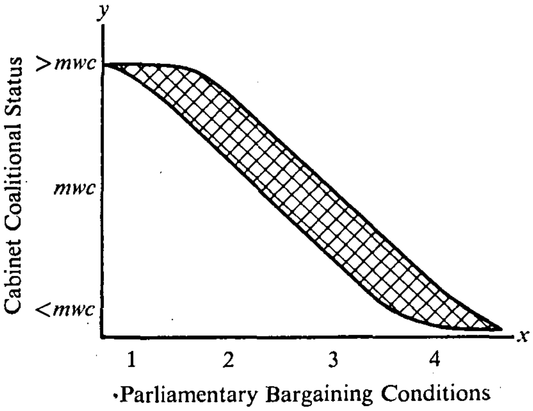
The seemingly paradoxical conclusion of the foregoing analysis is that an increase in information uncertainty can give rise to both oversized and undersized coalitions. Parties should be able to calculate minimum winning coalitions only in conditions of low information uncertainty. As information uncertainty increases, the ability of parties to calculate reliably in coalition maneuvering decreases. The type of cabinet that forms should depart from minimum winning status, with the *degree* of that departure being at least partially a function of the degree of information uncertainty. The *direction* of cabinet deviation from minimum winning status is dependent on the bargaining constraints that exist. In other words, as conditions depart from information certainty we can only predict that the resulting cabinet coalitions should depart from minimum winning status. We cannot predict the direction of the departure unless we know the nature of the *a priori* constraints that exist within parliament. If those constraints are fairly low, oversized coalitions should form as parties attempt to compensate for uncertainty. If those constraints are fairly high, undersized coalitions should form as parties act to avoid being in coalitions with undesirable parties.

Cabinet Coalitional Status and Parliamentary Bargaining Conditions. The theme of the foregoing argument is that the coalitional status of cabinets is determined by the interaction of two bargaining conditions: (1) the *a priori* willingness of parties to bargain and (2) information certainty. This interaction is fairly difficult to state or to capture precisely. In the long run, as the model constructed here is modified and expanded in a more rigorously deductive manner, it should prove possible to predict precisely the coalitional status of cabinets under different interactive conditions. For the present, Figure 2 presents the general predicted pattern. The x-axis in this figure represents the bargaining conditions that exist in parliament. The y-axis indicates the coalitional status of the cabinet that forms under the specified bargaining conditions. As Figure 2 indicates, oversized coalitions will form if the parties are fairly unconstrained in their willingness to bargain and are uncertain in their bargaining information. Minimum winning coalitions will form under a wide range of *a priori* bargaining constraints as long as parties possess reasonably certain bargaining information. Undersized cabinets will form if parties are fairly constrained in their willingness to bargain and if parties are fairly uncertain as to the reliability of their bargaining information. At the extreme ends, the relationship may assume curvilinear characteristics since coalitions cannot increase or decrease in size beyond certain absolute limits so that increases in information uncertainty beyond certain threshold limits will only reinforce (rather than augment further) the tendency toward oversized or undersized cabinets. The curvilinear characteristic of the relationship is further aggravated by the possibility that under conditions of high unwillingness to bargain, the constraints may be so great that only a one party minority government is possible. Under such conditions, increases in information uncertainty will only reinforce the existence of a minority cabinet rather than activate such a tendency.

The Durability of Cabinets. The quest for governmental power does not cease with the formation of a cabinet. Parties outside of the cabinet still desire cabinet status. Parties within the cabinet may be unhappy about their ministerial payoffs. Consequently, there should be continued challenges to the composition of a cabinet both from parties outside and from parties inside the cabinet. The nature and success of these challenges will depend on the type of cabinet coalition that has formed. In particular, the minimum winning status of cabinets is vital. If a cabinet is of minimum winning status, it is in a relatively ideal position. On the one hand, it controls a majority of parliament's votes: unlike a minority cabinet it is not at the mercy of a hostile parliament that it cannot control. The cabinet should be able to meet any threat that comes solely from parties outside of the cabinet. On the other hand, no party in the cabinet could be removed without impairing the cabinet's majority status. The significance of these factors can be clearly seen if we compare minimum winning cabinets to cabinets that are not of minimum winning status.

Consider first a comparison between minimum winning and oversized coalitions. If a cabinet is of minimum winning size, by definition no unnecessary party is sharing in the ministerial payoffs. By contrast, in an oversized cabinet at least one party is sharing ministerial status that was in reality unnecessary to attaining the ministerial payoffs. At least one party could be removed from the cabinet without destroying the reliable majority of the cabinet. The unnecessary status of the party(s) was not clear in the coalition formation stage because of information uncertainty. Once the cabinet is formed, however, it becomes obvious which parties were in on the final bargain and what the relative voting strength of these parties is. It thus becomes obvious that at least one party is expendable. If this party is omitted from the cabinet, some or all of the other coalition partners will gain in the sense that they will obtain new ministerial positions formerly controlled by the expendable party(s). Realizing this, the various parties within the coalition act to reduce the size of the cabinet by removing at least one party. Similar action will not be taken in a minimum winning cabinet because there is no expendable party among the coalition partners. Consequently, in comparing minimum winning cabinets to greater than minimum winning cabinets, minimum winning cabinets should be more durable. In addition, the more "extra" parties there are in a coalition and the greater the size of these parties, *ceteris paribus*, the smaller is the proportion of significant ministerial seats controlled by each cabinet party relative to the proportion it would control in a minimum winning cabinet. It is this relative deprivation that is important. The greater the deprivation parties suffer relative to a potential minimum winning size, the more quickly these parties should act to remove the deprivation: in the context of this study, the more oversized a cabinet coalition, the more rapidly parties will act to reduce the coalition toward minimum winning size. The more oversized the cabinet coalition is, the shorter the durability of the cabinet coalition should be.

A similar pattern occurs when we compare minimum winning cabinets with cabinets of less than minimum winning status. In a minority cabinet, the party or parties in the cabinet do not possess a reliable majority of legislative votes. The cabinet thus exists in a hostile environment that it cannot control. Because of information uncertainty, the hesitancy of various parliamentary parties to bargain among themselves, and time constraints on coalition negotiations, the minority cabinet was installed. This cabinet is faced constantly with the possibility that while it attempts to govern, other parties are negotiating their differences so that they can overthrow the cabinet and attain ministerial status, or at the least force the dissolution of parliament and new elections. In fact, the parties within the cabinet may themselves be negotiating with parties outside of the cabinet so as to form a larger and more secure coalition. Similar pressures do not exist for minimum winning coalitions. Consequently, minimum winning coalitions should be more durable than minority cabinets. In addition, the more a minority cabinet departs from minimum winning status (that is, the greater the number and size of the parties that it would take to transform the cabinet to minimum winning status), the more vulnerable the cabinet is to the rise of a larger coalition. Among minority cabinets, cabinet durability should be a function of the degree to which the cabinet departs from minimum winning status.



Code to Parliamentary Bargaining Conditions:

1 = low constraints on bargaining and high information uncertainty
2 = low to moderate constraints and low to moderate uncertainty
3 = moderate to high constraints and low to moderate uncertainty
4 = high constraints on bargaining and high uncertainty

Figure 2. The Predicted Relationship Between Cabinet Coalitional Status and the Interaction of Parliamentary Bargaining Conditions

An Empirical Interpretation

The thesis developed thus far can be summarized as follows: *cabinet durability is a function of cabinet coalitional status*; in turn, cabinet coalitional status is a function of the bargaining conditions that exist within parliament. The critical problem is to interpret the thesis in a justifiable and useful manner that maintains the power of the theory while joining it to interesting and empirically observable phenomena.[19] The most difficult aspect of the theory to interpret is information certainty and the *a priori* willingness of parties to bargain. These conditions are attitudinal phenomena that, in the most rigorous sense, require us to focus on an attitudinal interpretation of the theory. This would necessitate survey data that are obviously unavailable for sufficient time periods to allow serious analysis. In addition, an attitudinal focus—while indicating the most immediate determinants of cabinet coalitional status—does not isolate those general structural variables that are of greater interest in the cross-national analysis of parliaments. In lieu of an attitudinal focus, the interpretive approach that I propose is to focus on party system characteristics as indicators of parliamentary bargaining conditions. My basic argument is that the parliamentary party system is among the determinants of parliamentary bargaining conditions. Since the latter cannot be measured directly, given current resources, the theory must be expanded an additional causal link, employing the party system referents to test the hypotheses presented earlier. This expansion is based on plausibility rather than determinative proof; the arguments need to be subjected to theoretical refinement and extensive empirical investigation. The current study serves to direct scholars in these directions and to present empirical patterns that can be studied to evaluate the probable relationships. The party system characteristics that I examine are fractionalization, instability, and cleavage conflict.

Parliamentary Party System Fractionalization. This refers to the number and relative strength of the parties within parliament. A highly fractionalized party system is one in which a large number of parties have seats in parliament and these parties are of relatively equal strength. The inference is that the greater the fractionalization of the parliamentary party system, the greater the uncertainty of bargaining within parliament, *ceteris paribus*. This inference is based on two considerations.

(1) The greater the fractionalization of the parliamentary party system, the greater the number of relevant party relations that must be observed in the coalition bargaining process. Any party attempting to follow the various possible bargaining moves is consequently faced with a difficult and, under highly fractionalized cases, virtually impossible task. Given this situation, as the number of relevant parties increases within parliament, a decrease occurs in the ability of parties (or party leaders) to follow or to feel that they are following all moves being made by the other parties in the bargaining arena. Consequently, *as party system fractionalization increases the perfection of information about moves should decrease.*

(2) The more parties that are relevant to the bargaining process, the more difficult it should be to estimate with certainty the reliable strength of the various parliamentary parties. This is true particularly if we assume that, in estimating relative strength of parties, any given party leader must observe the internal party relations of each party, or at least must feel that, if there were serious internal fights, he would be aware of them. In a parliament consisting of only three relevant parties, that might be true since a party leader would need follow only two parties. By contrast, in a parliament of ten relevant parties, the party leader must follow nine other parties in order to estimate their reliable strength. Consequently, *as parliamentary party system fractionalization increases, the completeness of information as to weights should decrease.*

As an operational indicator of parliamentary party system fractionalization, the following measure is employed:

$$F_t = \text{antilog}\left[-\sum_{I=1}^{m} P_I \cdot \log_e P_I\right]$$

where

m = number of parties in Parliament
P_I = proportion of parliamentary seats held by Party I in Parliament t.

This fractionalization (F_t) measure has two essential attributes: (1) it increases as the number of parties increases, and (2) it increases as the relative equality of the parties increases.[20] Because it reflects not only the number of parties but the relative strength of the parties, the index is a fairly good indicator of the number of relevant parties in the bargaining process. This property follows if we assume that, the more parties are of equal size, the less likely any party is either (1) to hold a pivotal position or (2) to be totally unimportant for coalition formation. In other words, the more parties are of equal legislative size, the greater likelihood that they are equally relevant in the coalition bargaining process. It should be noted that the fractionalization index employed here is not the one employed by Douglas Rae or Taylor and Herman. As Wildgren has shown, their fractionalization measure does not increase linearly with an increase in the number of parties in a party system, *ceteris paribus*. Rather, it fails to distinguish fractionalization scores clearly for party systems beyond that point where four to five parties exist.[21] This study deals with some systems that have as many as ten or twelve parties; the measure of fractionalization employed here provides the necessary differentiation.

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[19] For discussions of the nature of empirical interpretation see Robert T. Holt and John M. Richardson, "Competing Paradigms in Comparative Politics," *The Methodology of Comparative Research,* ed. Robert T. Holt and John E. Turner (New York: The Free Press, 1970), p. 25; Carl G. Hempel, *Aspects of Scientific Explanation* (New York: The Free Press, 1965), pp. 111–112, 130–133, 184–185; John G. Kemeny, *A Philosopher Looks at Science* (Princeton, N.J.: Van Nostrand and Co., Inc., 1959), pp. 134–138; Thomas Kuhn, *The Structure of Scientific Revolutions* (Chicago: University of Chicago Press, 1962), pp. 30–40. For a more general discussion of the relation of empirical and theoretical analysis in comparative politics see James A. Bill and Robert Hardgrave, *Comparative Politics: The Quest for Theory* (Columbus, Ohio: Charles E. Merrill Pub. Co., 1973), pp. 1–41. An excellent example of theory interpretation and testing is contained in Norman Frolich, Joe Oppenheimer, Jeffrey Smith and Oran Young, "Rationality and Voting," a paper prepared for the Public Choice Society Meetings, New Haven, Connecticut, March 21–23, 1974.

[20] See John K. Wildgen, "The Measurement of Hyperfractionalization," *Comparative Political Studies,* 4 (July, 1971), 233–243. This measure is associated

Parliamentary Party System Stability. This refers to the continuity within parliament over a given set of years in the relative strength of the parliamentary parties. For the purposes of this study the following operational definition has been employed:[22]

a. To measure the continuity of the party system in Parliament t with the party system in Parliament $t-1$, the following index was used:

$$C_t = \frac{1}{2}\sum_{l=1}^{M} | L_{l_{t-1}} - L_{l_t} |$$

where

$$L_{l_{t-1}} = \frac{\text{number of legislators for Party } l \text{ at } t-1}{\text{number of legislators in Parliament } t-1}$$

$$L_{l_t} = \frac{\text{number of legislators for Party } l \text{ at } t}{\text{number of legislators in Parliament } t}$$

m = the number of parties in Parliament. Any party in parliament at $t-1$ or t is treated as a party in paliament during both time periods.

b. To determine the stability of the party system for any parliament t, the C (continuity) scores for all new parliaments held in the past five years, including parliament t, were calculated and the scores were summed. This sum is our Party System Stability (S_t) score.

Party system stability as measured above is an indicator of information certainty in the parliamentary bargaining process because:

(1) The greater the continuity of the parliamentary party system, the greater the probability that a stable partisan voting pattern will develop and the greater the probability that these patterns will be recognized by the leaders of the various parties. In other words, *as the stability of the parliamentary party system increases, the completeness of information as to weights (i.e., legislative votes) should increase.*

(2) As the stability of the parliamentary party system increases party leaders become more familiar with one another and with the behavior of the parties. Consequently it should be easier for them to anticipate and follow the various moves being made in the bargaining process. In other words, *as parliamentary party system stability increases, complete information about prior moves should increase.*

The overall inference is that as parliamentary party system stability increases, information certainty increases.

Interaction of Fractionalization and Instability. Special combinations of the two variables may affect information uncertainty in a manner not predictable from knowledge of the effects of each variable taken separately. For example:

(1) Behavior among a large number of parties may not be difficult to follow if these parties have been repeatedly represented in parliaments over a significant time period and if their strength has been fairly consistent so that prior behavior serves to guide current expectations.

(2) Discontinuity over time in the party system may not be a serious problem in less fractionalized systems since only a few parties must be followed.

(3) By contrast, the joint occurrence of high fractionalization and high instability together may activate or at least augment the potential effects of each: party leaders must follow manuevers among a large number of parties with little past behavior to rely on to delineate or predict parliamentary

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with information theory, particularly the concept of entropy or, as Galtung presents it, "uncertainty." See Claude E. Shannon and Warren Weaver, *The Mathematical Theory of Communication* (Urbana: University of Illinois Press, 1959); John Galtung, *Theory and Methods of Social Research* (New York: Columbia University Press, 1967); Henri Theil, *Economics and Information Theory* (Chicago: Rand McNally, 1967).

[21] See Douglas Rae, *The Political Consequences of Electoral Laws* (New Haven: Yale University Press, 1967); Taylor and Herman, "Party Systems and Governmental Stability"; Wildgen, "The Measurement of Hyperfractionalization," p. 236. For other approaches see Richard A. Pride, "Origins of Democracy," in *Sage Professional Papers in Comparative Politics*, ed. Harry Eckstein and Ted Robert Gurr, 1 (No. 12, 1970), p. 708; Lawrence S. Mayer, "An Analysis of Measures of Crosscutting and Fragmentation," *Comparative Politics*, 4 (April, 1972).

[22] This index is a variation on Przeworski and Sprague's measure of Party System Institutionalization. See Adam Przeworski and John Sprague, "Concepts in Search of Explicit Formulation: A Study in Measurement," *Midwest Journal of Political Science*, 15 (May, 1971), 183–218.

> moves; stable partisan patterns do not exist to depress the complications created by a large number of parties, and *vice versa*.

If these projections are correct, the joint occurrence of high fractionalization and instability may produce an additional degree of information uncertainty that can not be predicted from knowledge of the effects of either variable examined separately or in an additive model. This relationship can be perceived as a multiplicative one. To test for its significance the fractionalization and instability scores for each parliament have been multiplied together creating a third variable that will be referred to as the $F_t \times S_t$ index.

Cleavage Conflict. The existing literature on Western party systems indicates that voters tend to align with parties according to the salient societal cleavages with which the parties are identified. Within parliament the parties represent the various cleavage constituencies by which they are supported. There are two vital elements to this representative function: (1) parties work to attain ministerial status so as to formulate, enact, and administer legislation that is favorable to their cleavage constituencies; (2) parties act in a manner that advances their constituency rather than another constituency, so there are limits on the bargains that they can strike in attaining power.[23]

Because of these two dimensions of representation, the cleavage system is both a major source of the quest for power and, at the same time, a major constraint on the behavior that is possible in the quest. In the party negotiations to form cabinets, parties must be willing to compromise on some cleavage related policies in order to join in a majority coalition and gain cabinet status. Some compromise can be justified if the cabinet positions gained thereby allow the party to largely satisfy the policy demands of its cleavage constituency. The greater the compromise required to enter a coalition, however, the more difficult the compromise is to justify. Ministerial status may be achieved but at the expense of the cleavage constituency, jeopardizing the party's future electoral success. This places a constraint on the willingness of party leaders to bargain: the greater the cleavage conflict between parties, the greater the constraints on *a priori* willingness to bargain.

Based on this perspective, party system cleavage conflict is used as an indicator of the *a priori* willingness to bargain.[24] As a rough measure of party system conflict, the degree of cleavage conflict index (*DCC*) has been employed:

$$DCC = \frac{\sum_{i=1}^{N_C} V_{C_i}}{N_C},$$

where

N_C = the number of salient cleavages along which parties are distributed in a given parliament as determined by a reading of the relevant literature for each country,
V_{C_i} = the variance of party positions on cleavage i in the given parliament and
$V_C = \sum f_j\%(X_j - \bar{X})^2$,

given that

X_j = an evaluative position given by the author to party j on cleavage i in the parliament being examined,
$\bar{X}$ = the mean of the X_j's,
$f\%$ = the percentage of legislative seats associated with party j in the parliament.

This index measures the average variance of parties along those cleavage dimensions that were salient for a given parliament.[25] The variance measure weights a party's position by the number of legislative seats associated with it. Low conflict scores indicate that the major parties cluster closely together and thus, by inference, are willing to bargain with one another. High conflict scores indicate that the major parties are more widely dispersed and thus there should be a less generalized *a priori* willingness among them to bargain.

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[23] See Seymour Martin Lipset and Stein Rokkan, "Cleavage Structures, Party Systems, and Voter Alignments: An Introduction," in *Party Systems and Voter Alignments: Cross-National Perspectives*, ed. Lipset and Rokkan (New York: The Free Press, 1967); Seymour Martin Lipset, "Political Cleavages in 'Developed' and 'Emerging' Polities," in *Mass Politics: Studies in Political Sociology*, ed. Erik Allardt and Stein Rokkan (New York: The Free Press, 1970); Middleton, *French Political System*, pp. 62–133.

[24] Similar interpretations are reflected in Bracher, *The German Dictatorship;* Groennings, "Notes Toward Theories of Coalition Behavior in Multiparty Systems," pp. 445–465; Sartori, "European Political Parties" in LaPalombara and Weiner, *Political Parties and Political Development*.

[25] This index is similar to the one used by Taylor and Herman in "Party Systems and Governmental Stability," p. 34. Unlike Taylor and Herman, however, I have employed more than one cleavage dimension in my calculations. I do not maintain this measure is an optimal one. An exhaustive treatment of cleavage systems as constraints on party bargaining should include such elements as (1) the intensity of party conflict on cleavage dimensions; (2) the degree to which parties share similar relative positions on various cleavage dimensions (in other words, are cleavages cumulative or cross-cutting); (3) differences between parties' intensity and their perceptions of cleavage conflict. These factors as well as party positions on cleavage dimensions should be incorporated eventually into an Index of Cleavage Conflict. For the present, data and measurement problems inhibit this. On some of the problems see Douglas W. Rae and Michael Taylor, *The Analysis of Political Cleavages* (New Haven: Yale University Press, 1970).

To determine which cleavages are salient in a given parliament and what the party positions are on these cleavage dimensions, the literature on cleavages and party systems was reviewed. The evaluations of the salient cleavages and the location of each party on these cleavage dimensions were examined by two scholars in the field and appropriate changes were made where necessary. Among the party system conflict scores, which range from 1.75 to 3.05, the largest scores were obtained by the First Austrian Republic, Denmark, Weimar Germany, and interwar Sweden, all of which had at least one parliament with a score above 2.90; the smallest scores were obtained by Australia, the Second Austrian Republic, Belgium, The Federal German Republic, and postwar Sweden, all of which had at least one parliament receive a score below 1.90. As a basis for evaluating the author's work, Table 3 presents the highest and lowest score obtained by a parliament for each country.

Table 3. Party System Conflict Scores

Country	Low Score	Year	High Score	Year
(1) Australia	1.85	1949+	2.09	1934
(2) Austria	1.85	1949+	3.05	1923
(3) Belgium	1.845	1925	2.46	1946
(4) Canada	2.12	1921	2.42	1962
(5) Denmark	1.91	1947	2.90	1945
(6) Finland	1.95	1966	2.73	1922
(7) France	2.04	1928	2.91	1956
(8) Germany	1.75	1965	2.92	1930
(9) Great Britain	2.33	1918	2.63	1923
(10) Iceland	2.23	1927	2.63	1959
(11) Ireland	1.91	1961	2.23	1951
(12) Italy	2.16	1946	2.53	1953
(13) Luxembourg	2.04	1968	2.56	1948
(14) Netherlands	2.00	1956	2.31	1933
(15) New Zealand	2.11	1931	2.36	1922
(16) Norway	2.13	1965	2.88	1924
(17) Sweden	1.83	1948	2.98	1920

Cabinet Coalitional Status. In this study, the following operational definition of cabinet coalitional status has been employed:

(1) The percentage of legislative seats held by each parliamentary party is determined.

(2) It is determined whether the parties in the cabinet hold a majority of the legislative seats within parliament.

(3) If the parties in the cabinet do hold a majority of the seats in parliament, *but* if the subtraction of any party from the cabinet would deprive the colition of its majority status, the cabinet is termed a *minimum winning cabinet* (*mwc*).
   a. All such cabinets are given a score of zero.
   b. Since all parliaments with a majority party have been omitted from the analysis in this article, all minimum winning cabinets contain at least two parties.

(4) If the parties in the cabinet do not hold a majority of the seats in parliament, the cabinet is designated a *less than minimum winning cabinet* ($<mwc$). Such cabinets are also referred to as undersized or minority cabinets.
   a. Among less than minimum winning cabinets we are interested in determining the extent to which these cabinets deviate from minimum winning status.
   b. The following formula is used to determine the extent of this deviance: $<mwc = -D_{mwc} = N_a[\sum w_a]$.
      1. where w_a = the weight (percentage of legislative seats) of those parties whose addition to the cabinet would make it a minimum winning coalition. Since more than one set of such parties exists, choose that set whose $\sum w_a$ is smallest, given that it would still produce a minimum winning cabinet.
      2. where N_a = the number of parties whose addition to the cabinet would produce the necessary $\sum w_a$ to produce a cabinet of smallest minimum winning status. In case of ties choose that N_a which is smallest.

(5) If the parties in the cabinet hold a majority of the seats in parliament, and the subtraction of some party(s) will *not* deprive the coalition of its majority status. then the coalition is designated a *greater than minimum winning cabinet* ($>mwc$). Such cabinets are also referred to as oversized cabinets.
   a. Among greater than minimum winning cabinets, we are interested in determining the extent to which the cabinet deviates from (surpasses) minimum winning status.
   b. The following formula is used to determine the extent of this deviancy: $>mwc = +D_{mwc} = N_E[\sum w_E]$

   where w_E = the weight (percentage of legislative seats) of those parties that could be subtracted from the cabinet and its majority status maintained. Where more than one set of such extra parties exists, choose that set with the largest $\sum w_E$; and where N_E = the number of parties that could be subtracted from the cabinet and its majority status maintained.

(6) Summary. These procedures give us an indicator of the coalitional status of cabinets for nonmajority party parliaments. A score of zero indicates that a cabinet is of *mw* status. A negative score indicates that a cabinet is of less than *mw* status. A positive score indicates that a cabinet is of greater than *mw* status. The larger the absolute score, the more a cabinet deviates from minimum winning status.

In measuring the deviation of cabinets from minimum winning status two dimensions were

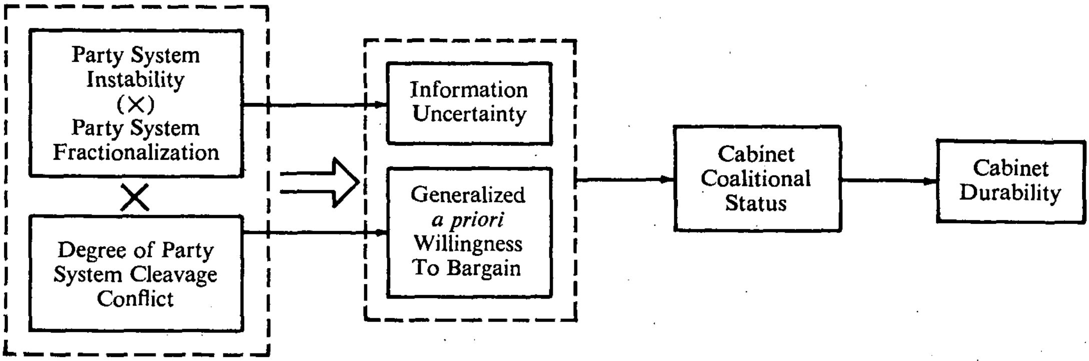
Party System Instability
(×)
Party System Fractionalization
×
Degree of Party System Cleavage Conflict
Information Uncertainty
Generalized *a priori* Willingness To Bargain
Cabinet Coalitional Status
Cabinet Durability

Figure 3

employed: the number of parties involved in the deviation and the overall weight of the parties involved in the deviation. This approach reflects the view that deviation is not just a question of weight but also of the number of parties.

Cabinet Durability. The final operational problem is delineating cabinet durability. Past scholars, notably Jean Blondel and Taylor-Herman, have stated that a cabinet exists so long as there is no change in the parties in the cabinet or in the Prime Minister.[26] For present purposes it is questionable whether this definition is useful. For example, does the death of a prime minister really constitute in any useful sense a change in cabinets if the new prime minister is of the same party and there is no other change in the cabinet? More important, definitions of concepts really depend on the theory within which one is working. In the present theory the key element is the coalitional behavior of parties. Because of this, the most consistent approach is to define cabinet durability solely in terms of the maintenance of the party coalition that exists within the cabinet. Thus the operational definition of cabinet durability is:

> A cabinet exists so long as there is no change in the parties that compose the cabinet.

This definition is consistent with the theory stated in this study since the theory centers on the conditions under which party coalitions arise and on how durable different types of coalitions will be.[27]

Party Systems and Cabinet Durability

According to the theory outlined in the first section and interpreted in the second, the coalitional status of a cabinet is determined by the nature of the bargaining conditions that exist in parliament. The characteristics of a parliament's party system constitute one set of factors determining the nature of these bargaining conditions. Consequently, a parliament's party system should influence the coalitional status of the cabinets that form in a parliament. Three key party system characteristics were posited to influence the bargaining conditions within a parliament. Including these three elements, the thesis can be diagrammed as in Figure 3. Certainly, the three party system characteristics specified in this figure are not the only factors that influence the bargaining conditions within a parliament. For example, information certainty may increase the longer a parliament lasts, the more structured the interaction of party leaders, the more frequent the sessions of parliament, or the more professionalized the legislature. Likewise, the willingness to bargain will not be a simple function of partisan cleavage conflict at the electoral stage but will be subject to shifts throughout a parliamentary session on the basis

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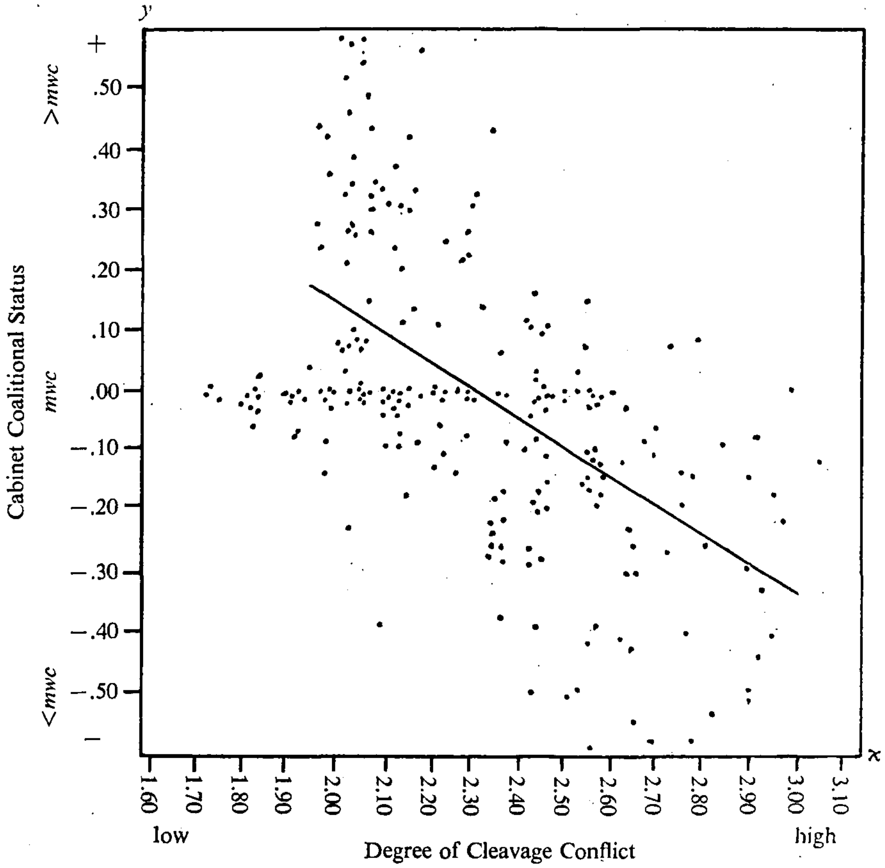
[26] Blondel, "Party Systems and Patterns of Government," pp. 190–191; Taylor and Herman, "Party Systems and Governmental Stability," p. 29.

[27] This operational definition is not the only conceivable one that is consistent with the theory of cabinet durability outlined in this study. The key question is this: what constitutes a breakdown or change in a party coalition within the cabinet? A variety of answers can be given: (1) any alteration in the distribution of payoffs (ministerial seats) among parties could be viewed as a breakdown in the old coalition and the formation of a new one, even if the parties in the cabinet retained the same number of seats; (2) only "significant" alteration in payoff distribution could constitute a change; (3) only changes in the parties composing the cabinet could be viewed as a change in cabinet coalitional status. There is no easy way to choose among these approaches. My position is that the greater the number of defensible operational definitions that sustain the predictions of the theory, the more faith we can have in the theory. For the present, however, I am constrained by problems of data collection and verification. Consequently, in this study I have limited my definition to the third approach.



$r = -.516$ $N = 223$
$\bar{x} = 2.323$ $a = 1.0108$
$\bar{y} = -.0225$ $b = -.4454$

Figure 4. Covariation of Party System Conflict and Cabinet Coalitional Status

of external and internal events. Any attempt to predict or postdict cabinet coalitional status or cabinet durability on the basis of these three characteristics alone obviously will not account for all variations. In addition, the theory is difficult to test because of the complex interaction hypothesized between the various parliamentary bargaining conditions. The theory does not predict simple additive relations; rather, given different degrees of *a priori* willingness to bargain, information certainty may produce exactly the opposite behavioral patterns. This is a fairly difficult process to examine. The following sections present an exploratory test of the theory, but the conclusions must be seen as tentative, to be augmented through the application of other cross-national research methods, case study analyses, and experimental research in small group settings.

Party Systems and Cabinet Coalitional Status: Simple Relationships. The first test of the theory is to examine the simple relationships between party system characteristics and cabinet coalitional status. In these tests, cleavage conflict is viewed as an indicator of the generalized *a priori* willingness to bargain; the predicted relationship can be inferred from Figure 1. Party system fractionalization and instability are indicators of information uncertainty: in conditions of low fractionalization and low instability, minimum winning cabinets should form; as fractionalization and instability increase, cabinets should deviate from minimum winning status.

Figure 4 presents the pattern that obtains between party system conflict and cabinet coalitional status in multiparty parliaments, 1918–1972. This pattern indicates that there is a general

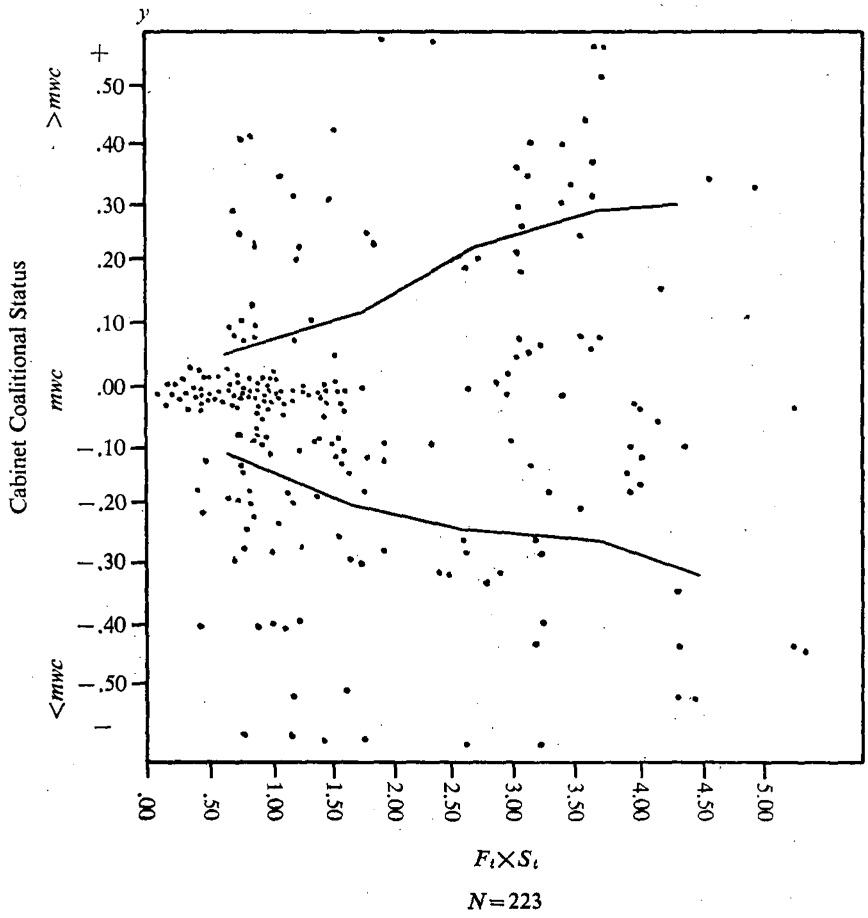
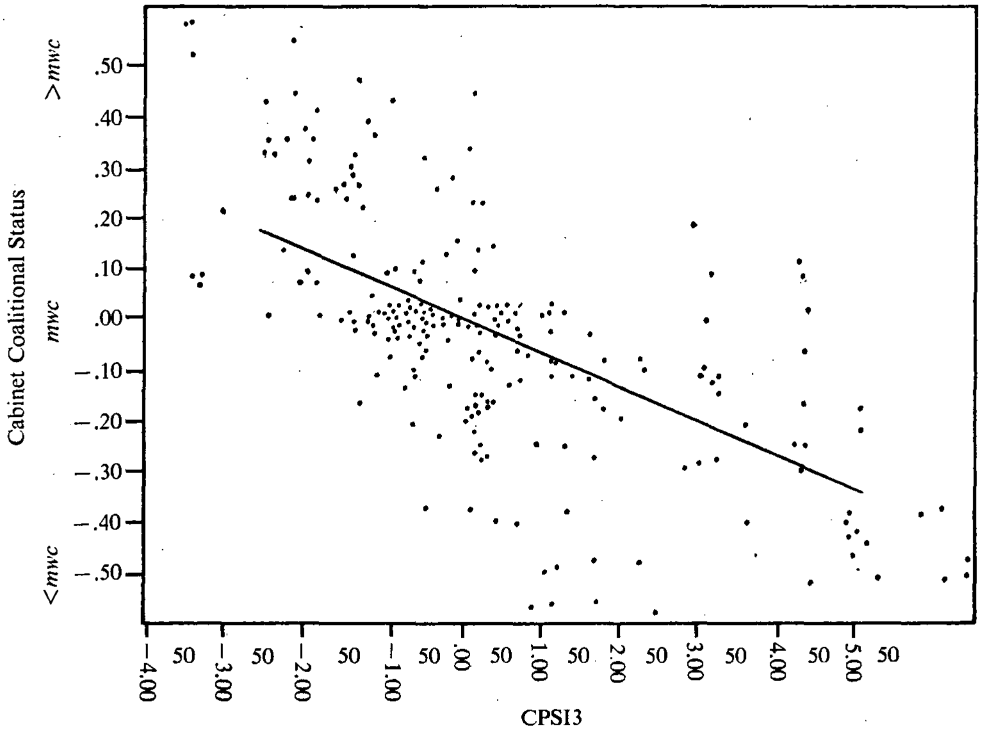


Figure 5. Covariation of $F_t \times S_t$ and Cabinet Coalitional Status

relationship between party system conflict and cabinet coalitional status. Oversized cabinets form predominantly in the area of low to moderate cleavage conflict. Undersized cabinets form primarily in areas of moderate to high cleavage conflict. Minimum winning cabinets form under virtually the entire range of cleavage conflict conditions although they predominate in the area of low to moderately high cleavage conflict. Overall, the correlation between party system conflict and cabinet coalitional status is −.516: cleavage conflict accounts for approximately 26 per cent of the variance in cabinet coalitional status.

Figure 5 indicates that there is a very general relationship between cabinet coalitional status and the interaction of fractionalization with party system instability. To analyze the linear relation between cabinet deviation and $F_t \times S_t$ we can drop the oversized-undersized designation, and find that a .353 correlation coefficient obtains. Similar patterns hold when we examine the statistical relationships between cabinet coalitional status and fractionalization or instability taken separately. The coefficients are .317 and .319 for fractionalization and instability, respectively, when correlated with the absolute deviation of cabinets from minimum winning status. Fractionalization, instability, and the interaction term all enter in a regression equation together at a statistically significant level. The multiple regression coefficient is .370, indicating that party system fractionalization, instability, and their interaction account for approximately 14 per cent of the absolute deviation of cabinets from minimum winning status.

These findings support the hypothesis that party system fractionalization and instability influence the deviation of cabinets from minimum winning status. Minimum winning cabinets do predominate in parliaments experiencing low party system fractionalization and instability, whereas deviant cabinets predominate in the more fractionalized and unstable party systems. Nevertheless, the



$r = -.561$ $N = 223$
$x = .215$ $a = -.00875$
$y = -.023$ $b = -.0646$

Figure 6. Covariation of Cabinet Coalition Status and CPSI3

analysis does not constitute a full test of the theory. It indicates nothing about the *direction* of cabinet deviation from minimum winning status and little about the *degree* of cabinet deviation. According to the theory, both the direction and degree are products of the complex interaction of fractionalization, instability, or both with party system conflict.

Party Systems and Cabinet Coalitional Status: Complex Relationships. The more complex predicted relationships between party system characteristics and cabinet coalitional status can be summarized as follows: at low levels of cleavage conflict, party system fractionalization and instability should be associated with oversized coalitions; as the degree of cleavage conflict between parties increases, this tendency should reverse and, at the higher levels of cleavage conflict, party system fractionalization and instability should be associated with undersized coalitions. To test for the relationship, the following strategy is employed: the party system cleavage scores are standardized—a procedure that does not change the strength of the relationship with cabinet coalitional status; it does, however, alter the nature of the cleavage conflict score: low scores are now negative and high conflict scores are now positive. These standardized scores have been multiplied with the unstandardized scores of party system fractionalization, instability, and $F_t \times S_t$ for each parliament. The resulting indices will be referred to as Complex Party System Interaction Indices: index 1 (CPSI1) represents the interactive score between party system conflict (*DCC*) and party system fractionalization; CPSI2 represents the interactive score between party system conflict and party system instability; CPSI3 represents the interaction of party system conflict with $F_t \times S_t$.

Large negative CPSI scores indicate parliamentary party systems that experience moderate to low cleavage conflict (thus the negative scores) and are highly fractionalized, unstable, or both, depending on the index employed. Using a variation on Sartori's terminology in order to characterize such parliaments,[28] these parliaments can be referred to as experiencing unstable and *depolarized* hyperpluralism: the party system is unstable, basically moderate in terms of interparty conflict (i.e., major parties do not approach the ideological extremes), but highly fractionalized in number and strength of parties. In such a parliament, oversized cabinets are expected. France throughout much of the Third and Fourth Republics illustrates unstable, depolarized hyperpluralism.

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[28] Sartori, "European Political Parties: The Case of Polarized Pluralism," pp. 137–140.

Large positive CPSI scores indicate parliaments that experience moderate to high cleavage conflict between parliamentary parties (thus the positive scores) and are highly fractionalized, unstable, or both. For simplicity this condition can be referred to as unstable, *polarized* hyperpluralism: the parliamentary party system is unstable, highly fractionalized, and polarized with the major parties dispersed toward extreme positions on the salient cleavage dimensions. In such systems, undersized coalitions are expected. France from 1956 to 1958 and Weimar Germany throughout much of its existence illustrate unstable polarized hyperpluralism.

The intermediate CPSI scores indicate parliaments that experience lower degrees of party system fractionalization or instability: parliaments that obtain low but positive scores possess more polarized party systems; parliaments that obtain low but negative scores possess less polarized systems. The former can be referred to as experiencing stable, *polarized* pluralism; the latter experience is stable *depolarized* pluralism. In such parliaments, minimum winning cabinets are expected. First Republic Austria during the early 1920s illustrates stable polarized pluralism. Recent experience in West Germany illustrates stable *depolarized* pluralism.

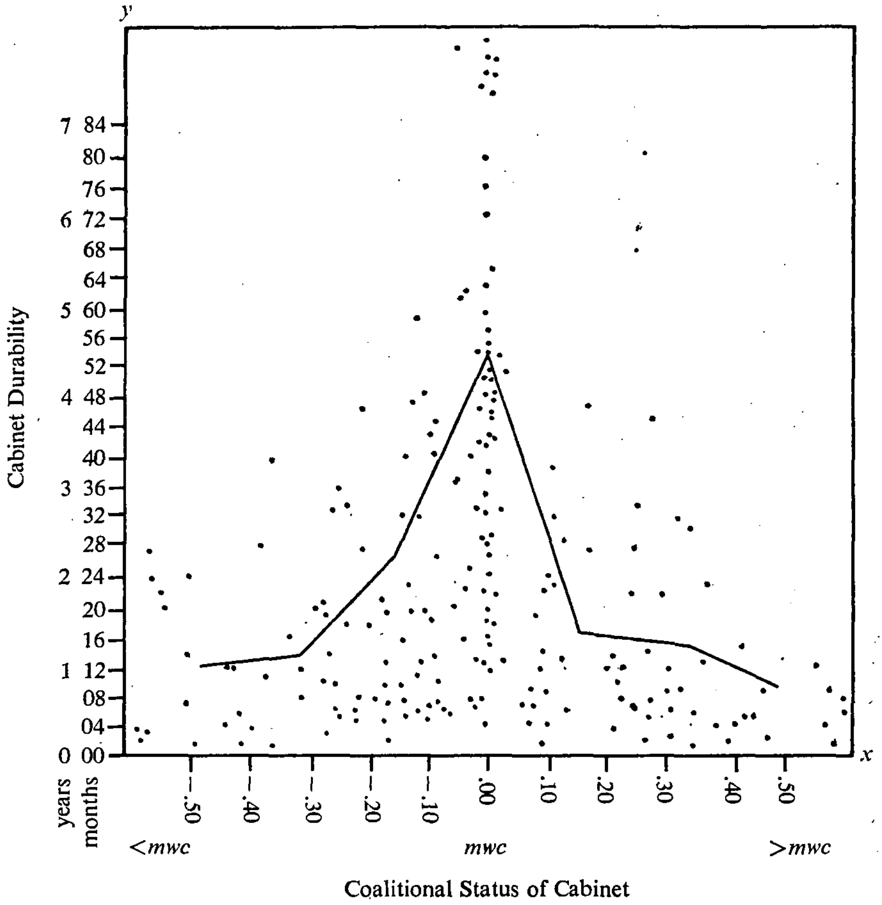
The appropriate interaction terms have been calculated for all multiparty parliaments, 1918–1972. As an example of the resulting relationship, Figure 6 presents the covariation of cabinet coalitional status and CPSI3. In terms of its general form as predicted, the relationship is curvilinear, although the curvilinear characteristic obtains clearly only in conditions of high cleavage conflict. To capture this curvilinear form employing linear statistics the CPSI3 scores have been logarithmically transformed; the resulting product moment coefficient is −.626.

Similar patterns obtain for the covariation between cabinet coalitional status and both CPSI1 and CPSI2; the product moment coefficient associated with each independent variable (logged) is −.580 and −.614, respectively. The multiple regression coefficient obtaining when these two variables are entered in an additive model is .620. The inclusion of CPSI3 (logged in a regression equation with CPSI1 (logged) and CPSI2 (logged) raises the coefficient to .632 with all three variables entering the equation at a statistically significant level (.01). Neither DCC nor DCC (logged) will enter the regression equation at the .01 level. These findings suggest three conclusions:

(1) As we move from the simple relationship between cleavage conflict and coalitional status to the more complex interactive patterns suggested by the theory, we observe a 15 per cent increase in variance explained: overall the three CPSI variables (logged) account for approximately 40 per cent of the variance in cabinet coalitional status.
(2) It is the interactive occurrence of fractionalization and instability together, and their joint interaction with cleavage conflict that seems most influential in determining cabinet coalitional status, with CPSI3 (logged) accounting for approximately 39 per cent of the variance in cabinet coalitional status.
(3) Once its complex interaction with party system fractionalization and instability is considered, cleavage conflict has no significant independent effect on cabinet coalitional status.

These findings support the theory that the complex interaction among party system fractionalization, instability and cleavage conflict determines cabinet coalitional status. This interaction process can be captured in a very parsimonious yet powerful fashion through the use of one variable (CPSI3-logged). The substantive relationship can be summarized as follows: oversized cabinets form primarily in unstable, depolarized hyperpluralism; undersized cabinets form in conditions of unstable, polarized hyperpluralism; minimum winning cabinets form under conditions of stable, polarized pluralism and stable depolarized pluralism. Of course, a good deal of variance does remain unexplained within these general patterns. Given the nature of the theory, unexplained variance is to be expected: party system characteristics influence only the general range of parliamentary bargaining conditions that will prevail; within that range an ongoing process of bargaining, maneuvering and cabinet transformations may take place. The critical question therefore is whether the findings matter: is cabinet coalitional status related to cabinet durability?

Cabinet Coalitional Status and Cabinet Durability. Figure 7 presents the relationship that obtains between cabinet coalitional status and cabinet durability for the 17 parliaments during peacetime years, 1918–1972. As can be seen, the general aggregate predictions derived from the theory are supported by the data. Minimum winning cabinets are more durable than oversized or minority cabinets. In addition, the durability of cabinets is a function of the degree to which cabinets deviate from minimum winning status. This relationship is curvilinear. Employing w^2 (an



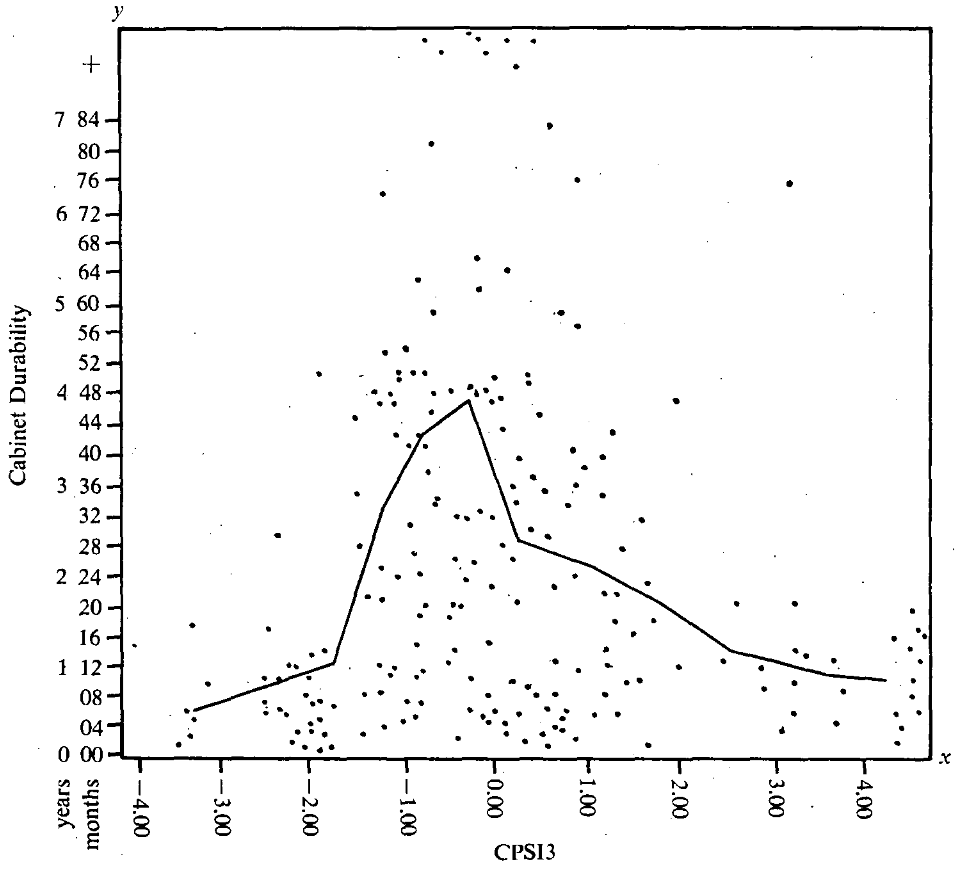
$N = 223$
$w^2 = .31$

Figure 7. Covariation of Cabinet Durability and Cabinet Coalitional Status

analysis of variance statistic that indicates the percentage of variance accounted for in curvilinear relationships), cabinet coalitional status explains approximately 31 per cent of the variance in cabinet durability.

In order to examine the relationship employing linear statistics, I have collapsed Figure 7 and utilized the absolute deviation of cabinets from minimum winning status as the independent variable. In addition, cabinet durability has been logarithmically transformed. Using these procedures, cabinet coalitional status statistically explains approximately 24 per cent of the variance in cabinet durability (logged). A second procedure which can be employed to estimate the significance of this relationship with linear statistics is to transform the independent variable according to the following procedure: $X' = 1/(1+x)$. If this transformation is conducted, cabinet coalitional status accounts for approximately 27 per cent of the variance in cabinet durability (logged). Judging from these various statistical procedures, we can conclude that cabinet coalitional status does account for a sizable proportion of the variance in cabinet durability. These findings support the hypothesis that cabinet durability is at least partially a function of cabinet coalitional status.

Party Systems and Cabinet Durability. Thus far we have seen that the coalitional status of cabinets is at least partially a function of the parliamentary party system, whereas cabinet durability is a function of cabinet coalitional status. It should follow, therefore, that cabinet durability is an indirect function of the parliamentary party system. Figure 8 presents the pattern that obtains between CPSI3 and cabinet durability. Employing w^2 analysis, we note that the complex interaction of party system fractionalization, instability, and



$N = 223$
$w^2 = .235$

Figure 8. Covariation of Cabinet Durability and CPSI3.

cleavage conflict (CPSI3) explains 23 per cent of the variance in cabinet durability among peacetime multiparty parliaments, 1918–1972. When the CPSI3 (logged) scores are collapsed around the modal point in Figure 8, the product moment coefficient between cabinet durability and CPSI3 (logged) is −.4402. The similar product moment coefficients for CPSI1 (logged) and CPSI2 (logged) are −.293 and −.195 respectively. The multiple regression coefficient for these two independent variables in an additive model is .295. Adding CPSI3 (logged) to the regression equation raises the coefficient to .4602.

These findings indicate that cabinet durability is at least partially a function of the nature of the parliamentary party system. If multiple regression analysis is employed to estimate very roughly the strength of the overall relationship, the complex interaction of the party system characteristics accounts for approximately 22 per cent of the variance in cabinet durability (logged). More importantly, one variable alone (CPSI3 logged) accounts for approximately 20 per cent of the variance in cabinet durability. The substantive relationship can be summarized as follows: durable cabinets form under conditions of stable, depolarized pluralism and stable, polarized pluralism; transient cabinets form under conditions of unstable *depolarized* hyperpluralism and unstable, *polarized* hyperpluralism.

Clearly, the theory developed in earlier sections has yielded a parsimonious yet reasonably powerful statistical explanation of the relationship between cabinet durability and parliamentary party systems. This conclusion is particularly significant in light of three considerations: (1) the analysis here applies only to multiparty parliaments, a set of parliaments for which cabinet durability has been largely unexamined heretofore; (2) cabinet durability has not been averaged by parliament and thus a good deal of unexplained variance is due to within-parliament variation in cabinet durability; (3) the timespan covered is quite wide and includes some deviant cases that could be justifiably omitted in the context of the theory but which, in the current study, I have included since exclusion would require a case-by-case discussion that cannot be attempted here. In addition, the findings are substantively significant because they can clarify our understanding of the failure of other theories to explain cabinet durability.

A "Nonobvious" Conclusion. In *Division and Cohesion in Democracy*, Harry Eckstein writes that

> the most stable democracy is one in which men disagree, without intensity and within a narrow range, on specific issues, widely share basic orientations, and have politically weak segmented identifications. The least stable case has the opposite characteristics: extensive and intense disagreements, flowing from highly divergent orientations and strong segmental cleavages.

In defining stable democracies, Eckstein maintains that they are polities "marked by what might be called intrasystem stability, especially the stability of governments." His illustrative index of governmental stability is the durability of cabinets.[29] Eckstein's position is the primary supplement in the literature to Lowell's thesis. It is an intriguing position and, indeed, it is the position that I held as I began my analysis of cabinet durability. Nothing could be more obviously true than the assertion that cabinet durability is a function of the degree of cleavage conflict or polarization that exists in a society or in a parliament.

Despite the plausibility of the hypothesis, systematic cross-national analysis does not appear to confirm it. Analyzing 19 Western nations, 1946–1970 (including majority party systems), Michael Taylor and V. H. Herman found only a slight relationship between partisan ideological differences and cabinet durability.[30] The present study reinforces their findings. The product moment correlation coefficient between cabinet durability (logged) and party system cleavage conflict (DCC) among multiparty parliaments, 1918–1940, 1945–1972 is only −.111. This does not change, furthermore, if we examine the relationship for curvilinear patterns: the w^2 coefficient is .035, and the product moment correlation coefficient between cabinet durability and DCC (logged) is −.117. Clearly, the degree of cleavage conflict or party system polarization by itself does not greatly influence cabinet durability. Why has this "obvious" generalization been rejected?

The resolution of this dilemma lies, I believe, in the theory outlined in previous sections: the low relationship between cleavage conflict and cabinet durability is due to the nature of coalition formation and maintenance in multiparty parliaments. An essential determinant of cabinet durability is the coalitional status of the cabinet. A minimum winning cabinet will generally be durable; the more a cabinet deviates from minimum winning status, the less durable it will be. The formation of *deviant* cabinets is not a simple function of cleavage conflict. It depends on a complex interaction of cleavage conflict with party system fractionalization and instability. There is a difference in the *direction* of cabinet deviation under different cleavage conflict conditions. If the degree of cleavage conflict is low, cabinets in a fractionalized and unstable party system will deviate toward an oversized coalitional status. If cleavage conflict is high, cabinets in a fractionalized and unstable party system will deviate toward an undersized coalitional status. In *either* case, cabinets will tend to be transient.

Once we perceive the relationship in this manner, there is a sense in which Eckstein's general theory may be more plausible. A critical question is this: should the same meaning, interpretation or systemic consequences be attached to transient oversized cabinets and to transient minority cabinets? Are there potentially detrimental consequences that obtain from nondurable minority cabinets that do not accrue (or at least do not accrue as often or as immediately) from nondurable oversized cabinets? For example, in terms of partisan or policy continuity, the avoidance of disruptive elections, or the maintenance of widespread popular attachment to the cabinet ministry, it is at least conceivable that transient oversized cabinets are preferable to transient minority cabinets. If greater significance is attached to transient minority cabinets, then Eckstein's theory assumes a more defensible stance since minority cabinets form primarily in the more conflictual or polarized cleavage systems.

In addition, it is important to realize that the association between cleavage systems and cabinet durability is not just a question of the relationship of cleavage polarization to cabinet durability. A cleavage system has at least three separate characteristics: (1) the complexity of the cleavage system structure; (2) the institutionalization of cleavage structure attachments; (3) the degree of cleavage dimension conflict. The complexity of a cleavage system's structure refers to the number of cleavage dimensions that are salient in a polity and the degree to which these dimensions cut across one another. The degree of cleavage dimension conflict refers to the intensity and extremity of electoral polarization on the salient cleavage dimensions. Cleavage structure institutionalization refers to the degree to which (1) the cleavage dimensions salient to an electorate are continuous overtime; (2) cleavage positions of the electorate on each cleavage dimension have solidified; and (3) these cleavage positions have been formalized through stable electoral attachments to specific political parties.

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[29] Harry Eckstein, *Division and Cohesion in Democracy: A Study of Norway* (Princeton, N.J.: Princeton University Press, 1966), pp. 38, 17–18.

[30] Taylor and Herman, "Party Systems and Governmental Stability," pp. 32–35.

If we accept the proposition that a parliamentary party system is partially a product of a polity's cleavage system,[31] then the three cleavage characteristics specified above are factors that give rise to the party system characteristics analyzed in this study. Cleavage system complexity influences how fractionalized a party system will be. The extremity of electoral polarization on cleavage dimensions influences the degree of cleavage conflict that will characterize a parliamentary party system. Cleavage system institutionalization influences how stable the parliamentary party system will be. Interpreted in this manner, the findings presented in this study do suggest a relationship between cleavage systems and cabinet durability. Nondurable cabinets will exist in polities possessing a highly complex and uninstitutionalized cleavage system. If the cleavage system is highly conflictual or polarized the transient nature of cabinets will be due to a tendency toward minority cabinets. If the cleavage system is not highly conflictual, the transient nature of cabinets will be due to a tendency toward oversized cabinets.

Toward a General Theory

The purpose of this study has been to present a theory of cabinet formation and maintenance in multiparty parliaments and to present initial tests of hypotheses derived from the theory. The significance and underlying thesis of the theory is summarized best perhaps by the British parliamentary analyst, W. L. Middleton, writing in 1932:

> No political system can evade the problem of authority. It must be a principle aim of any constitution, even of the most democratic, to produce Governments which shall govern, which shall possess real initiative and command. . . . If a wide stretch of experience be reviewed it will be found that the power of the Executive . . . has varied very greatly from one legislature to another. . . . The explanation of these variations is not far to seek. . . . A ministry exercises its power . . . not directly, but through the majority to which it looks for support. If a Government has a confident and continuous command of its majority, it can be firm in its handling of the Chamber. It can be a Government which governs. If, on the contrary, it has only a loose hold over its majority, it is less able to impose respect for the proper attributes of the Executive. It is the variation in the efficiency of this machinery which accounts for the fluctuation of executive authority. Everything depends on the majority . . . on the degree of confidence in the Ministry which it possesses. . . .

And, as Middleton concludes, "where all majorities are coalitions, this means that everything depends on the nature of the coalition. The Executive is strongest when the Government is the trusted agent of a coalition of natural allies. . . ."[32] In a very real sense the theory and analysis of this study is an attempt to explicate, operationalize, and refine Middleton's insight. In the process, A. Lawrence Lowell's thesis is modified.

In his work, Lowell stressed the consequence of a cabinet's composition for cabinet durability. In particular, he argued that "A cabinet which depends for its existence on the votes of the Chamber can pursue a consistent policy with firmness and effect only when it can rely for support on a compact and faithful majority." Unfortunately, in making this argument and in investigating its consequences, Lowell was not working within a broader set of clearly and explicitly articulated assumptions about partisan motivations within parliament. He did not attempt to systematically explicate the meaning of "compact and faithful majority."[33] He did not clearly specify why one

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[31] See Erik Allardt and Stein Rokkan, eds., *Mass Politics* (New York: The Free Press, 1970); Seymour M. Lipset and Stein Rokkan, eds., *Party Systems and Voter Alignments: Cross-National Perspectives* (New York: The Free Press, 1967); Stein Rokkan, *Citizens, Elections, Parties* (New York: David McKay Company, 1970); Robert A. Dahl, ed., *Political Oppositions in Western Democracies* (New Haven: Yale University Press, 1966).

[32] Middleton, *French Political System*, pp. 151–154. For relevant discussions on the nature of cabinet governments and their significance for the regime stability of parliamentary democracies, see W. G. S. Adams, "Has Parliamentary Government Failed?," *The Modern State*, ed. Mary Adams (New York: The Century Company, 1933); Gerhard Loewenberg, "The Influence of Parliamentary Behavior on Regime Stability," *Comparative Politics*, 3 (Jan., 1971), 177–179; Ernest Fraenkel, "Historical Handicaps of German Parliamentarianism," *The Road to Dictatorship*, trans. Lawrence Wilson (London: Oswald Wolff, 1964), pp. 27–37; Karl Dietrich-Bracker, "Problems of Parliamentary Democracy in Europe," in *Comparative Legislative Systems: A Reader in Theory and Research*, ed. Herbert Hirsch and M. Donald Hancock (New York: The Free Press, 1971), pp. 343–359; William L. Shirir, *The Collapse of the Third Republic* (New York: Simon and Schuster, 1969); Richard K. Ullman and Sir Stephen King-Hall, *German Parliaments: A Study of the Development of Representative Institutions in Germany* (New York: Frederick A. Praeger, 1954), esp. pp. 87–91; Philip M. Williams, *Crisis and Compromise: Politics in the Fourth Republic* (Hamden, Conn.: Archon Books, 1964).

[33] Lowell, *Governments and Parties*, 73.

type of cabinet composition might have a more faithful majority and the relationship of this faithfulness to the compact nature of the cabinet. He did not consider whether the conditions which would underlie the explanation of cabinet durability in single-party majority cabinets might have an analogous form in a multiparty system.

The linkage which Lowell missed, which I believe exists in the cabinet behavior of multiparty and majority-party systems, centers on the role of a cabinet's minimum winning status. In a multiparty setting, as I have emphasized, a minimum winning *coalition* of parties can form under certain conditions; if one does form, it probably will be quite durable. Analogously, a minimum winning cabinet can form in a majority party system. It will not be a *coalition* of parties. It will be a cabinet composed of one party. Nevertheless, it is of *minimum winning status* in the sense that (a) there is no unnecessary party in the cabinet and (b) the cabinet does control a majority of the seats in parliament. This perspective shows us a foundation for the construction of a general theory of cabinet formation and maintenance in parliamentary democracies. In the present study theory development has been limited to cabinet formation and maintenance in multiparty settings because the central concepts of the theory (such as bargaining conditions, information certainty, willingness to bargain, cabinet coalitional status) can be most clearly grasped in this more limited setting. It should be clear, however, that this focus in no way limits these concepts or the broader theory that they compose to a multiparty parliament.

Once the significance of minimum winning status is grasped, it is only a short step to discover that Lowell's thesis derives its power from having isolated within a majority party system a situation in which minimum winning cabinets are virtually guaranteed under normal conditions. In such an unfractionalized system, information certainty should be approached. Unless the majority party is unsure of its own voting cohesion (owing, for example, to a dramatically unstable party system), a single-party, majority cabinet will form. This cabinet will be of minimum winning status. The cabinet will not face internal dissension or disruption because of conflict between cabinet parties over ministerial payoffs to unnecessary parties; nor can the cabinet be overthrown by parties outside of the cabinet which coalesce to form a larger cabinet. The single party cabinet, as a minimum winning cabinet, will be quite durable.

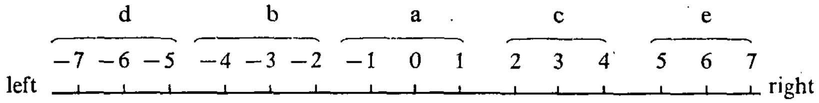
Seen in this context, the analysis of cabinet durability within majority party systems can be integrated into a larger theory along the form outlined in the first two sections. If we employ the concept of 'pluralism' to indicate parliaments experiencing low to moderate fractionalization and 'hyperpluralism' to indicate moderate to high fractionalization, we can summarize the propositions of a general theory as follows:

(1) In parliaments with party systems characterized by stable depolarized pluralism or stable polarized pluralism, minimum winning cabinets will form and be quite durable; the primary exception to this rule will be in extremely polarized multiparty parliaments where only minority cabinets are possible because of extreme interparty animosities; such cabinets will tend to be nondurable, falling primarily through the dissolution of parliament;
(2) In parliaments with party systems characterized by unstable depolarized hyperpluralism, oversized cabinets will form and be nondurable;
(3) In parliaments with party systems characterized by unstable, polarized hyperpluralism, undersized cabinets will form and be nondurable;
(4) A strong relationship between cabinet durability and partisan cleavage conflict or polarization should not exist: deviant (and thus nondurable) cabinets can form in polarized or depolarized party systems.

These propositions emphasize that durable cabinets can exist in multiparty settings as well as majority party settings. Lowell's generalization that cabinets in majority party systems will be durable can be subsumed in a more general theory of cabinet formation and maintenance. A key to durable government in peacetime parliamentary democracies is the minimum winning status of the cabinet; minimum winning cabinets can form in both majority party and multiparty parliaments.

Methodological Appendix

Index of Cleavage Conflict. In placing parties on the relevant cleavage dimensions, the following scale was used:



Positions on this scale were attributed to parties on the basis of the following guidelines:

(1) If the cleavage dimension related to basic policy dimensions such as economic intervention in the economy, secularization of the polity, etc., then the following meaning was imputed to the various categories of the model:

a—these are parties that are pro-system as it currently is, zero being the absolute center and −1

or +1 being parties that are center parties but that lean slightly toward the left (−) or toward right (+).
b—these parties want moderate restructuring of the social system in order to institute some new program in regard to cleavage *x*.
c—these are parties that are moderate in their opposition to change toward the left and in their opposition to the present system and hark back to some aspect of prior policies in search of the desirable policy on cleavage *x*.
d—these parties want fundamental, radical change of the social system in regard to cleavage *x*.
e—these parties are reactionary in opposition to leftist change and want to return to a societal position that they maintain realizes the traditions of the past.

(2) In some countries at certain times a critical salient cleavage is support of or opposition toward the political system itself. In those situations where this cleavage is salient the following interpretation is given.

a—those parties that accept the political system but very hesistantly
b—parties that moderately support the present system
c—parties that moderately oppose the present system
d—parties that are extremely supportive of the present system
e—parties that extremely oppose the present system

Within the various categories above, three different numerical positions can be given depending on whether a party falls clearly and precisely within the category designated or whether it leans slightly leftward or rightward within the given category.

My evaluations of party positions on the cleavage dimensions are contained in Dodd "Party Coalitions and Cabinet Government" (doctoral dissertation, University of Minnesota, 1972), Appendix A. After the publication of my evaluations in the dissertation, I became aware of similar efforts by two other political scientists, Kenneth Janda and John C. Thomas, both of whom have been kind enough to share their data with me. Janda analyzes party systems in 48 countries during the postwar years; Thomas analyzes party systems in 12 countries from the late 1800s to 1960. Between them the two studies cover 14 of the 17 countries in my study, all except Finland, Norway and Belgium. Both Janda and Thomas place parties on "issue" dimensions in which parties can receive scores ranging from −5 to +5. Among the issues they study are four economic issues, one issue related to clericalism, and two issues related to regime support.

Since the three most recurrent cleavages in the present study were conflicts over economics, clericalism, and regime support, the work by Janda and Thomas provides an independent basis by which to evaluate my scales. To obtain comparable codings, party positions on their four economic issues and on their two regime support dimensions have been averaged, creating one economic dimension and one regime support dimension. Their clericalism scale is used unaltered. The resulting product-moment coefficients are as follows:

		Interwar	Postwar	Total
Economic cleavage	Janda	*X*	.852 (32)	*X*
	Thomas	.810 (27)	.833 (25)	.814 (52)
Clerical cleavage	Janda	*X*	.800 (20)	*X*
	Thomas	.782 (12)	.779 (15)	.776 (27)
Regime Support cleavage	Janda	*X*	.954 (4)	*X*
	Thomas	.850 (8)	.800 (7)	.828 (15)

The parentheses indicate the number of parties on which the correlations are based. The numbers vary according to whether cleavages were deemed salient for a given country and according to whether complete data existed for given parties on the issue dimensions.

Considering that we used different coding specifications and different spatial dimensions, the product-moment coefficients are high, and these tests appear to provide a good basis for assuming the concurrent validity of our scales. For additional information on the coding procedures employed by Janda and Thomas see Kenneth Janda, *Comparative Political Parties: A Cross-National Handbook* (The Free Press, forthcoming), and John C. Thomas, "The Decline of Ideology in the West: A Longitudinal Study of the Changing Public Policy Orientations of Political Parties in 12 Nations" (Ph.D. dissertation, Northwestern University in progress). My thanks to Janda and Thomas for sharing their data with me.

Party System Stability Index. The PSS Index employed in this article is based on a sum of the *C* (continuity) scores for all new parliaments held in the preceding five years. Obviously, some other time span could have been selected. Because of this, all relevant hypotheses have been tested using the following alternate indices:

PSS-A—the stability score is the *C* score for parliament *t* only

PSS-B—the stability score is the sum of the C scores for parliaments t and $t-1$
PSS-C—the stability score is the sum of the C scores for parliaments t, $t-1$ and $t-2$
PSS-D—the stability score is the sum of the C scores for all parliaments held in the past five years
PSS-E—the stability score is the sum of the C scores for all parliaments held in the past ten years

The party system stability index employed in this article is PSS-D. The appropriate product moment coefficients for the other indices are reported in Dodd, *Party Coalitions and Cabinet Government*. All findings approximate those reported in this article.

Cabinet Coalitional Status. Other measures of cabinet coalitional status that are consistent with the theory could have been employed in this article. To examine the consequences of different measures, all relevant hypotheses have been tested using the following indices:

CCS1—cabinet coalitional status 1 (the index used in this study)
CCS2—cabinet coalitional status 2. The same procedures followed in CCS1 were maintained with these exceptions: (a) for less than minimum winning coalition, choose that $N_a \sum wa$ that is smallest; (b) for greater than minimum winning coalitions, choose that $N_e \sum w_e$ that is largest.
CCS3—cabinet coalitional status 3. The scores are based solely on $\sum wa$ or $\sum w_e$ without weighting by N_a or N_e. All other aspects of CCS1 are followed.

This study reports the results based on CCS1 but similar findings are approximated when either CCS2 or CCS3 are employed. See Dodd, *Party Coalitions and Cabinet Government*.

Of course, the above alternate operational definitions do not exhaust the possible alternate approaches to cabinet coalitional status. For example, Robert Axelrod has developed a very intriguing concept of minimal connected winning coalitions that moves coalitional analysis from a nominal to an ordinal level: coalitions must not only be of minimum winning status but also composed of parties that are adjacent on ideological or cleavage dimensions in terms of spatial ordering. His approach can be extended further to an interval level: coalitions must only be composed of that set of parties *closest* to one another on ideological or cleavage dimensions in terms of spatial distance; I refer to this as minimal winning compact coalitions. In this study neither minimal connectedness or minimal compactness has been investigated because such an approach would introduce a great deal of complexity to the data analysis considering the number of coalitions to be analyzed and the assumption that multiple cleavage dimensions will require connectedness or compactness at a multidimensional level. Second, by focusing on a nominal approach to minimum winning coalitions I am providing a baseline against which to compare future work that employs ordinal or interval approaches to coalitional status. For a fuller discussion of minimal connectedness see Axelrod, *Conflict of Interest*.

The Mediating Influence of State Legislatures on the Linkage Between Interparty Competition and Welfare Policies*

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Introduction

Does interparty competition significantly influence the level of welfare expenditures in the American states? From a theoretical perspective, this would seem to be a plausible contention. Essentially, students of state politics have pointed to two basic arguments as underscoring its theoretical plausibility. First, it is argued that in highly competitive situations political parties compete for support of all groups in the society. They do so by supporting public policies which are beneficial to these groups. Since welfare expenditures are directly relevant to the interests of lower socioeconomic groups, it follows that to gain the support of these groups, political parties should support the expenditure of public monies in this area. This is to be contrasted with noncompetitive situations in which the majority party does not have to be greatly concerned with the interests of lower socioeconomic groups.[1]

The second, complementary argument[2] is based on the following interrelated assumptions: (1) that the degree of interparty competition can be thought of as an index of the degree to which politics is organized in the states; (2) that organized politics "lessens the difficulty of lower status groups in sorting out political actors and issues, thereby enabling them to promote their own interests more effectively;"[3] and (3) that an important component of the interests of lower status groups is welfare expenditures.

The logic of these arguments leads to an important proposition: that interparty competition should be positively and significantly associated with welfare expenditures in the American states. But despite the plausibility of the proposition, much of the recent, empirical research in state politics has not supported it.[4] Rather, these studies have indicated that when socioeconomic factors are controlled, the initial relationship between interparty competition and welfare expenditures is greatly reduced and often disappears. These investigations have supported an alternative proposition: that interparty competition and welfare expenditures are both dependent on the level of the states' socioeconomic development and that any association between interparty competition and welfare expenditures is largely spurious.

But still other researchers, using different types of data and different data manipulation techniques, have concluded that there is a substantial link between interparty competition and welfare expenditures even after controls have been introduced for various socioeconomic factors.[5]

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* This paper grew out of the author's participation in a graduate seminar at the State University of New York at Buffalo conducted by Kenneth N. Vines. In addition to Professor Vines, I would like to thank Roberta S. Sigel of Douglass College, Rutgers University, James A. Stimson of the State University of New York at Buffalo, and Richard A. Zeller of the State University of New York at Buffalo for useful comments on the paper. The paper has also benefited from many stimulating discussions on this general topic with John E. Sinclair of the University of Texas at Austin.

[1] For statements of this argument see, in particular, V. O. Key, Jr., *Southern Politics in State and Nation* (New York: Alfred A. Knopf, 1949), pp. 298–311; Duane Lockard, *New England State Politics* (Princeton: Princeton University Press, 1959), pp. 320–340; and Richard E. Dawson and Virginia Gray, "State Welfare Policies," in *Politics in the American States: A Comparative Analysis*, ed. Herbert Jacob and Kenneth N. Vines (Boston: Little, Brown and Company, 1971), p. 461.

[2] This argument is stated most succinctly in Charles F. Cnudde and Donald J. McCrone, "Party Competition and Welfare Policies in the American States," *American Political Science Review*, 63 (September, 1969), 858.

[3] *Ibid.*

[4] See especially Richard E. Dawson and James A. Robinson, "Inter-Party Competition, Economic Variables and Welfare Policies in the American States," *Journal of Politics*, 25 (May, 1963), 265–289; Thomas R. Dye, *Politics, Economics and the Public: Policy Outcomes in the American States* (Chicago: Rand McNally and Company, 1966), pp. 143, 144; and Richard I. Hofferbert, "The Relationship between Public Policy and Some Structural and Environmental Variables in the American States," *American Political Science Review*, 60 (March, 1966), 73–82. For an analysis which uses a more extended time period and comes to essentially the same conclusion see Richard E. Dawson, "Social Development, Party Competition, and Policy," in *The American Party Systems: Stages of Political Development*, ed. William Nisbet Chambers and Walter Dean Burnham (New York: Oxford University Press, 1967), pp. 203–237.

[5] See, in particular, Ira Sharkansky, *Spending in the American States* (Chicago: Rand McNally, 1968); Ira Sharkansky and Richard I. Hofferbert, "Dimensions of State Politics, Economics, and Public Policy," *American Political Science Review*, 63 (September, 1969), 867–879; Duane Lockard, "State Party Systems and Policy Outputs," in *Political Research and Political Theory*, ed. Oliver Garceau (Cambridge: Harvard University Press, 1968), pp. 190–220. Also relevant to this point is Cnudde and McCrone, pp. 858–866.

The Mediating Influence of the State Legislature

In this paper I do not wish to enter this debate, much less to attempt to resolve it.[6] The purpose of this paper, rather, is to reformulate the theoretical link between interparty competition and welfare policies and to present some evidence which indicates that the reformulation is empirically fruitful.

The major problem with the party competition-welfare expenditures hypothesis is that it does not consider the influence that state political institutions can have on the determination of public policy. In particular, to argue that interparty competition significantly influences the level of welfare expenditures assumes that state legislatures are able to convert party conflict into public policies. But given the weak organizational structure of many state legislatures, this would seem to be a questionable assumption at best. Indeed, since many state legislatures provide only a minimum of services to legislators, meet relatively infrequently, and in general, can be considered organizationally ineffective, it is understandable that even the most salient forms of interparty conflict do not have a firm, consistent impact on state policies. On the other hand, in those states which have organizationally strong, effective legislatures, the political system at least has the potential capacity to translate party conflict into state policies. Understood in this way, the legislature can be seen to function as a mediating influence in the state policy-making process.

The above reasoning leads to the specific proposition which will be tested in this paper: that interparty competition and welfare expenditures should be more strongly associated among those states which have organizationally strong, effective legislatures than among those states which have weak legislative systems. A logical implication of this proposition will also be investigated: that after controlling for various socioeconomic factors the partial correlations between interparty competition and welfare expenditures will be significantly stronger among those states which have strong, as opposed to weak, legislative systems.[7]

Measurement of Variables

Interparty Competition. The measure of interparty competition used in this research was developed by Austin Ranney.[8] The Ranney index covers the 1956–70 time period and applies only to state offices. The measure is based on four components: (1) the average percentage of the popular vote won by the Democratic gubernatorial candidate; (2) the average percentage of the seats in the state senate held by the Democrats; (3) the average percentage of the seats in the state house of representatives held by the Democrats; and (4) the percentage of all terms for governor, senate, and house in which the Democrats had control. For each state Ranney averaged together all four percentages to produce an "index of competitiveness."[9]

Welfare Policies. Since many state policies have a welfare dimension I have defined welfare policies broadly to include: (1) per capita state and local education expenditures (1968–69); (2) per capita state and local welfare expenditures (1969); (3) average monthly payment for old-age assistance (1969); (4) average monthly payment for aid to dependent children (1969); (5) average monthly payment for aid to blind (1969); (6) average monthly payment for aid to disabled (1969); and (7) percentage of public assistance supplied by state and local governments (1968).

Socioeconomic Factors. Of the many socioeconomic characteristics of the states that are relevant to public policy generally and welfare policies in particular, the present analysis considers the following four factors: (1) per capita personal income; (2) per capita retail sales; (3) percentage of population employed in manufacturing; and (4) percentage of population living in urban areas. These measures were selected because they

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[6] For discussions of these conflicting findings and explanations see John H. Fenton and Donald W. Chamberlayne, "The Literature Dealing with the Relationships between Political Processes, Socio-economic Conditions and Public Policies in the American States: A Bibliographical Essay," *Polity*, 1 (Spring, 1969), 388–394; Herbert Jacob and Michael Lipsky, "Outputs, Structure, and Power: An Assessment of Changes in the Study of State and Local Politics," *Journal of Politics*, 30 (May, 1968), 511–519; Brian R. Fry and Richard F. Winters, "The Politics of Redistribution," *American Political Science Review*, 64 (June, 1970), 508–522; and John L. Sullivan, "A Note on Redistributive Politics," *American Political Science Review*, 66 (December, 1972), 1301–1305.

[7] Throughout this paper when correlations and differences between correlations are described as significant, I do not mean to imply that these correlations are statistically significant at a given level. This classical notion of statistical inference from sample to population would be inappropriate in this case, since the data came from the population of states, not a sample thereof. Rather, my use of the term "significant" means simply that the correlation or difference between two correlations is not a trivial one; that it is substantively important. The statistics provided in this paper, therefore, are intended to make inferences from the data to specified theoretical notions, not to estimate population parameters.

[8] Austin Ranney, "Parties in States Politics," in *Politics in the American States*, ed. Jacob and Vines, pp. 82–121. The index appears on page 87.

[9] All index scores above .5000 were subtracted from 1.0000 in order to transform the index from one of degree of Democratic control to degree of party competition, with higher scores representing greater degrees of competition.

Table 1. Zero-Order Correlation Coefficients (Pearson r's) Between Interparty Competition and Welfare Expenditures, by Level of Legislative Professionalism[a]

	Among States High in Legislative Professionalism	Among States Low in Legislative Professionalism	Difference Between Correlations
1. Education Expenditures	.62	.41	.21
2. Total Welfare Expenditures	.30	−.15	.45
3. Old-Age Assistance	.51	.23	.28
4. Aid to Dependent Children	.79	.45	.34
5. Aid to Disabled	.79	.32	.47
6. Aid to Blind	.61	.35	.26
7. Percentage of Public Assistance Supplied by State and Local Governments	.86	.68	.18
Mean Correlation	.64	.33	.32

[a] Alaska and Hawaii not included.

seem to be valid indicators of the social, economic, and ecological development of the states and because they have been used extensively in previous research on state politics.

This study also requires a measure of the organizational effectiveness of state legislatures. I have utilized Grumm's legislative professionalism index as the closest approximation to the desired measure.[10] Grumm's index provides a single, summary score for each state based on a factor analysis of the following variables: the biennial compensation of legislators; expenditures for legislative staff, services, operations, and printing; number of bills introduced in a session; length of sessions in calendar days; and a score measuring legislative services. The time period for most of these components is 1963–64. The higher the score for the state, the more professional the state legislature is thought to be. The scores were dichotomized at the median. Those states with a score above the median were considered to be high in legislative professionalism while those below the median were considered to be low in legislative professionalism.

Analysis

Table 1 presents the correlations between interparty competition and welfare expenditures among states high and low in legislative professionalism. If the theoretical notions outlined above are empirically valid, we should observe a stronger set of correlations among states high in legislative professionalism than among states low in legislative professionalism. Turning first to states that are high in legislative professionalism, one can see that the correlations range from .30 to .86 with the mean correlation being .64. There is, in other words, a strong, positive relationship between party competition and level of welfare expenditures among states with professional legislatures. Among states low in legislative professionalism, the correlations are, with one exception, positive, but in every case, they are lower than the corresponding correlations among states high in legislative professionalism. The magnitude of the differences range from a low of .18 to a high of .47 with the mean difference being .32. Overall, the results presented in Table 1 offer strong and consistent support for the central proposition advanced in this paper: that the linkage between party competition and welfare policies is greater for those states with effective legislatures than for those states with ineffective legislative systems.

Table 2 presents the first-order partial correlations between interparty competition and welfare expenditures after controlling for each of the socioeconomic factors. The results are shown separately for states high and low in legislative professionalism. While the control for per capita income substantially reduces the correlations among states with professional legislatures, controlling for the other socioeconomic factors does not greatly alter the zero-order relationships. Among states low in legislative professionalism, controlling for both per capita retail sales and per capita income significantly reduces the original relationships.

The more important point to note about Table 2, however, is that when each SES factor is controlled for, the set of partial correlations remains

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[10] John G. Grumm, "The Effects of Legislative Structure on Legislative Performance" in *State and Urban Politics: Readings in Comparative Public Policy*, ed. Richard I. Hofferbert and Ira Sharkansky (Boston: Little, Brown, and Company, 1971), pp. 298–322. The index appears on page 317. Evidence relevant to the construct validity of Grumm's index appears in The Citizens Conference on State Legislatures, *State Legislatures: An Evaluation of Their Effectiveness* (New York: Praeger Publishers, 1971), p. 70. Their analysis shows that Grumm's index is significantly related to a measure of legislative capability which they devised for each state.

Table 2. First-Order Partial Correlation Coefficients (Pearson r's) Between Interparty Competition and Welfare Expenditures, by Level of Legislative Professionalism[a]

	Legislative Professionalism							
	High				Low			
	Controlling For							
	SES1	SES2	SES3	SES4	SES1	SES2	SES3	SES4
1. Education Expenditures	.76	.61	.41	.65	.46	.01	.23	.41
2. Total Welfare Expenditures	.20	.26	.28	.22	−.17	−.16	−.08	−.15
3. Old-Age Assistance	.36	.42	.05	.37	.26	−.08	.07	.27
4. Aid to Dependent Children	.68	.75	.51	.69	.45	.05	.27	.52
5. Aid to Disabled	.69	.73	.34	.69	.32	.20	.09	.16
6. Aid to Blind	.53	.51	.22	.46	.37	−.17	.08	.35
7. Percentage of Public Assistance Supplied by State and Local Governments	.78	.82	.60	.81	.70	.30	.24	.59
Mean Correlation	.57	.59	.34	.56	.34	.02	.13	.31

[a] Alaska and Hawaii not included.
SES1—Percentage of population employed in manufacturing.
SES2—Per capita retail sales.
SES3—Per capita personal income.
SES4—Percentage of population living in urban areas.

substantially stronger among states high in legislative professionalism than among states low in legislative professionalism. In fact, of the 56 correlations presented in Table 2, 55 are consistent with this pattern and the remaining partial (old age assistance controlling for per capita personal income; .07 versus .05) is more a tie than a complete reversal. The major conclusion to be drawn from this analysis is that the effectiveness of state legislatures continues to mediate the link between party competition and welfare policies even after controls are introduced for each of the socioeconomic factors.

Table 3 shows the correlations between interparty competition and welfare expenditures after simultaneous controls have been introduced for the four socioeconomic factors. Examining those states which are low in legislative professionalism, one can see that three of the partial correlations are negative and the other correlations though positive are very weak. Conversely, among those states high in legislative professionalism, all seven partials are positive, and three of them (education expenditures, aid to dependent children, and percentage of public assistance supplied by state and local governments)[11] are quite impressive. Indeed, only the partial correlation between party competition and old-age assistance is insubstantial.

A comparison of the two sets of fourth-order partials indicates that for each of the welfare measures the partial is higher (i.e., has a higher positive value) for those states with professional legislatures than for those states with unprofessional legislatures. Similarly, an examination of the mean partials shows that there is a practically negligible correlation (.01) among states low in legislative professionalism, while the average partial among states high in legislative professionalism is substantial (.38). Thus, the link between party competition and welfare expenditures continues to be mediated by the differential effectiveness of state legislatures even after simultaneous controls are introduced for the socioeconomic factors.

Table 4 presents the results of a multiple regression analysis which utilizes the four socioeconomic factors and the interparty competition measure as predictor (independent) variables for each of the seven welfare policies. The analysis is performed separately for states high and low in legis-

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[11] Among states high in legislative professionalism, then, these three welfare expenditures (of the seven studied) are the most significantly affected by interparty competition. In this sense, they may be described as the most politically controversial and perhaps publically salient of the policies in the welfare domain. While it may be obvious that aid to dependent children and percentage of public assistance supplied by state and local governments are central elements in the welfare domain, it seems somewhat more difficult to justify the centrality of educational expenditures in terms of the struggle between the haves and have-nots. A possible reason for its centrality in the present case is that the measure of educational expenditures used in this paper does not include capital expenditures but reflects instead, such controversial items as the salaries of teachers, cost of books, special education costs, etc. See Cnudde and McCrone, pp. 859, 862, and 865 for a more extended discussion and analysis of the relation of educational expenditures to the struggle between the haves and have-nots.

Table 3. Fourth-Order Partial Correlation Coefficients (Pearson r's) Between Interparty Competition and Welfare Expenditures, by Level of Legislative Professionalism[a]

	Legislative Professionalism	
	High	Low
	Controlling For	
	Four SES Variables	Four SES Variables
1. Education Expenditures	.65	.18
2. Total Welfare Expenditures	.27	−.13
3. Old-Age Assistance	.02	−.14
4. Aid to Dependent Children	.54	.10
5. Aid to Disabled	.31	.11
6. Aid to Blind	.24	−.25
7. Percentage of Public Assistance Supplied by State and Local Governments	.63	.18
Mean Correlation	.38	.01

[a] Alaska and Hawaii not included.
SES1—Percentage of population employed in manufacturing.
SES2—Per capita retail sales.
SES3—Per capita personal income.
SES4—Percentage of population living in urban areas.

lative professionalism. The coefficients are standardized partial regression coefficients (beta weights) indicating the direct explanatory contribution of interparty competition for each welfare policy. These beta weights can be interpreted as the average change in the specified welfare expenditure per standardized unit change in interparty competition with the influence of the four SES factors statistically removed.[12] A comparison of these betas across states high and low in legislative professionalism indicates: (1) that for five policies—education expenditures, total welfare expenditures, aid to dependent children, aid to blind, and percentage of public assistance supplied by state and local governments—the betas are significantly higher among states with professional, as opposed to unprofessional, legislative systems; (2) that, on the whole, all the betas are fairly weak among states low in legislative professionalism; and (3) that among states high in legislative professionalism, only expenditures for old-age assistance is not significantly affected by party competition.[13]

Summary and Conclusions

In this brief paper I have argued that the hypothesis linking interparty competition and welfare policies was basically incomplete, for it, in effect, "assumed away" the decisive influence that political institutions can have on policy making. My alternative formulation presumes that the degree of competition exhibited by political parties does influence the nature and scope of public policy in general and welfare policy in particular but that the precise dimensions of its influence are determined by the states' formal decision-making institutions. Put somewhat differently, this formulation asserts that political institutions can affect the intensity of the linkage between party competition and welfare policies.

I argued, specifically, that the relationship between interparty competition and welfare expenditures should be mediated by the differential effectiveness of state legislatures. Because the

Table 4. Standardized Partial Regression Coefficients Regressing Welfare Expenditures on Interparty Competition With SES Controlled, By Level of Legislative Professionalism[a]

	Among States High in Legislative Professionalism	Among States Low in Legislative Professionalism
1. Education Expenditures	.76	.22
2. Total Welfare Expenditures	.45	−.21
3. Old-Age Assistance	.03	−.20
4. Aid to Dependent Children	.59	.13
5. Aid to Disabled	.27	.17
6. Aid to Blind	.32	−.31
7. Percentage of Public Assistance Supplied by State and Local Governments	.53	.18

[a] Alaska and Hawaii not included.
SES1—Percentage of population employed in manufacturing.
SES2—Per capita retail sales.
SES3—Per capita personal income.
SES4—Percentage of population living in urban areas.

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[12] See Karl Schuessler, *Analyzing Social Data: A Statistical Orientation* (Boston: Houghton Mifflin Company, 1971), p. 24.

[13] The comparison of beta-coefficients across different samples or different subsets within a sample can be distorted by unequal variances. However, an examination of the unstandardized partial regression coefficients, which are not affected by unequal variances, confirms the interpretation offered here in all essential respects. For a discussion of the use of standardized versus unstandardized regression coefficients see H. M. Blalock, Jr., "Causal Inferences, Closed Populations, and Measures of Association," in *Causal Models in the Social Sciences*, ed. H. M. Blalock, Jr. (New York: Aldine Atherton, 1971), pp. 139–151.

state legislature is a principal political institution for converting party conflict into public policy, and because the conversion process should be effective to the degree that the legislature is organizationally capable, it seemed likely that the linkage between party competition and welfare expenditures would be more pronounced among states with strong, effective legislative systems than among those with weak legislative systems.

On the whole, the empirical results of the analysis were highly congruent with these theoretical expectations. When legislative effectiveness was defined in terms of professionalism and welfare effort was assessed by seven specific measures, it was found that the link between party competition and welfare expenditures was indeed stronger among states with effective, as opposed to ineffective, legislative systems. The differences, moreover, were not marginal and did not lack consistency; on the contrary, the correlations were substantially stronger for states with professional legislatures with respect to all seven welfare policies[14] (i.e., the mean difference in correlations was .32).

This differential pattern of correlations persisted when controls were introduced for four socioeconomic factors. That is, after controlling for socioeconomic development, the partial correlations between interparty competition and welfare expenditures were significantly stronger among those states high in legislative professionalism than among those low in legislative professionalism. Among those states low in legislative professionalism, three of the partials were negative and the other four were exceedingly weak. This indicates that the link between party competition and welfare policies is not only tenuous but may, in fact, be spurious for some welfare expenditures (i.e., total welfare expenditures, old-age assistance, and aid to blind). Figure 1 indicates the overall pattern of intercorrelations found among socioeconomic development, party competition, and welfare policy among states low in legislative professionalism. The broken arrow connecting party competition and welfare policies indicates that there is, at best, only a weak linkage between them after controls are introduced for the SES factors.

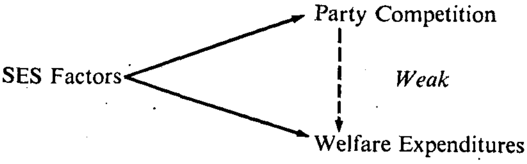

Figure 1. Pattern of Intercorrelations Among SES Factors, Party Competition, and Welfare Expenditures Among States Low in Legislative Professionalism.

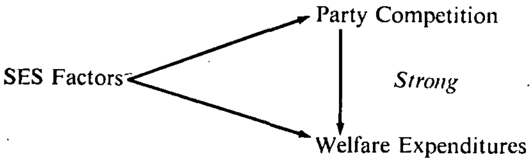

Figure 2. Pattern of Intercorrelations Among SES Factors, Party Competition, and Welfare Expenditures Among States High in Legislative Professionalism.

The partials between interparty competition and welfare expenditures among states high in legislative professionalism present an entirely different situation. When the SES factors were controlled for, only the correlation between party competition and expenditures for old age assistance proved insubstantial; three of the other partials were moderately high and three of them were quite high. In other words, the introduction of controls for the SES factors did not produce insignificant correlations for six of the seven welfare measures. The unbroken arrow connecting party competition and welfare policies in Figure 2 schematically depicts the strong link between these variables for states high in legislative professionalism.

This paper, in sum, has attempted to do more than simply describe the relationship between party competition and welfare policies in the American states. By demonstrating that the state legislature can mediate this linkage, it has also sought to reaffirm the importance of political institutions in any explanatory model of the policymaking process.

(*Appendix follows on next page*)

[14] The states were also trichotomized according to their scores on the legislative professionalism index and relevant correlations were computed between interparty competition and welfare expenditures within each set of states. As would be expected on the basis of findings reported in this paper, the correlations became stronger as the set of states became more professionalized.

APPENDIX: SOURCE OF VARIABLES

1. Per capita state and local education expenditures (excludes capital expenditures)	Council of State Governments, *The Book of the States, 1970–71* (Lexington, Kentucky: Council of State Governments, 1970), p. 306.
2. Per capita state and local welfare expenditures	U.S. Bureau of the Census, *Governmental Finances in 1968–69* Washington, D.C., 1969
3. Average monthly payment for old-age assistance	U.S. Bureau of the Census, *Statistical Abstract of the United States: 1970* (91st ed.) Washington, D.C., 1970, Table 455, p. 301.
4. Average monthly payment for aid to dependent children	U.S. Bureau of the Census, *Statistical Abstract of the United States: 1970* (91st ed.) Washington, D.C., 1970, Table 455, p. 301.
5. Average monthly payment for aid to disabled	U.S. Bureau of the Census, *Statistical Abstract of the United States: 1970* (91st ed.) Washington, D.C., 1970, Table 455, p. 301.
6. Average monthly payment for aid to blind	U.S. Bureau of the Census, *Statistical Abstract of the United States: 1970* (91st ed.) Washington, D.C., 1970, Table 455, p. 301.
7. Percentage of public assistance supplied by state and local governments	Council of State Governments, *The Book of the States, 1970–71* (Lexington, Kentucky: Council of State Governments, 1970), p. 375.
8. Percentage of population employed in manufacturing	U.S. Bureau of the Census, *Statistical Abstract of the United States: 1965* (86th ed.) Washington, D.C., 1965, Table 308, p. 225.
9. Per capita personal income	U.S. Bureau of the Census, *Statistical Abstract of the United States: 1965* (86th ed.) Washington, D.C., 1965, Table 458, p. 334.
10. Per capita retail sales	U.S. Bureau of the Census, *Statistical Abstract of the United States: 1965* (86th ed.) Washington, D.C., 1965, Table 1195, p. 824.
11. Percentage of population living in urban areas	U.S. Bureau of the Census, *Statistical Abstract of the United States: 1965* (86th ed.) Washington, D.C., 1965, Table 11, p. 16.

Urbanization and Political Demand Making: Political Participation Among the Migrant Poor in Latin American Cities*

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The delegation of petitioners from Community "X" has been waiting patiently for several hours: seven entire families including restless children and family heads who have taken time off from their jobs, led by officers of the community's improvement association. The anteroom leading to the office of a government bureaucrat is crowded with dozens of similar delegations from other neighborhoods of the city. The petitioners from Community "X" are finally granted admittance. They are urban squatters, and present a laboriously typed petition signed by virtually all residents of their community, requesting government recognition of their land tenure rights. Acting as spokesman for the group, one of the petitioners argues that land titles are essential to assuring a modest inheritance for their children. Moreover, he continues, security of tenure stimulates investment in permanent house construction and commercial enterprises within the community. The spokesman also observes pointedly that with property titles, community residents may begin contributing to government tax revenues. To strengthen the image of their community as one deserving of government aid, other members of the delegation cite examples of the hardships they have endured in settling the land and the self-help efforts they have undertaken to improve the community. The petitioners stress their belief in the government's commitment to humanitarian concerns, and the desirability of granting their request in the interests of social justice and social tranquility. The official promises that the community's situation will be thoroughly investigated. The session is concluded amid fervent expressions of the petitioners' support for the incumbent regime.

This scene is repeated many times daily in government offices in many Third World cities. It represents the first step in a long, complex, and often frustrating effort to manipulate the political system to satisfy a deeply felt need. The petitioners from low-income urban settlement zones such as Community "X" are typically of rural origin. They have migrated to the city in search of economic opportunity, and find that rent-free housing provided by residence in squatter settlements is a key element in fulfilling their material aspirations for themselves and their children.

Petitioning of government officials for such benefits as land titles and basic urban services also represents a form of political participation which is all too often overlooked in research and writing on the political behavior of cityward migrants or the urban poor in general. In fact, most researchers who have contributed to this literature have been concerned primarily with the potential for political radicalism or violence among such populations. The present study is an attempt to fill in some of the numerous gaps in our understanding of how migrants to the city become involved in the political process in nonviolent, nondisruptive ways.

The particular mode of political participation upon which this paper focusses is political demand making, defined as individual or collective activities aimed at extracting certain types of benefits from the political system by influencing the decisions of incumbent government officials. Demand making is thus differentiated from political participation aimed at influencing government resource allocation by replacing or retaining the incumbent authorities (i.e., electoral participation) or by overthrowing or restructuring the political system itself (e.g., through revolutionary violence). Political demands are defined as needs whose satisfaction is felt to depend upon governmental action, and which are asserted by individuals or groups as specific claims upon the government. The term "demand" implies nothing about the way in which such claims are asserted. Particularly among low-income city dwellers in Latin America, demand articulation need not, and usually does not, involve table pounding, protest demonstrations, or other aggressive forms of behavior.

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* This paper represents a report of findings from the first stage of a cross-national study of politicization and demand-making behavior among low-income migrants to large cities, being conducted in collaboration with Henry A. Dietz (University of Texas, Austin) and James W. White (University of North Carolina, Chapel Hill). The study was supported from 1972–1974 by a grant from the Ford and Rockefeller Foundations' Program of Social Science and Legal Research on Population Policy. The author is also indebted to the Danforth Foundation, the National Science Foundation (Grant GS-2738), the Foreign Area Fellowship Program, and the Center for International Affairs at Harvard University for financial support during the periods of data collection in Mexico City and data processing at Stanford University and Harvard University. The assistance of Harley Browning, Alex Inkeles, and Sidney Verba in construction of the basic survey instrument used in this study is gratefully acknowledged. Helpful comments on a preliminary version of this paper were provided by Ann L. Craig, Henry A. Dietz, Richard R. Fagen, John O. Field, Frederick W. Frey, and Merilee Grindle. Portions of this paper were presented at the Annual Meeting of the American Political Science Association, New Orleans, September 1973.

Demand making as a mode of political participation has several distinctive characteristics. Because the individual participant—or the leaders of the demand-making group of which he may be a member—determines the subject matter of contacts with public officials, the outcome of the participatory act is certain to be highly salient and important to the individual.[1] The linkage between the participatory act and its results is more direct and visible in this mode of political activity than in any other. Moreover, the motivations for participation in the demand-making mode are almost entirely instrumental rather than expressive or regime-supportive.[2] Although demand-making activity may sometimes have ritualistic, system-supportive overtones, it is nevertheless a form of behavior aimed primarily at manipulating the political system and influencing value allocations. Demand makers are persons who are "actively seeking explicit responses from the political system" and perceive their behavior as "a tactic to enhance their chances of eliciting those responses."[3] If political participation is conceived generally as citizen activity aimed at influencing governmental decision making, participation within the demand-making mode is the most direct and often the most effective strategy of goal attainment among the types of urban dwellers who are the focus of this study.

Urbanization and Political Demand Making

Theorists of political and social change, as well as government policy makers, have often posited a close relationship between increased rural-to-urban migration and changes in the types and frequency of demands made upon governments.[4] It has been argued that migration to the city tends to expand the range of human needs which are susceptible to satisfaction through governmental action. At the same time, exposure to the urban environment is thought to increase levels of political awareness among the migrant population. Finally, cityward migration places a larger proportion of the population in close proximity to government officials, facilitating citizen access to such officials and reducing the costs of articulating needs in terms of time, effort, and money. The result is assumed to be accelerated demand making; and it is further assumed that demands for public services and other governmental benefits will increase much faster than does the capability of the political system to respond to them. Applying this line of argument specifically to Latin America, Robert Scott has written that

> as the character of the general population alters from predominantly rural and isolated to partly integrated and urban, . . . shifts in the kinds, amounts, and intensity of demands on government occur. Both totally and proportionately more people leave subsistence agriculture and sharecropping to become part of the money economy in the city, or outside it as members of the rural proletariat and participants in commercial or market agriculture operations. To such persons, what happens in national politics is important, and the kinds of government programs they will condone change drastically. In the past . . . much of what government provided either was symbolic—a land-reform law that was not implemented, for instance——or not so much what the masses wanted as what government leaders felt they should have. Now, as the general populace becomes more knowledgeable, this sort of government activity does not satisfy them.[5]

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[1] See the discussion of contacting of officials as a mode of political participation in Sidney Verba and Norman H. Nie, *Participation in America* (New York: Harper and Row, 1972), p. 52.

[2] On the distinction between "instrumental" and "expressive" political participation, see Lester Milbrath, *Political Participation* (Chicago: Rand McNally, 1965), p. 12. Verba and Nie (*Participation in America*, p. 2) also differentiate between instrumental participation and "ceremonial" or "support" participation, in which "citizens 'take part' by expressing support for the government, by marching in parades, by working hard in developmental projects, by participating in youth groups organized by the government, or by voting in ceremonial elections." See also the discussion of ritualistic and system-supportive participation in the Mexican political system, in Richard R. Fagen and William S. Tuohy, *Politics and Privilege in a Mexican City* (Stanford, Calif.: Stanford University Press, 1972), pp. 38–39.

[3] Peter K. Eisinger, "Protest Behavior and the Integration of Urban Political Systems," *Journal of Politics*, 33 (November, 1971), 984.

[4] See especially Karl W. Deutsch, "Social Mobilization and Political Development," *American Political Science Review*, 55 (September, 1961), 498–499; Samuel P. Huntington, *Political Order in Changing Societies* (New Haven: Yale University Press, 1968), p. 5; and W. Howard Wriggins and James F. Guyot, "Demographic Change and Politics," in *Population, Politics, and the Future of Southern Asia*, ed. W. H. Wriggins and J. F. Guyot (New York: Columbia University Press, 1973), pp. 5–16. See also, with specific reference to Latin America, Ernest A. Duff and John F. McCamant, "Measuring Social and Political Requirements for System Stability in Latin America," *American Political Science Review*, 62 (December, 1968), 1125–1129; and the studies cited in Wayne A. Cornelius, "The Political Sociology of Cityward Migration in Latin America: Toward Empirical Theory," in *Latin American Urban Research, Volume I*, ed. Francine F. Rabinovitz and F. M. Trueblood (Beverly Hills, Calif.: Sage Publications, 1971), pp. 100–101.

[5] Robert E. Scott, "National Integration Problems and Military Regimes in Latin America," in *Latin American Modernization Problems*, ed. R. E. Scott (Urbana, Ill.: University of Illinois Press, 1973), pp. 301–302.

Such arguments appear to be based upon rather mechanistic assumptions about the conversion of objectively defined needs into effective demands upon government. The demand for public services is viewed largely as a function of the number of potential clients, whose ranks are swelled not only by migration from the countryside but also by persistently high fertility rates among native-born residents of Third World cities. The evidence presented in this paper suggests that this kind of "one man, one demand" assumption may prove highly misleading in any attempt to estimate the potential impact of rapid population growth or redistribution upon the functioning of political systems. Particularly with regard to migrants from the countryside, the extent to which increased political awareness and objectively defined needs are actually translated into increased pressures for extension of public services and other types of benefits is an empirical question, and one which has rarely been addressed in previous research.

Research bearing on this question must go beyond measurement of the absolute level of frequency of political demand making among newly urbanized sectors of the population. It must seek answers to a number of questions relating to the *process* by which objective needs are converted into political demands. How do migrants to the city perceive or define their most acute needs and problems? To what extent is the government perceived by such people as the relevant problem-solving agency? What is the substantive nature of the demands presented? Are they concerned with individual needs, or collective needs of urban neighborhoods or other types of social units? Toward what kinds of officials or agencies are the demands directed? What strategies are used in demand-making attempts, and what factors determine the outcomes of such attempts? In sum, how do those seeking to influence the distribution of government benefits accomplish their goals?

This paper will present some research findings bearing on these questions, drawn from a larger comparative study of political attitudes and behavior among low-income migrants to the capital cities of Mexico, Peru, and Japan. In Mexico City, male heads of family residing in six predominantly low-income settlement zones or *colonias proletarias* comprise the universe of the study. These communities, all located on the periphery of the metropolitan area, were selected purposively so as to maximize the range of variation among them on three dimensions of particular revelance to political demand making: (1) the age of the community; (2) its type of origin (i.e., how the community was originally occupied or created); and (3) the overall developmental level of the community, defined in terms of access to basic urban services and improvements. Thus the research sites in Mexico City include three squatter settlements at different stages of evolution (designated here as Colonia Nueva, Colonia Periférico, and Colonia Militar), two different types of government-initiated settlements (Colonia Esfuerzo Propio and Unidad Popular), and a commercially developed subdivision (Colonia Texcoco).[6]

In each community a stratified probability sample of male heads of family was selected for structured, personal interviews. Approximately one third of the total sample consists of non-migrants, defined as individuals born or raised in the Mexico City metropolitan area. The remainder are migrants, defined as people born outside of Mexico City who spent most of their lives between ages 5 and 15 outside of the city. The data gathered through sample survey interviews were supplemented with ethnographic data gathered through several months of participant observation in each of the research communities. Unstructured interviews were also conducted with the leaders of each community, as well as with government officials who have had extended contact with the community.[7] Large portions of the basic survey instrument were also administered in an independent study conducted in four rural communities which have served as points of origin for migrants to Mexico City.[8] In the analysis below, some comparisons are made between migrants to Mexico City and residents of these rural communities. Contrasting findings from a parallel study of migrants to Lima, Peru, will also be reported.[9]

The Conversion of Needs into Political Demands

It is possible to identify several distinct stages in the conversion of objectively defined needs into

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[6] Pseudonyms have been used to identify all of the research communities. The formation and subsequent development of each community are described in Wayne A. Cornelius, *Political Learning Among the Migrant Poor: The Impact of Residential Context* (Beverly Hills, Calif.: Sage Publications, 1973 [Sage Professional Papers in Comparative Politics, Vol. 4, No. 01-037]), pp. 10–15.

[7] Field work was conducted in Mexico City from January to December, 1970, with supplemental data gathering in 1971 and 1972. For a more detailed description of the design and methodology of the study, see Chapter 1 and Appendix "D" in Wayne A. Cornelius, "Poverty and Politics in Urban Mexico: Political Learning Among the Migrant Poor" (Ph.D. dissertation, Stanford University, 1973).

[8] F. Christopher Arterton, "Patterns of Political Participation in Four Rural Mexican Villages" (Ph.D. dissertation, Massachusetts Institute of Technology, 1973).

[9] See Henry A. Dietz, "Becoming a Poblador: Political Adjustment to the Lima Urban Environment" (Ph.D. dissertation, Stanford University, 1973). Additional data from the Lima survey are presented in Wayne A. Cornelius and Henry A. Dietz, "Urbanization, Demand-Making, and Political System 'Overload': Political Participation Among the Migrant Poor in Latin American Cities," paper presented at the Annual Meeting of the American Political Science Association, New Orleans, Louisiana, September, 1973.

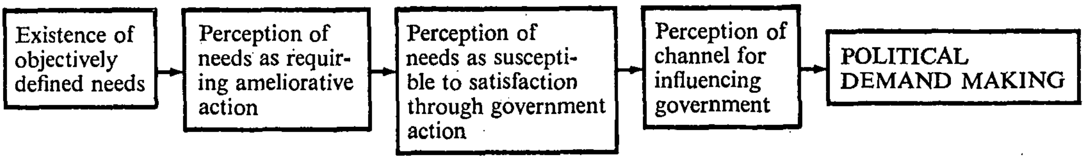
Existence of objectively defined needs
Perception of needs as requiring ameliorative action
Perception of needs as susceptible to satisfaction through government action
Perception of channel for influencing government
POLITICAL DEMAND MAKING

Figure 1. Basic Stages in the Process of Demand Creation

actual demands upon government. Objective deprivations or problems must first be perceived by the individual as requiring some kind of solution or ameliorative action. Secondly, these felt needs must be viewed as needs particularly susceptible to satisfaction through governmental action. Finally, the potential demand maker must be able to perceive a strategy or channel through which to articulate these "politicized" needs. The basic stages in this process of demand creation are summarized graphically in Figure 1.[10]

The assumption implicit in most discussions of the relationship of urbanization to demands on the political system is that the conversion of objective needs into political demands occurs almost automatically. It remains, however, a largely unproved assumption. In the analysis which follows I will attempt to demonstrate that there are often major lags in the process of demand creation among migrants to the city, and that, in fact, many kinds of felt needs are viewed by migrants as needs to be satisfied primarily through individual action. Such needs rarely become the objects of political demand-making attempts.

Table 1. Measures of Economic and Service Needs Among Migrants to Mexico City[a]

Have water piped into house	29.7%
Have connection with public sewerage system	30.0
Have paved street adjacent to house	24.8
Lack electricity for house	4.4
Obtain electricity illegally	48.3
Have regular electricity supply	47.4
Have one-room house	31.9
Have house with walls of non-permanent materials	23.3
Have formal, definitive title to land	4.0
Unemployed and seeking work	4.7
Have family income of minimum wage or less[b]	53.7
Have six or more dependents living at home	60.8
Have school-age children	90.0
Had children who died before age 6, while living in city	50.6

[a] Percentages are based on an average N of 618.

[b] At the time of the study, a minimum-wage income in Mexico City was equivalent to about $60.00 (U.S.) per month.

Objective and Perceived Needs. The objective needs of migrants residing in the six research communities are massively evident. As reported in Table 1, less than a third had piped water or connections with public sewerage systems for their houses at the time of the study. Less than a third of the migrants had access to paved streets or permanent sidewalks near their homes. Nearly half obtained their electricity for home use through illegal connections with public transmission lines in nearby urbanized areas. More than one in four of the migrant families still resided in one-room shacks made predominantly of laminated cardboard or other nonpermanent materials. Only 4 per cent had received formal title to the land which they occupied.[11] Less than 5 per cent of the migrant heads of family were unemployed and seeking work at the time of the study, but most held jobs which were poorly paid. Over half had family incomes of less than (U.S.) $60 per month. Migrant families were large (more than 60 per cent of the family heads had six or more dependents living at home) and 90 per cent had school-age children. Child mortality was high; more than half of the families reported having had children who died before reaching the age of six while residing in Mexico City.

What kinds of needs or problems are *perceived* as most important among this sector of the urban population? In the survey, respondents were asked to specify what they felt to be their most

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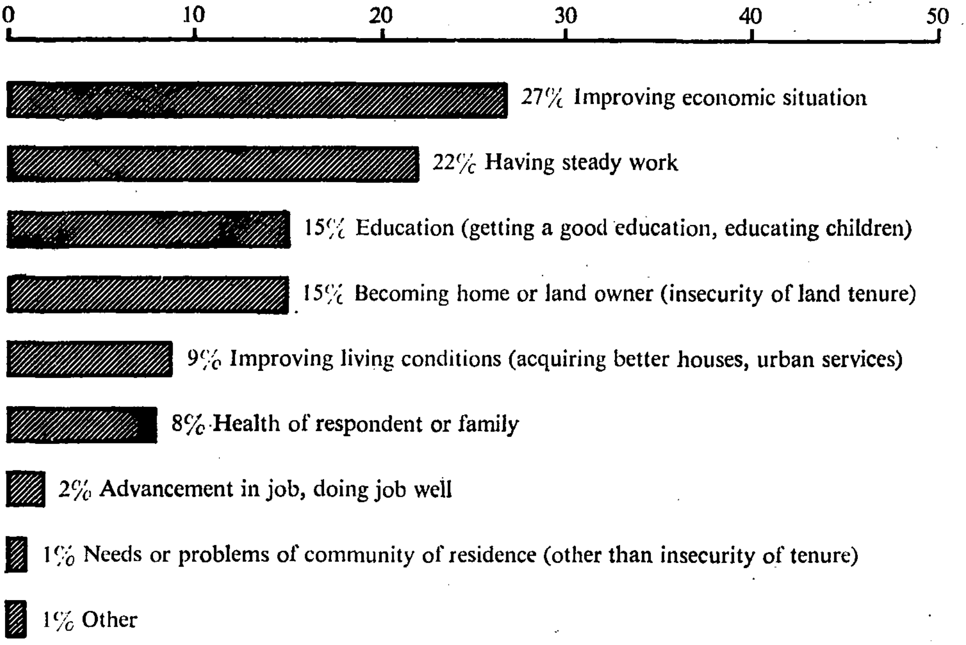
[10] For an alternative conceptualization of the process of demand creation at the level of the individual, see Jerry F. Medler, "Negative Sanctions: Their Perception and Effect in the Political System" (Ph.D. dissertation, University of Oregon, 1966), chapter 5. My model of the process differs from that of Medler primarily in the lesser importance which I attribute to cognitive involvement in politics and a sense of individual political efficacy as preconditions of the act of demand makng. See also George J. Graham, Jr. and Richard A. Pride, "Styles of Political Participation, Want Conversion, and Political Support among Adults and Students in a Metropolitan Community," paper presented at the Annual Meeting of the American Political Science Association, Chicago, September, 1971.

[11] The extremely low proportion of migrants having formal, "definitive" title to their land reflects not only the illegal origins of three of the communities but a lack of money to pay the fees involved in obtaining an officially recognized property title, even after all necessary payments for the cost of the land have been made.



[a] Question: "What about your worries . . . what kinds of things do you worry about most?" $N = 629$. "Don't know" and "Have no worries" responses (representing 7.3% of the sample) are excluded.

Figure 2. Greatest Personal Concern Among Migrants to Mexico City.[a]

pressing personal concern. The responses, presented in Figure 2, indicate that problems relating to socioeconomic mobility are uppermost in the concerns of the respondents. Nearly half of the sample report their greatest personal concern to be either improving their economic situation or having steady employment. Education—either for oneself or one's children—is another key area of concern, followed closely by the need to become a land or home owner. Fewer than one in ten migrants regard improvement of living conditions (through acquiring better housing or access to basic urban services) as their most important concern.

The migrants were also asked to identify the most serious problem or need relating to their community of residence. Their responses, classified by community, are reported in Table 2. Security of tenure is the overwhelmingly salient need in the two squatter settlements, Colonia Nueva and Colonia Periférico, where official recognition of individual occupancy rights had not yet been secured at the time of the survey. In the four officially recognized communities (Colonia Texcoco, Colonia Esfuerzo Propio, Unidad Popular, and Colonia Militar), virtually no one perceived security of tenure as a key need. In the latter communities, basic urban services and improvements—particularly piped water, sewerage systems, and paved streets—are regarded as the most serious deficiencies, followed by community facilities such as schools, public markets, and health care centers. Such problems as environmental pollution, lack of parks and other recreational facilities, and inadequate public transportation are salient only in the two most highly developed communities (Unidad Popular and Colonia Militar); and even in these communities, deficiencies in water supply and in community facilities such as schools and public markets are regarded as more serious. The overall frequency (across communities) with which needs for water and sewerage systems, paved streets, and security of tenure are mentioned as the most important community-related deficiencies corresponds closely with the findings of other studies of perceived needs among residents of low-income communities in Latin American cities.[12]

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[12] Douglas Butterworth, "Squatters or Suburbanites? The Growth of Shantytowns in Oaxaca, Mexico," in Scott, *Latin American Modernization Problems*, p. 231; Frank M. Andrews and George W. Phillips, "The Squatters of Lima: Who They Are and What They Want," *Journal of Developing Areas*, 4 (Janu-

Table 2. Most Important Community Problem Perceived by Migrants to Mexico City, by Community[a]

Problem	Colonia Nueva	Colonia Periférico	Colonia Texcoco	Colonia E. Propio	Unidad Popular	Colonia Militar	ALL
Water supply	23.4%	12.6%	32.5%	12.0%	*35.6%*	7.9%	20.1%
Sewerage	11.0	28.7	*41.7*	14.5	0.0	0.9	16.9
Security of tenure	*46.7*	*45.8*	0.8	0.0	0.0	2.6	16.5
Medical care facilities	2.0	0.0	10.4	*20.5*	13.6	13.2	10.0
Street pavement	2.3	8.9	7.5	19.6	3.4	2.6	7.7
Public markets	0.0	0.0	0.0	12.9	0.0	*28.1*	7.2
Schools	9.1	1.1	0.0	0.0	1.7	25.3	6.4
Environmental[b]	0.0	0.0	0.0	4.3	23.7	0.0	3.6
Electricity	1.0	0.0	5.0	3.4	0.0	0.0	1.8
Police protection	0.0	1.1	2.1	0.8	3.4	0.9	1.2
Garbage collection	0.0	1.9	0.0	0.0	0.0	3.5	0.9
Other	4.4	0.0	0.0	11.9	13.6	7.2	6.8
None	0.0	0.0	0.0	0.0	5.1	7.9	1.9

[a] $N=663$. Inter-community differences are statistically significant, by Chi-square test, at the .001 level. Column percentages may not sum to 100.0 due to rounding. Don't know/no response cases excluded. Question: "Think now about the most important needs and problems of [this community]. In your opinion, what are the most urgent or serious problems and needs of this community? [Probe:] Of these problems or needs which you have mentioned, which do you think is the most important?"

[b] Includes air pollution, insects, unstable subsoil conditions, and overcrowding.

The degree of importance attached by residents of the research communities to three particular needs—security of tenure, schools, and housing—merits further discussion. The intensity with which migrants lacking legal title to their land feel a need to secure such titles is illustrated by the responses to three additional questions asked in the Mexico City study. When asked whether they had been personally concerned or worried about any particular community-related problem during the last year or so, a larger proportion of the total sample mentioned security of land tenure than any other single problem. In response to another question, 30 per cent of the nonrenting migrants expressed the belief that their claim to their house and land was not yet secure, and the proportions of respondents holding this belief were much higher in Colonia Nueva and Colonia Periférico, the two communities whose legalization by the government was not yet complete at the time of the survey. Migrants residing in the research communities were also asked whether they had ever worked or cooperated with other residents of their community to try to solve some community problem. A plurality (28 per cent) of those who had done so reported that they had been concerned with the security of tenure problem.

The overriding importance attached to this problem by so many migrants residing in illegally formed communities is not surprising, since the opportunity afforded by such communities to become owner of a homesite constitutes one of the primary motivations for moving there in the first place. Apart from the important psychological satisfactions deriving from land ownership, it assures at least a modest property inheritance (a "*patrimonio familiar*") for one's children. But short-term economic advantages for the migrant and his family are equally important in explaining the high salience of the security of tenure problem. Most migrants residing in the communities included in this study build their own homes, an activity which represents their principal form of capital investment. The rent-free dwelling provides a source of economic security during periods of unemployment or rampant inflation.[13] Protection of this source of security requires official recognition of land tenure rights. Moreover, land titles are often essential for obtaining loans for home improvements from banks and other private lending institutions.

With regard to the need for more—or better—schools, the reader may find it surprising that individuals who perceive education as a key personal concern (see Figure 2) generally do not place schools at the top of the improvement agenda for their community. The simplest and most adequate explanation for this finding lies in the fact that even in low-income communities suf-

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ary, 1970), pp. 217–218; Cornelius and Dietz, "Urbanization, Demand-Making, and Political System Overload," p. 8, Table 2; John F. C. Turner, "Barriers and Channels for Housing Development in Modernizing Countries," in *Peasants in Cities*, ed. William Mangin (Boston: Houghton Mifflin, 1970), pp. 8–9.

[13] See Andrews and Phillips, "The Squatters of Lima," p. 216.

fering from extreme service deprivation and insecurity of tenure, residents are usually able to construct their own make-shift school buildings, often using the same temporary materials as in their individual dwellings. Teachers may be obtained by petitioning the Ministry of Education or the Popular Sector of the official party. The federal government's program of school construction is also well developed in the Mexico City metropolitan area. Despite extreme overcrowding in many primary and secondary schools, the regime's performance in providing educational opportunities is more impressive than in any other problem area affecting the city's low-income neighborhoods.

One of the most striking findings of this and other studies of perceived needs among the urban poor in Third World cities relates to the very low priority attached by such people to housing needs. Only 3 out of 678 migrants interviewed in Mexico City specified housing as the most important need affecting people in their community of residence. It is clear that most of these individuals regard basic services and improvements such as piped water, sewerage systems, and paved streets as of more immediate importance in the creation of an adequate dwelling environment than a house built of permanent materials. Permanent construction of the house proceeds, but only at a pace commensurate with a family's financial resources. Most families are extremely reluctant to assume the continuing financial burden that occupancy of government-provided housing would impose upon them. Instead they prefer to build and improve their own houses as time and money become available, even though the basic construction period alone may last for 10 to 15 years.[14]

Politicized Needs. What, then, is the "citizen agenda" for governmental action among the migrant population? What kinds of needs do they perceive as susceptible to satisfaction through government resources? The migrants interviewed in Mexico City were asked to give their opinion of how well the government was doing in a wide variety of specific problem areas. They were also asked to specify those kinds of problems or needs for which they thought the government had no responsibility. The responses showed that while the migrants were somewhat hesitant to attribute responsibility to the government for providing economic help to those in need, providing employment, and redistributing wealth ("trying to even out the differences between the rich and poor classes in Mexico"), they overwhelmingly perceived the government as responsible for providing most kinds of community-related services and improvements.

The degree to which community-related problems and needs have been politicized among migrants to Mexico City is further illustrated by their responses to several questions about the role of government in community development. In answer to the question, "Who or what is to blame for most of the problems" of their community, nearly two thirds of the respondents mentioned "the government" in general, or some specific government agency or official. Responding to another question, only 14 per cent of the sample felt that the most important problems of their community could be solved by the residents themselves, without external assistance. When asked, "Who is it from outside of this community who may be of help in solving the kinds of problems you have mentioned," more than 97 per cent of the respondents mentioned some government agency or official. Only 1 per cent mentioned private sources of aid (business firms, private land developers, the Church, etc.), and less than 1 per cent viewed the official party (P.R.I.) as a potential source of aid.

The perceived importance of government assistance versus self-help in community development is also revealed by the migrants' responses to a fixed alternative question which required them to identify the single most important factor for improving living conditions in their community. As shown in Table 3, most migrants to Mexico City viewed government help as most important, followed by self-help efforts by the residents. Analysis by community of residence shows that even among Mexican migrants residing in squatter settlements, where the ethic of self-help is more deeply ingrained than in other types of low-income communities, government assistance is still regarded as paramount in the community development process.[15]

Very significant differences emerge on the issue of self-help versus government assistance in community problem-solving when the responses of migrants residing in the Mexican research com-

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[14] *Ibid.*, p. 220; and Turner "Barriers and Channels in Housing Development," pp. 10–15.

[15] Squatter settlement residents in Ecuador, Chile, and Venezuela have also tended to place a relatively low valuation on self-help efforts. See Thomas M. Lutz, "Self-Help Neighborhood Organizations, Political Socialization, and the Developing Political Orientations of Urban Squatters in Latin America: Contrasting Patterns from Case Studies in Panama City, Guayaquil, and Lima" (Ph.D. dissertation, Georgetown University, 1970), pp. 156–158; and Talton F. Ray, *The Politics of the Barrios of Venezuela* (Berkeley: University of California Press, 1969), p. 85. (But compare, with regard to Venezuela, Kenneth L. Karst, Murray L. Schwartz, and Audrey J. Schwartz, *The Evolution of Law in the Barrios of Caracas* [Los Angeles: Latin American Center, University of California, 1973], pp. 50–51, 76–77.)

Table 3. Perceived Importance of Alternative Sources of Help in Community Development, Among Migrants to Mexico City, Rural Community Residents in Mexico, and Migrants to Lima, Peru[a]

Source of Help	Mexico City ($N=676$)	Rural Mexico ($N=474$)	Lima, Peru ($N=328$)
Government	40.5%	57.6%	37.8%
Community residents	28.4	29.3	57.9
God	17.5	10.1	2.4
Good luck	13.5	2.1	1.8

[a] Differences between migrants to Mexico City and Lima are statistically significant, by Chi-square test, at the .001 level.

Question: "Generally speaking, which is the most important for improving the conditions of life in this community: The hard work of the residents, God's help, the government's help, or good luck?"

munities are compared with those of low-income settlement residents in Lima, Peru. More than 30 per cent of the migrants to Lima believed that the most important problems of their community could be solved by the residents alone, as compared with only 14 per cent of the Mexican migrants. As shown in Table 3, the Peruvian respondents regard self-help as considerably more important to community development than government aid. Although differences in the distribution of responses within other categories ("God's help," "good luck") are also substantial, the even larger cross-national difference in the *relative* importance attached to self-help and government assistance is particularly relevant to our concern with the politicization of needs among migrants.

The most adequate explanation for this difference may lie in variations in the content of directed political socialization from one country to another. In both Mexico and Peru, the government has strongly influenced mass perceptions of the appropriate scope of its activities, although in very different ways. In Peru, most administrations since the late 1950s have sought to stimulate and capitalize upon the self-help efforts of low-income city dwellers. The self-help orientation was also nurtured by U.S. Peace Corps volunteers in low-income urban communities. Moreover, until 1968, when a leftist military regime came to power, the strong governmental emphasis on the need for self-help among residents of such communities was accompanied by a relatively low level of official responsiveness to their needs. Partly in response to these conditions, large and effective self-help projects were launched in many of the squatter settlements ringing large cities.[16]

In Mexico, the low propensity of the poor for self-help in both urban and rural areas (see middle column, Table 3) reflects the efforts of successive governments since 1940 to encourage a sense of mass dependence upon the regime for community improvements and other types of social welfare benefits. In fact, the response of Mexican government officials to community development projects initiated without their knowledge or involvement has often been one of anger or embarrassment.[17] The leaders of low-income urban communities, many of whom are tied to government bureaucrats and PRI functionaries through patron-client relationships, have also encouraged a sense of dependence upon government assistance among their followers.[18]

The cross-national evidence thus suggests that the range of needs perceived by cityward migrants as requiring governmental action is partly a function of the presence or absence of overt governmental attempts to create feelings of dependence among the lower classes. Where the official incentives to self-help have been weak or lacking altogether, as in Mexico and Venezuela, the range of politicized needs will be broader. However, the conversion of diffuse claims upon the government into specific demands communicated to public officials will be retarded by an emphasis on paternalism and dependence in the government's relationship with the poor.

The responsiveness of the political system to petitioning by low-income migrants may also condition their attitudes toward reliance on government aid. In countries where competitive party systems have induced incumbent governments to be more responsive to petitions from low-income urban communities, as in Chile since the early 1960s and Venezuela since 1958, residents of such communities have devoted most of their energies to petitioning rather than self-help efforts.[19] In the Mexican case, while competition for the votes of low-income city dwellers from opposition parties has been minimal, the regime's emphasis on support-building among the poor has nevertheless ensured a relatively high level of responsiveness to the needs of low-income urban communities.[20]

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[16] See Sandra Powell, "Political Participation in the Barriadas: A Case Study," *Comparative Political Studies,* 2 (July, 1969), 211; David Collier, "Squatter Settlement Formation and the Politics of Co-optation in Peru" (Ph.D. dissertation, University of Chicago, 1971), p. 76; Donald L. Maruska, "Government Policy and Neighborhood Organizations in the Squatter Settlements of Lima" (Honors Thesis, Department of Government, Harvard University, 1972), p. 6; Alfredo Rodríguez et al., "De invasores a invadidos," *Revista Latinoamericana de Estudios Urbano Regionales,* 2, No. 6 (November, 1972), 101–142; Henry A. Dietz, "The Office and the Poblador: Perceptions and Manipulations of Housing Authorities by the Lima Urban Poor," paper presented at the Meeting of the American Society for Public Administration, Los Angeles, Calif., April 3, 1973, pp. 3, 10–11; and Sara Michl, "Urban Squatter Organization as a National Government Tool: The Case of Lima, Peru," in *Latin American Urban Research, Volume III,* ed. Francine R. Rabinovitz and F. M. Trueblood (Beverly Hills, Calif.: Sage Publications, 1973), pp. 155–178.

[17] Similar responses have been observed among Venezuelan officials. See Ray, *Politics of the Barrios of Venezuela,* pp. 89–90.

[18] See Wayne A. Cornelius, "Contemporary Mexico: A Structural Analysis of Urban Caciquismo," in *The Caciques: Oligarchical Politics and the System of Caciquismo in the Luso-Hispanic World,* ed. Robert Kern (Albuquerque, N.M.: University of New Mexico Press, 1973), pp. 145–146.

But even in communities where a strong emphasis on self-help has taken root, due either to government neglect or the attitudes of local leaders, the long-term trend appears to be toward increasing dependence upon governmental assistance. This trend is well illustrated by the data from the Mexico City research communities reported in Table 4. Attitudes and behavior supportive of self-help efforts are most prevalent in the most recently established and most poorly developed squatter settlements (Colonia Nueva and Colonia Periférico). In Colonia Militar, a squatter settlement established about 20 years ago, only a third of the respondents currently exhibit a propensity for self-help efforts.

As Lisa Peattie has observed on the basis of [illegible]ld work in a low-income urban neighborhood [illegible] Venezuela, such a community "has, of necessity, only limited power to solve its own problems," and "may in the end find itself powerless to solve what it sees as problems because the solutions lie at points which [it] cannot control."[21] Thus community residents may cooperate to install a provisional water system, but an adequate supply of potable water must depend upon permission to tap a large public water main in a nearby area. Permanent solutions to many such problems of community development must await governmental action. Particularly in older squatter settlements, the residents reach a point where they view their own efforts and resources as inadequate to satisfy their needs.

Perception of Channels for Demand Making. Whether politicized needs will be translated into actual demands upon government often depends on the ability of the potential demand maker to perceive an adequate channel for influencing government decisions. Though migrants to the city may take it upon themselves individually to contact government officials to present requests for aid, most data from Latin American cities (including my own) indicate that such individual demand-making activity is quite infrequent, and that the overwhelming majority of contacts with officials are made by the migrant as a member of a delegation of residents from his community. Since demand-making groups are typically recruited from among the members of community improvement associations, and are led by elected officials of such associations, the importance of community organizations and their leaders to demand-making activity among the migrant population becomes obvious. If no such leadership and organizational resources are available to the migrant, he is much less likely to engage in any form of political demand making.

Table 4. Self-Help Orientation Among Migrants to Mexico City, by Community[a]

Index score[b]	Colonia Nueva	Colonia Periférico	Colonia Texcoco	Colonia E. Propio	Unidad Popular	Colonia Militar
0 or 1	32.4%	50.0%	64.3%	65.0%	84.7%	64.6%
2	67.6	50.0	35.7	35.0	15.3	35.4

[a] $N=678$. Inter-community differences are statistically significant, by Chi-square test, at the .001 level.

[b] Self-help orientation is measured by a summative index constructed from responses to the following questions:

"When you think of problems of the sort you have mentioned . . . can they be solved by those living within the community, or would help be needed from outside the community?"

Scoring: *1 point*	*0 points*
"Can be solved by those living within"	"Help needed from outside"

"Have you ever worked or cooperated with other residents of this community to try to solve some problem or meet some need of the community?"

Scoring: *1 point*	*0 points*
"Yes"	"No"

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[19] See Samuel P. Huntington and Joan M. Nelson, "Socio-Economic Change and Political Participation: Report to the Civic Participation Division of the Agency for International Development," Center for International Affairs, Harvard University, September, 1973, p. 5–33.

[20] For a fuller discussion of the dimensions of governmental performance affecting low-income residents of Mexico City, see Wayne A. Cornelius, "The Impact of Governmental Performance on Political Attitudes and Behavior: The Case of the Urban Poor in Mexico City," in *Latin American Urban Research, Volume III,* ed. Rabinovitz and Trueblood, pp. 213–251.

[21] Lisa R. Peattie, *The View from the Barrio* (Ann Arbor: University of Michigan Press, 1968), p. 67.

Numerous studies of political participation in both Third World and advanced industrial nations have demonstrated that those who are involved in any type of formal voluntary organization participate in politics more frequently than citizens who are not so involved.[22] These studies have also shown that organizational involvement may lead to increased political participation *in the absence* of other personal attributes or attitudes such as high socioeconomic status, a sense of political efficacy, or a high level of political information. Moreover, there is specific evidence from studies of low-income communities in Santiago, Chile, and Lima, Peru, indicating that members of community improvement organizations are considerably more likely to engage in political demand making than are their neighbors who have not been involved in improvement organizations.[23] Among the migrants interviewed in Mexico City, those who had participated in community improvement organizations were *five times* more likely to have engaged in demand making than the nonparticipants.

Participant observation in the Mexican research communities suggests that community improvement organizations provide important psychological support for the low-income migrant seeking to make demands upon government. Participation in communal demand-making efforts greatly reduces the amount of individual initiative required, and provides the potential demand maker with a clear strategy of action for influencing government decisions.[24] Moreover, to the extent that community-based organizations are successful in securing benefits through collective demand-making attempts, they strengthen the migrant's perceptions of the political system as being subject to manipulation and increase his sense of political efficacy. Such attitudes and perceptions increase his propensity to engage in future demand-making attempts.

Participation in collective demand making channeled through community organizations may not, however, increase the migrant's confidence in dealing with government officials as an individual. Community improvement organizations in low-income urban neighborhoods seem to affect the attitudes of their members in much the same way that certain types of peasant unions do in rural areas:

> The syndicate is something concretely visible and understandable to peasants. No ability to imagine oneself individually trying to influence national government is required for a sense of efficacy. A peasant need only learn that the syndicate exists as broker between himself and the government—and this lesson is learned by observing and especially by participating in the brokerage activities of the union. . . . Consequently, if the union is adept at brokerage . . . there is a greater likelihood that its members will be taught that they are, in fact, efficacious.[25]

This helps to explain why so few of the migrants to Mexico City who have engaged in collective demand-making efforts have *also* attempted individually to contact government officials about personal or family-related problems (see next section). Individual demand making with a purely particularistic referent apparently requires a different set of perceptions of the political system and one's competence to influence its outputs—perceptions which do not necessarily result from collective demand-making experiences.

Political Demands. In most countries for which reasonably comparable data are available (see Table 5), demand making appears to be a relatively uncommon form of political participation among the general population. Even in the United States, only about a third of the population report that they have contacted a government official at least once. Among migrants interviewed in Mexico City, about one out of four has contacted a political or governmental official on at least one occasion.[26] The incidence of demand-making activity varies considerably from one community to another. The proportion of migrants who have engaged in some form of demand making varies from 45 per cent in Colonia Nueva (the most recently established squatter settlement) to 10 per cent in Colonia Texcoco (the commercial subdivision). These inter-community differences can be attributed partly to the nature of developmental problems which each community has confronted (e.g., demand making has been more frequent among migrants residing in illegally formed settlements, due to extended negotiations for government recognition of tenure rights); but differences in community leadership and organizational structure appear to be equally important determinants of demand-making propensities among the residents.

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[22] See especially Gabriel A. Almond and Sidney Verba, *The Civic Culture* (Princeton: Princeton University Press, 1963), pp. 262–265; William Erbe, "Social Involvement and Political Activity: A Replication and Elaboration," *American Sociological Review*, 29 (April, 1964), 198; Norman H. Nie, G. Bingham Powell, and Kenneth Prewitt, "Social Structure and Political Participation: Developmental Relationships," *American Political Science Review*, 63 (September, 1969), 813; William H. Form and Joan Huber, "Income, Race, and the Ideology of Political Efficacy," *Journal of Politics*, 33 (August, 1971), 688; Verba and Nie, *Participation in America*, pp. 174–208; and Paul Burstein, "Social Structure and Individual Political Participation in Five Countries," *American Journal of Sociology*, 77 (May, 1972), 1087.

[23] James W. McKenney, "Voluntary Associations and Political Integration: An Exploratory Study of the Role of Voluntary Association Membership in the Political Socialization of Urban Lower-Class Residents of Santiago, Chile, and Lima, Peru" (Ph.D. dissertation, University of Oregon, 1969), p. 184; Daniel M. Goldrich, "Political Organization and the Politicization of the Poblador," *Comparative Political Studies*, 3 (July, 1970), 192; and Raymond B. Pratt, "Community Political Organizations and Lower Class Politization in Two Latin American Cities," *Journal of Developing Nations*, 5 (July, 1971), 536–537.

[24] Cf. Goldrich, "Political Organization," p. 192; Pratt, "Community Political Organizations," p. 537.

[25] John R. Mathiason and John D. Powell, "Participation and Efficacy: Aspects of Peasant Involvement in Political Mobilization," *Comparative Politics*, 4 (April, 1972), 327.

[26] Due to the wording of the relevant survey questions, all of these reported contacts with officials involved a personal visit to the office of some functionary of the government or the official party. Thus the data do not reflect contacting through letters or

When migrants contact government officials, what is the substantive nature of their demands? The relevant data for the Mexico City sample are presented in Figure 3. Nearly two-thirds of the reported contacts with officials were concerned with the need for security of tenure. In the period immediately following a land invasion, demands relating to this need are defensive in nature—i.e., requests that the settlers be permitted to remain on the land without fear of eviction by the government. Subsequently the tenure-related demands take the form of petitions for legalization of the community and the granting of individual land titles to its residents. Petitions relating to other kinds of community needs account for most of the remaining demand-making attempts reported by the respondents.

In their study of a national sample of the U.S. population, Verba and Nie found that about one-third of all citizen-initiated contacts with government officials were concerned with particularistic problems relating only to the individual demand-maker or his family, while the remaining two-thirds had a broader social referent.[27] A similar pattern emerges from the data on demand making by migrants to Mexico City, although the frequency of contacting with regard to particularistic problems is even lower among this population than among U.S. citizens. Only about 3 per cent of the contacts with government officials reported by the migrants dealt with personal or family-

Table 5. Frequency of Citizen-Initiated Contact With Government Officials in Mexico, Peru, Chile, Brazil, United States, and India

Country	%*	Sample Composition
Mexico[a]	25.8	Lower-class male, migrant heads of family in Mexico City (six low-income neighborhoods)
Mexico[b]	20.0	Lower-class adult residents of city of Jalapa
Mexico[c]	17.0	Male household heads in four rural communities, State of Michoacán
Peru[d]	32.3	Lower-class male, migrant heads of family in four squatter settlements of Lima
Peru[e]	5.9	Lower-class male household heads in Lima
Chile[f]	11.1	Lower-class male household heads in Santiago
Brazil[g]	42.0	Lower-class male residents of squatter settlements in Rio de Janeiro
U.S.[h]	33.0	National sample of adult population
U.S.[i]	27.0	Registered voters in New Haven, Connecticut
U.S.[j]	27.5	Adult residents of Nashville, Tennessee.
U.S.[k]	12.0	Adult residents of five low-income urban neighborhoods in several cities
India[l]	20.0	National sample of adult population

* Percent who have contacted a government official on one or more occasions.

[a] Cornelius, "Poverty and Politics in Urban Mexico."

[b] Fagen and Tuohy, *Politics and Privilege in a Mexican City*, p. 89.

[c] Arterton, "Patterns of Political Participation in Four Rural Mexican Villages."

[d] Dietz, "Becoming a Poblador."

[e] McKenney, "Voluntary Associations and Political Integration," p. 181.

[f] McKenney, "Voluntary Associations and Political Integration," p. 181.

[g] Janice E. Perlman, "The Fate of Migrants in Rio's Favelas" (Ph.D. dissertation, Massachusetts Institute of Technology, 1971), p. 317.

[h] Verba and Nie, *Participation in America*, p. 106.

[i] Robert Dahl, *Who Governs?* (New Haven: Yale University Press, 1961), p. 279.

[j] Graham and Pride, "Styles of Political Participation," p. 41.

[k] Curt Lamb, "Political Culture in Poor America," cited in Perlman, "The Fate of Migrants in Rio's Favelas," p. 316.

[l] John O. Field, "Partisanship in India: A Survey Analysis" (Ph.D. dissertation, Stanford University, 1973), p. 355A.

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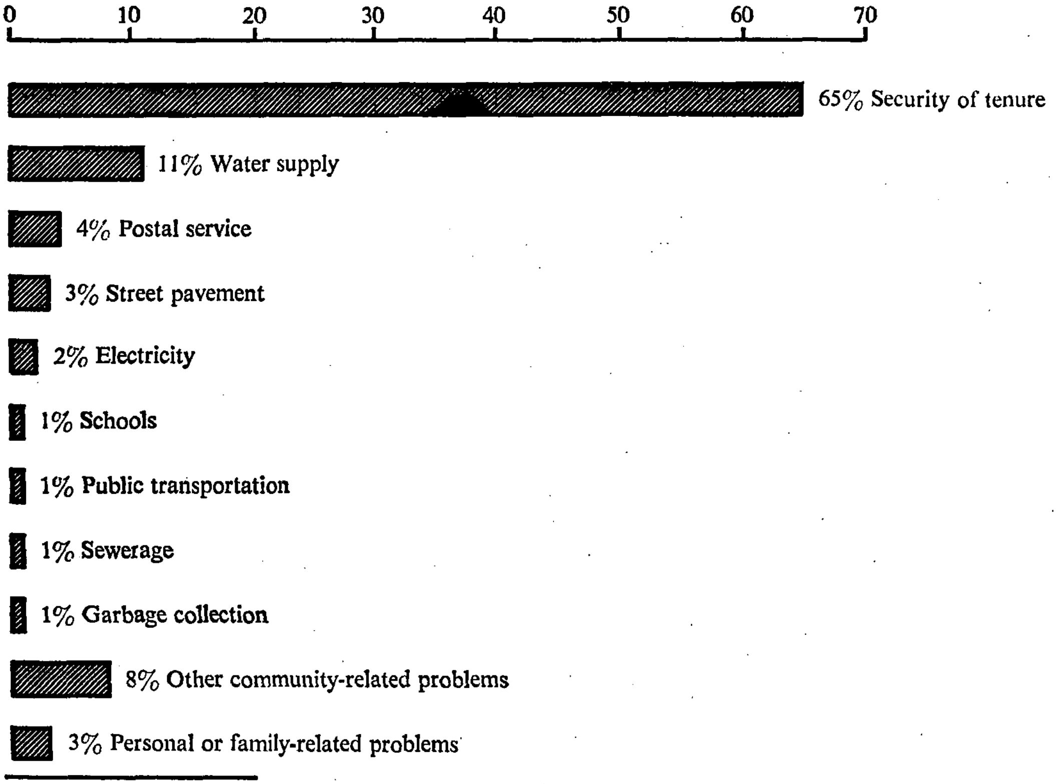
petitions signed by community residents and sent to government officials. The actual wording of the questions is as follows: "Have you ever gone to an office of the P.R.I.? [If Yes:] For What purpose?"; "Have you ever personally—either alone or with other residents of the community—gone to see some official of [in Colonia Texcoco: the Municipal Government; elsewhere: the Department of the Federal District], or some other person of influence in the government, about some problem or need?"

[27] Verba and Nie, *Participation in America*, p. 67.



[a] $N=112$. Don't know/no response cases excluded from table.

Figure 3. Subject Matter of Citizen-Initiated Contacts with Government Officials, Among Migrants to Mexico City.[a]

related needs.[28] Thus it is clear that the migrant's agenda for governmental action is strongly influenced by the priority he attaches to certain problems and needs which affect him most directly as a member of a given residential community. Most of the demands emanating from the migrant population are demands articulated in the name of an entire community, for collective goods benefitting all residents of the community. Highly particularistic requests for jobs or other special dispensations benefitting only the individual demand maker or his family constitute a very small proportion of migrant demands upon government.

This low frequency of particularistic contacting also reflects the migrant's perceptions of what types of needs are most amenable to satisfaction through governmental action. Perhaps the most important reason that individual needs for housing and employment are not frequent objects of demand making is that the migrant usually perceives the satisfaction of such needs as an *individual* responsibility.[29] As government-employed engineers supervising the construction of housing by residents of some low-income settlements in Mexico City have discovered, the migrant may actively resist governmental involvement in house construction, which he regards as interference in a personal matter. Alternatively, despite the urgency of such problems as unemployment, seeking government assistance may not be viewed as an effective way to deal with the problem, either because of the complexities and delays involved in obtaining government help or because of low governmental responsiveness to petitions concerned with individual needs. By contrast, securing land tenure rights and obtaining access to basic urban services are problems not viewed as being susceptible to "individual" solution. Even the most affluent residents of illegally formed communities in Mexico City have found it impossible to acquire title to their land through individual negotiations with government officials.[30] Security of tenure is achieved only through a government decision to recognize the land occupancy rights of all residents of a community; and such a decision is almost always the result of collective demand-making attempts by the residents.

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[28] Among the reported contacts with functionaries of the official party, about 13 per cent had a particularistic (personal or family) referent; but contacts with P.R.I. officials represent only 9 per cent of the total number of demand-making attempts reported by the respondents.

[29] Fagen and Tuohy also found that among residents of the city of Jalapa, Mexico, "the major source of personal difficulties—the economic situation, and specifically the matter of jobs—is still widely perceived in the first instance as a private or individual domain. Individual mobility in the context of aggregate national growth is seen as the mechanism through which economic conditions will be ameliorated. A sense of personal blame and responsibility thus colors perceptions of these problems. . . . When Jalapeños were asked, 'If a man is unemployed, who has the chief responsibility for finding work for him?,' over 70 per cent said that the man himself had that responsibility. Five per cent said 'friends and relatives' and 23 per cent mentioned the government or some other organization. . . . Despite substantial diffuse reliance on government, economics has not been connected with politics in ways that lead the average citizen to seek political redress for economic grievances" (Fagen and Tuohy, *Politics and Privilege in a Mexican City*, pp. 142–143). Similarly privatized perceptions of the problem of unemployment have been observed among residents of squatter settlements in Lima, Peru (Rodríguez et al., "De invasores a invadidos," p. 131).

Strategies of Demand Making

What methods are likely to be employed by migrants engaged in demand-making attempts? To help identify their preferences among a number of alternative demand-making strategies, migrants interviewed in Mexico City were presented with the following hypothetical situation: "Suppose a group of people in this city strongly feels that the government is treating them unfairly. I'm going to read a list of some of the things these people might do to try to get the government to change the situation. Which do you think would be the most effective way?" The responses, reported in Table 6, indicate a very marked preference for working through conventional bureaucratic channels (specifically, by sending a group representative to talk with a government official).

Table 6. Preferred Strategy of Demand Making Among Migrants to Mexico City[a]

Sending a representative to a government office	47.9%
Working through personal connections with public officials	25.2
Working through the official party (PRI)	11.5
Organizing a protest demonstration	9.5
Organizing public meetings or rallies to get other people interested in the problem	5.8

[a] $N=545$. Don't know/no response cases excluded from table.

Nearly half of the respondents felt that this would be the most effective strategy for influencing the government. Since investigation of past and current demand-making efforts of residents of the research communities revealed that they often sought to circumvent regular bureaucratic processes by entering into patron-client relationships with key politicians or bureaucrats, this was also specified as an alternative in the survey question regarding demand-making strategies. About one in four of the respondents endorsed this type of approach, defined in the question as "working through personal connections with public officials." Much smaller proportions of respondents asserted the efficacy of working through the official party to influence the government's actions, organizing a protest demonstration, and trying to exert pressure on the government by mobilizing the support of people outside the demand-making group through public meetings or rallies.

Parties as Demand Articulators. Working with party politicians in order to influence decisions of the government bureaucracy is a strategy employed frequently among the poor in some Latin American cities, primarily those in which competitive multi-party systems are operative.[31] Yet data from several of these cities reveal considerable disillusionment with political parties as

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Such perceptions and expectations may help to explain the rarity of collective political action organized around the issue of unemployment among low-income residents of Latin American cities. Cf. Peter C. W. Gutkind, "From the Energy of Despair to the Anger of Despair: The Transition from Social Circulation to Political Consciousness among the Urban Poor in Africa," *Canadian Journal of African Studies,* 7, No. 2 (1973), 179–198.

[30] Individual negotiations to secure land titles have apparently been feasible and successful in some Third World cities with large squatter populations. See, for example, Joan M. Nelson, "Migration, Integration of Migrants, and the Problem of Squatter Settlements in Seoul, Korea: Report on a Field Study for the Smithsonian Institution," mimeographed, Washington, D.C., July, 1972, p. 20.

[31] See Peattie, *The View from the Barrio,* p. 67; Anthony Leeds, "The Significant Variables Determining the Character of Squatter Settlements," *América Latina,* 12 (July-September, 1969), pp. 78–79; and David G. Epstein, *Brasília: Plan and Reality* (Berkeley and Los Angeles: University of California Press, 1973), pp. 113, 130–131. Cf. Aprodicio A. Laquian, "Slums and Squatters in South and Southeast Asia," in *Urbanization and National Development: South and Southeast Asia Urban Affairs Annual, Vol. I,* ed. Leo Jakobson and Ved Prakash (Beverly Hills, Calif.: Sage Publications, 1972), p. 199; and Kemal H. Karpat, "The Politics of Transition: Political Attitudes and Party Affiliation in Turkish Gecekondu," unpublished paper, Department of History, University of Wisconsin, Madison, 1972, p. 26.

articulators of lower-class needs.[32] The findings from Mexico City are consistent with this general Latin American pattern. As shown in Table 6, the official party is not widely viewed by migrants residing in the research communities as an effective vehicle for demand making, especially when the matter at issue is some community problem or need. Informal interviews with community residents and their leaders further revealed that working with opposition political parties is not regarded as a rational strategy of goal attainment. These parties have no independent resources of utility to low-income communities; and their electoral weakness vis-à-vis the PRI insures that they have no chance of gaining control of the government agencies which do control the allocation of resources to such communities.

Despite the absence of a credible threat to PRI hegemony, the Mexican regime continues to place a high premium upon maintaining or even increasing the official party's already overwhelming margin of support at the polls. Yet low-income communities find it difficult or impossible to take advantage of this emphasis on voter mobilization to further their interests. Most *colonias proletarias* in Mexican cities are relatively small, by comparison with similar communities in other Latin American cities, and hence unable to deliver a substantial bloc of votes to the PRI. Moreover, the boundaries of electoral districts are rarely coterminous with the boundaries of specific *colonias;* hence electoral statistics are not gathered and reported in such a way as to accrue to the credit or discredit of a given *colonia.* In short, such communities typically have little or no bargaining power with government officials based on a capacity to grant or withhold electoral support from the regime.

In some provincial Mexican cities, the official party has been observed to function effectively as "a decentralized clearinghouse through which [lower-class] demands can be articulated and, at times, attended to."[33] There is, however, considerable variation in the performance of this function by the PRI, from city to city and even within single cities. For example, in some sections of the Mexico City metropolitan area, community improvement organizations are tied administratively into the local PRI structure, and direct their petitioning for community improvements toward the Party office. Where such direct ties do not exist, demand making is not usually filtered through the Party apparatus. In general, working exclusively through PRI functionaries is viewed by residents of the *colonias proletarias* as a highly indirect strategy of demand making. They believe that better results can be achieved through direct contacting of government officials, relying on official party contacts only in the event that negotiations with government decision-makers break down or produce an unfavorable outcome. Such beliefs reveal a highly realistic perception of the locus of decisional authority in the Mexican political system: It is government bureaucrats rather than party politicians who control the allocation of resources most relevant to the solution of community problems.[34]

The PRI, mainly through its Popular Sector, is the source of a limited amount of patronage for individual residents of low-income neighborhoods. It may, for example, provide medical assistance, assist in obtaining a license or permit for commercial activity, or help to enroll a child in an already overcrowded neighborhood school. But as we have already noted, demand making with an individualized referent is very infrequent among the people included in this study, regardless of the type of official (PRI or government) toward which it is directed.

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[32] See Peattie, *The View from the Barrio,* pp. 69–70; Ray, *Politics of the Barrios of Venezuela,* pp. 80, 98–127; Powell, "Political Participation in the Barriadas"; Lutz, "Self-Help Neighborhood Organizations," pp. 194–195; Raymond B. Pratt, "Parties, Neighborhood Associations, and the Politicization of the Urban Poor in Latin America," *Midwest Journal of Political Science,* 15 (August, 1971), 509–510; Pratt, "Community Political Organizations," p. 538; John D. Uzzell, "Bound for Places I'm Not Known To: Adaptation of Migrants and Residence in Four Irregular Settlements in Lima, Peru" (Ph.D. dissertation, University of Texas, Austin, 1972), p. 240; Susan Eckstein, "The Poverty of Revolution: A Study of Social, Economic, and Political Inequality in a Central City Area, a Squatter Settlement, and a Low Cost Housing Project in Mexico City" (Ph.D. dissertation, Columbia University, 1972).

[33] Fagen and Tuohy, *Politics and Privilege in a Mexican City,* pp. 30–31. Cf. Antonio Ugalde, *Power and Conflict in a Mexican Community* (Albuquerque: University of New Mexico Press, 1970), pp. 139–149; and Antonio Ugalde, Leslie Olson, David Schers, and Miguel Von Hoegen, *The Urbanization Process of a Poor Mexican Neighborhood: The Case of San Felipe del Real Adicional, Juárez* (Austin, Texas: Institute of Latin American Studies, University of Texas at Austin, 1974).

[34] Drawing primarily upon research conducted in the provincial city of Jalapa, Mexico, one investigator has concluded that "Only persons and organizations which lack adequate political resources articulate demands to and through the Party. And since it is not a major decision-maker, those articulators can only hope for the Party's services as a broker or intermediary connected with the government." See William S. Tuohy, "Centralism and Political Elite Behavior in Mexico," in *Development Administration in Latin America,* ed. C. E. Thurber and Lawrence Graham (Durham, N.C.: Duke University 1973). See also David Schers, *The Popular Sector of the Partido Revolucionario Institucional in Mexico* (Tel Aviv: The David Horowitz Institute for the Research of Developing Countries, Tel Aviv University, Research Report No. 1, November, 1972), p. 68.

The Politics of Protest. The low degree of efficacy attached by migrants to protest tactics as a strategy of demand making is particularly significant, in view of the very extensive literature attributing to cityward migrants a high potential for protest behavior. In response to another interview question, almost half of the migrants interviewed in Mexico City expressed the belief that it would be wrong for a man to join in a protest march or demonstration "to get public officials to correct an unjust situation." The migrants were also quick to condemn, by a margin of 59 per cent to 15 per cent (with 26 per cent expressing no opinion), the protest activities of students who demonstrated against government violations of university autonomy and other grievances in Mexico City during the fall of 1968. Not surprisingly, levels of migrant involvement in overt protest activities were quite low. Only 3 per cent had ever gone with a group of people to protest or complain to a public official about some problem, and only about 2 per cent of the migrants had ever attended a protest meeting or participated in a protest demonstration. In my interviews with community leaders, several expressed the view that if a need of their community were particularly acute and the government had ignored community petitions regarding the need, they would try to organize a protest demonstration outside the office of some high-ranking government functionary; but none were able to document an instance in which this kind of action was actually taken.

It is also noteworthy that even in the context of conventional petitioning activity, communications by residents of the research communities were never formulated as protests, complaints, or expressions of anger at being deprived unjustly of some particular service or government benefit.[35] Both the written petitions and the language used in presenting them to government officials are replete with expressions of deference toward authority and expressions of hope that the benefits being sought will be granted by the government as a "special favor" to residents of the community.

The migrant's deferential, non-threatening approach to negotiating with public officials, as well as his avoidance of protest tactics in general, are rooted in a world view which emphasizes "conformity to the rules rather than manipulation of them."[36] Lower-class migrants to Latin American cities, like 19th-century immigrants to the United States, tend to view themselves as

> foreigners to the city. As such, they are more prone, at least initially, to question the legitimacy of their own presence as audacious newcomers than the general urban order. The city which offers the opportunity of upward [socioeconomic] ascent also sets its own hard rules. The migrant's decision to come to the city, like the immigrant's choice of settling in a new country, implies a tacit acceptance of these rules.[37]

Thus the migrant's preference for demand making through conventional bureaucratic channels or informal clientage relationships can be viewed as a reflection of his commitment to abiding by the political rules of the game in his new environment. Clearly illegal or violent forms of political action are never considered as a means of bringing pressure to bear on the authorities, because they represent a fundamental violation of these rules. Participation in a land invasion is usually the only instance in which the migrant may be willing to deliberately contravene social and political norms. And since such invasions are often carried out with the covert support of politicians or government officials, it is unlikely that they are even construed by many of the participants as a form of anti-system political action.[38]

The migrant's low tolerance for the risks and uncertainties of unconventional political action also reflects his perceptions of what kinds of demand-making strategies are most *instrumental* in gaining tangible benefits for himself and his community. He sees little or nothing to be gained by offending the authorities through protest activities, and perhaps a great deal to lose by doing so.

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[35] The reluctance of residents of low-income neighborhoods to formally protest or complain about deficiencies in public services is strikingly reflected in a recent compilation of data on citizen complaints received by the city government and Mexico City newspapers and television stations. During the period from January through June, 1973, more than 61 per cent of the complaints received were concerned with trash collection (a problem with very low priority in most low-income neighborhoods); and more than three-quarters of all complaints came from people residing in predominantly upper or middle-income sections of the city. See *El Día* (Mexico City), July 8, 1973, p. 13.

[36] Douglas Butterworth, "Two Small Groups: A Comparison of Migrants and Non-Migrants in Mexico City," *Urban Anthropology*, 1 (Spring, 1972), 41.

[37] Alejandro Portes, "Rationality in the Slum," *Comparative Studies in Society and History*, 14 (June, 1972), 284.

[38] On the involvement of government bureaucrats and politicians in urban land invasions in Chile, Peru, and Venezuela, see Franz Vanderschueren, "Political Significance of Neighborhood Committees in the Settlements of Santiago," in *The Chilean Road to Socialism*, ed. Dale L. Johnson (Garden City, N.Y.: Doubleday-Anchor Books, 1973), pp. 267–268; Collier, "Squatter Settlement Formation and the Politics of Co-optation in Peru," *passim;* and Ray, *Politics of the Barrios of Venezuela*, pp. 33–37, 42–43.

Examples of both the potential costs and the futility of protest activity as a strategy of goal attainment abound in recent Mexican history. In the countryside, open protest by peasants has been systematically and ruthlessly suppressed; and the 1968 protest demonstrations in Mexico City were violently repressed by police and military forces, resulting in the deaths of up to 300 persons and the sentencing of students accused of organizing the demonstrations to 15-to-20-year prison terms. (Most of them had been released by 1971.) More lives were lost in June, 1971, when a group of high-level government and PRI officials sponsored an attack by a terrorist street gang upon student demonstrators in the capital. Since 1968 perhaps a dozen more student protesters have been killed by riot police in Puebla, Culiacán, and other provincial cities.[39] These events, extensively reported in the mass media, have greatly increased popular awareness of the formidable government coercive apparatus which can be activated in response to overt protest. As in some contemporary U.S. cities where protest has been met with violent actions by the authorities, such actions are likely to reinforce tendencies toward less aggressive strategies of political demand making among low-income sectors of the population.[40]

Moreover, from the standpoint of the community leaders who typically organize demand-making efforts, protest-oriented strategies are risky and unproductive because they may damage the clientage relationships with government bureaucrats or politicians which they have labored so long to establish. Overt protest actions threaten the reputations of such officials as "capable managers" and hence, their prospects for future advancement within the regime.[41] When confronted with such a threat, an official is more likely to respond with retaliatory measures than with positive action on the offending community's requests for assistance. Henceforth the community's leaders are perceived as troublemakers and are likely to be refused access to the offended official if further contact is attempted.

Finally, the demonstrated efficacy of *conventional* demand-making strategies in securing community improvements serves to reduce the attractiveness of protest tactics among both community leaders and their followers. In Mexico City, the authorities have worked diligently to cultivate a popular image of accessibility and responsiveness to lower-class needs, even though their responses to petitions from low-income groups more often take the form of symbolic reassurances (e.g., making inspection tours, committing the problem at hand to technical study) than allocations of substantive benefits. Petitioners from low-income neighborhoods are seldom refused access to lower or middle-echelon government officials; and those who have visited a government or PRI office overwhelmingly report that they were treated courteously and fairly by the officials they contacted.[42] Furthermore, more than two-thirds of the migrants interviewed in Mexico City who had participated in demand-making attempts report that such attempts were successful in the sense of having contributed to the solution to some community problem. Table 7 summarizes the outcomes of demand-making attempts by residents of the six research communities over the period since settlement of the communities began, as reported in interviews with community leaders. Although these leaders are more likely to view any benefit received by their communities as the outcome of demand-making attempts rather than through governmental initiative, detailed investigation—including interviews with the relevant government decision makers—suggests that most of the benefits cited by community leaders would not have been provided in the absence of overt demand-making efforts.[43]

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[39] In a recent analysis of historical trends in the use of violence as a repressive tool by Mexican authorities, Evelyn P. Stevens notes that such practices are still fairly common at the local level in rural areas and even at the level of state governments in some parts of the country. See E. P. Stevens, *Protest and Response in Mexico* (Cambridge, Mass.: M.I.T. Press, 1974), chap. 9. The use of small-scale violence against peasants in rural areas is discussed further in James D. Cockcroft, "Coercion and Ideology in Mexican Politics," in *Dependence and Underdevelopment: Latin America's Political Economy*, ed. J. D. Cockcroft, Dale L. Johnson, and Andre Gunder Frank (Garden City, N.Y.: Doubleday-Anchor Books, 1972), p. 254. Cf. Fagen and Tuohy, *Politics and Privilege in a Mexican City*, p. 27.

[40] See Peter K. Eisinger, "The Conditions of Protest Behavior in American Cities," *American Political Science Review*, 67 (March, 1973), 27. Fear of physical and economic coercion has also depressed political participation among minority groups in the U.S. South. See the empirical analysis reported in Lester M. Salamon and Steven van Evera, "Fear, Apathy, and Discrimination: A Test of Three Explanations of Political Participation," *American Political Science Review*, 67 (December, 1973).

[41] Fagen and Tuohy, *Politics and Privilege in a Mexican City*, p. 71.

[42] See Cornelius, "Impact of Governmental Performance on Political Attitudes and Behavior," p. 224.

[43] Lest it be assumed that the research communities in Mexico City have benefitted from an atypically high level of governmental responsiveness, it should be noted that most of the benefits which they have received are the outcomes of difficult and protracted demand-making experiences, often extending over a period of five to ten years. Moreover, many serious needs of the communities have gone unmet. Even today, functioning sewerage systems, schools, public markets, and health care facilities are inadequate or non-existent in three of the six communities. In some cases these unmet needs reflect

Table 7. Outcomes of Demand-Making Efforts, by Community

Colonia Nueva	Colonia Periférico	Colonia Texcoco	Colonia E. Propio	Unidad Popular	Colonia Militar
Destruction of stone wall erected by former landowner to restrict access to community Reconstruction of houses destroyed by fire Expropriation of land occupied by community (first step toward legalization of community) Construction of primary school Installation of regular electricity supply system	Introduction of regular bus service Installation of provisional water system Construction of primary school Installation of regular electricity supply system and street lighting Pavement of main street Installation of 1 public telephone Expropriation of land occupied by community	Installation of preliminary electricity supply system Construction of secondary school Installation of provisional water supply system Construction of child-care center	(None—water and sewerage systems, electricity, and other services and improvements installed at government initiative)	Installation of water purification plant Postponement of property tax payments Construction of 2 primary schools, 1 secondary school Installation of several public telephones Paving of some streets	Government recognition of land tenure rights Installation of electricity for individual dwellings and street lighting Installation of sewerage system Construction of primary school Paving of streets Installation of public and private telephones

Even the residents of communities whose petitions for recognition of tenure rights or urban improvements have not yet elicited positive responses from the government are aware of nearby communities which *have* been so favored. These examples foster perceptions of the political opportunity structure as being relatively open, thus eliminating the necessity for resort to protest tactics. Migrants residing in the city's low-income neighborhoods are thus likely to feel that by working together they are capable of influencing at least some of the decisions of government affecting the development of their community; and most believe that they can do so through conventional, nonconflictual means.[44] If his community's petitions are consistently denied by the government, and deeply felt needs seem likely to remain unsatisfied in the foreseeable future, the migrant is far more likely to respond to the situation by moving to another community with better developmental prospects than by engaging in some form of protest activity.[45]

Conventional Demand-Making Strategies. The most commonly used strategy among the migrant poor in Mexico City is that of forming a committee or delegation of community residents to visit the office of a government official and present a formal request for assistance. What kinds of officials are the primary targets of such petitioning? In nearly half of the demand-making attempts reported by migrants residing in the six research communities, some high-ranking official within the Department of the Federal District (D.D.F.) had been contacted. (The D.D.F. serves as the principal organ of local government for Mexico City and its immediate metropolitan area.) Another 31 per cent of the demand-making attempts had been directed toward the head of the Office of Colonias, an appendage of the D.D.F. having primary responsibility for the problems of low-income neighborhoods of the city. Only 9 per cent of the reported contacts with officials involved a functionary of the official party, and only 6 per cent involved a *delegado*

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an absence or suspension of demand-making efforts by residents of the community; in others, a lack of skill or commitment among community leaders has led to unsuccessful negotiations with the authorities. Still other unmet needs reflect a sheer lack of responsiveness by government officials to persistent demand-making attempts.

[44] Cf. Alejandro Portes, "Cuatro poblaciones: informe preliminar sobre situación y aspiraciones de grupos marginados en el gran Santiago," Centro de Estudios Socio-económicos de la Universidad de Chile, August, 1969, pp. 39–39A.

[45] Of course, numerous factors beyond governmental neglect may contribute to a decision to abandon a community suffering from insecurity of tenure or service deprivation. A useful conceptualization of the cost/benefit calculations performed by low-income migrants to Lima, Peru, with regard to intracity residential mobility is provided in Douglas Uzzell, "The Interaction of Population and Locality in the Development of Squatter Settlements in Lima," in *Anthropological Perspectives on Latin American Urbanization: Latin American Urban Research, Volume IV*, ed. Wayne A. Cornelius and Felicity M. Trueblood (Beverly Hills, Calif.: Sage Publications, 1974). Much of Uzzell's analysis seems applicable to the population under study in this paper. Cf. John M. Orbell and Toro Uno, "A Theory of Neighborhood Problem-Solving: Political Action vs. Residential Mobility," *American Political Science Review*, 66 (June, 1972), 471–489.

(a lower-echelon official of the D.D.F., who is nominally responsible for all residential zones within one of the major political subdivisions of the Federal District).

The migrant's choice of demand-making targets exhibits an accurate understanding of the highly centralized nature of the government decision-making process in Mexico. In attempting to influence the outcomes of this process, "both efficiency and security argue that it is wise to seek support and decisions as close to the center as possible lest time be wasted with those who don't count and whose decisions may subsequently be overturned."[46] Thus residents of a community seeking a water supply system may send copies of their petition to a *delegado*, to the head of the city's water department, and to other middle-echelon officials, but will seek to negotiate directly with the Governor of the Federal District or one of his closest deputies. Direct approaches are also made occasionally to the First Lady or an assistant to the President of the Republic, despite the rarity with which audiences are granted by these individuals. Residents of one of the six research communities, located just outside the Federal District in the State of Mexico, often took advantage of the public audiences held each Saturday by the second-highest official of the state government to present petitions for assistance, rather than channeling them through subordinate officials of the municipal government. Of course, access to officials with personal political power and decisional autonomy is more difficult to obtain; yet the potential rewards flowing from contacts with such officials are likely to be far more substantial and immediate than any benefits provided by their subordinates.

In presenting a request for government assistance, community leaders take pains to emphasize the willingness of the residents to pay for any services extended to them ("We don't want you to 'give' us anything . . ."). Since residents of low-income communities in Mexico City are in fact required by the government to repay the full cost of installing basic urban services and improvements (usually through individual resident assessments, to be paid over a period of five to ten years), evidence of previous, successful fund-raising campaigns to finance community improvements may be presented to help assure officials of the community's credit-worthiness. Offers of free manual labor to help in the installation of improvements are also made routinely. Occasionally, gifts or outright cash bribes are presented to an official with whom negotiations are being conducted.

If these and other inducements fail to produce the desired results, the demand makers may attempt to exert indirect pressure on the government by publicizing their needs through the mass media.[47] One major newspaper in Mexico City maintains a section for "complaints and petitions" directed at government officials by residents of predominantly low-income neighborhoods; and delegations of residents from such communities frequently visit the newspaper's offices to present copies of their petitions to the government or request news coverage of the situation in their community. The resulting stories may be highly embarrassing to unresponsive bureaucrats. Another indirect tactic involves asking official party functionaries to intercede with government officials responsible for acting on their petitions.

Negotiations for official recognition of land tenure rights and installation of basic urban services are usually quite complex and require frequent attention by the demand makers over a long period of time. A government decision to "regularize" an illegally formed community must be followed by a number of additional governmental acts before individual tenure rights can be considered secure. These actions include expropriation of the land, if privately owned before it was invaded; initiation of technical studies to determine the feasibility of installing certain types of services and improvements; completion of a census of current occupants of land within the community; subdivision of the land into parcels assigned to individual residents; and determination of land prices, service assessments, and terms of payment. At any of these stages in the "regularization" of the community, serious difficulties and delays may develop; hence the necessity for numerous follow-up visits to check on the processing of the community's petitions and cultivate personal relationships with the officials in charge.[48]

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[46] Fagen and Tuohy, *Politics and Privilege in a Mexican City*, p. 23. For more detailed discussions of the mechanisms and consequences of political centralism in Mexico, see Tuohy, "Centralism and Political Elite Behavior in Mexico"; William L. Furlong, "Obstacles to Political Development: Case Studies of Center and Periphery in Northern Mexico," paper presented at the Annual Meeting of the American Political Science Association, Washington, D.C., September, 1972; and Susan K. Purcell and John F. H. Purcell, "Community Power and Benefits from the Nation: The Case of Mexico," in Rabinovitz and Trueblood, eds., *Latin American Urban Research, Volume III*, pp. 49–76.

[47] Cf. Peattie, *The View from the Barrio*, p. 75; Ray, *Politics of the Barrios of Venezuela*, pp. 96–97.

[48] Repeated contacts with the same official also serve importantly to fix in his mind the history and problems of the demand-making community, thus enabling him to distinguish more readily the petitions of that specific community from those of numerous similar communities competing for his attention. See Dietz, "The Office and the Poblador," p. 20.

During this follow-up period, the demand makers take advantage of every possible opportunity to renew their claims. Officials making inspection tours, social service workers of the ruling party, or politicians on electoral campaign visits may be confronted by a delegation of residents from the demand-making community to draw attention to their petitions. A disaster of some sort—particularly if on a scale sufficient to warrant coverage by the mass media—provides additional opportunities to dramatize the demand makers' plight and bring pressure to bear on the authorities to grant their requests. For example, a fire set by the original owner of the land occupied by Colonia Nueva squatters, resulting in the death of seven people, greatly facilitated that community's negotiations for official recognition of tenure rights.

The competition for government attention from many other communities whose needs are equally acute is often fierce, but persistence and skill in negotiating with officials can often make a great difference, particularly in a city where the allocation of government benefits to low-income areas is not governed by routinized, rationalistic developmental criteria. In their study of politics in the city of Jalapa, Mexico, Fagan and Tuohy noted a total absence of developmental planning of the sort that might provide criteria for bringing together and judging conflicting claims on scarce resources.[49] Much the same situation prevails in Mexico City, where the idea of comprehensive physical planning, even as an eventual goal, was discarded shortly after World War II and never revived.[50] Neither has there been any government office in the capital having responsibility for overall planning and resource allocation for low-income areas of the city. Many government agencies and individual politicians and bureaucrats act, or claim to act, in behalf of the *colonias proletarias*.[51]

These conditions are clearly reflected in the highly erratic, idiosyncratic responses made by officials to petitions from low-income neighborhoods within the city. Official behavior toward such communities has fluctuated sharply from overt hostility to benevolent permissiveness with top-level changes in city government personnel. Stubborn refusals to urbanize a squatter settlement on technical grounds that topography or subsoil conditions would make the introduction of basic services prohibitively expensive have given way virtually overnight to a decision to provide all necessary services.

While affording certain opportunities to lower-class petitioners, the absence of formally established guidelines by which needs are evaluated and the availability of multiple access points within the government bureaucracy may create other kinds of obstacles, especially for low-income communities whose leaders have failed to develop connections with high-ranking officials. The particularism of bureaucratic behavior "makes it hard for the [petitioner] to get a firm commitment on the part of any agency. He may even find it hard to find out what agency policy is, because agency policy may not exist; it may be that various bureaucrats each have their own mutually conflicting policies. . . . In such a system, it is quite true that an agreement with one official one day does not commit the agency" to any given course of action.[52]

Conclusion: Anticipating Demands and System "Overload" Due to Migration

We have observed that most demands articulated by migrants to the city are concerned with very concrete problems, close to the individual's own life space. But what happens after the most immediate, parochial, and acutely felt needs of migrants for such things as land titles and basic urban services are satisfied? Should we expect some sort of "spill-over" of demand making into other issue areas of broader social and economic import, such as inflation, unemployment, or inequalities in the country's distribution of wealth?

One prominent student of the urban poor in Asian countries has hypothesized that as economic and social conditions in low-income neighborhoods improve, there will be a shift in the role of community improvement associations from "selfishly particularistic pressure groups to more general welfare-oriented lobbies." He notes, however, that this may require "a generation of educated, upwardly mobile slum children" whose concerns extend beyond the developmental problems of their neighborhoods.[53]

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[49] Fagan and Tuohy, *Politics and Privilege in a Mexican City*, pp. 28–29.

[50] See Robert C. Fried, "Mexico City," in William A. Robson and D. E. Regan, eds., *Great Cities of the World*, 2 vols., 3rd ed. (Beverly Hills, Calif.: Sage Publications, 1972), p. 680; Francine F. Rabinovitz, "Urban Development Decision-Making in the Mexican Federal District," in *Programs for Urban Development in Latin America* (Washington, D.C.: U.S. Agency for International Development, 1965); and Louis Harris, "Government for the People of Mexico City" (Ph.D., dissertation, University of California, Los Angeles, 1956), chap. 5.

[51] One investigator has noted that functionaries of the official party are aware that neighborhood leaders "know very well how to make use of this situation [to] 'get things done' " (Schers, *The Popular Sector of the Partido Revolucionario Institucional*, pp. 71–72). Other observers have also documented the advantages offered to petitioners from low-income neighborhoods in Lima, Peru, and Guatemala City, by ambiguities in the jurisdictions of various government officials and agencies. See Dietz, "The Office and the Poblador"; Douglas Uzzell, "Bureaus and the Urban Poor in Lima," paper presented at the Annual Meeting of the American Anthropological Association, New Orleans, November-December 1973; and Bryan R. Roberts, "The Interrelationships of City and Provinces in Peru and Guatemala," in *Anthropological Perspectives on Latin American Urbanization: Latin American Urban Research, Volume IV*, ed. Cornelius and Trueblood.

[52] Peattie, *The View from the Barrio*, p. 89.

The demand-making activity observed among migrants to Mexico City and other major Latin American metropolitan areas lends little support to such expectations. After several decades of large-scale migration to these cities, demand making by residents of communities populated mostly by low-income migrants or their offspring remains confined to highly parochial, small-scale needs.[54] Moreover, demand making tends to fall off sharply within these communities once security of land tenure has been assured and basic urban services have been introduced.[55] In many cases, community improvement organizations have been created solely for the purpose of negotiating for land titles or some other specific benefit. When this purpose was fulfilled, the organizations lost their *raison d'être* and quickly disintegrated. Examples of community organizations which originally had a narrowly-defined function and subsequently evolved into more general-purpose lobbying groups are extremely rare.[56]

In assessing the potential impact of cityward migration on demand making aimed at local and national governments, one must also take into account the limited *scope* of the demand-making activity in which migrants are likely to engage. Attempts to aggregate demands across communities in which migrants take up residence are almost nonexistent, as are attempts by such communities to influence the content or scale of government policies and programs. For example, community-based organizations often negotiate for government recognition of the land tenure rights of their constituents, but almost never seek to alter government policies regarding land use or the legalization of squatter settlements in general.[57] Thus most demand making by low-income migrants to the city represents an attempt to influence the government decision-making process on the *output* (policy implementation) side.[58]

These observations are quite consistent with those of students of the urban poor in the United States, who often stress the difficulties of organizing the poor around larger policy issues.[59] Yet there are a few Latin American cases of low-income urban groups which represent important exceptions to this general pattern, to the extent that they have addressed themselves from time to time to concerns transcending their immediate, parochial needs for security of tenure and basic services. These include a substantial number of squatter settlements ("*campamentos*") in Chilean cities organized in the early 1970s by members of the extreme-left MIR (Movement of the Revolutionary Left), MAPU (a party originating from a 1969 split in the Christian Democratic Party), and some elements of the Socialist and Communist parties, as well as some urban neighborhoods in Cuba in which residents were organized into Committees for the Defense of the Revolution.[60] While the Cuban committees have often served primarily as instruments of political control or "popular mobilization" in support of the regime, the squatter *campamento* organizations in Chile were ostensibly aimed at stimulating demand making based on perceptions of class interests and class solidarity. They were viewed by their organizers as "instruments of political education" which would stimulate demands for government benefits going far beyond land titles and community services.

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[53] Laquian, "Slums and Squatters in South and Southeast Asia," pp. 199–200.

[54] Data from the Mexico City study also indicate that the adult sons of migrants to the city are less likely than their parents to engage in demand making and other forms of political participation. See Wayne A. Cornelius, "The Cityward Movement: Some Political Implications," in *Changing Latin America: New Interpretations of Its Politics and Society*, ed. Douglas A. Chalmers (New York: Academy of Political Science, Columbia University, 1972), pp. 38–39.

[55] See, for example, Goldrich, "Political Organization," pp. 186–187.

[56] Cf. Marc H. Ross, "Community Formation in an Urban Squatter Settlement," *Comparative Political Studies*, 6, No. 3 (October, 1973), 312–313.

[57] This pattern is often reinforced by private and public development agencies operating in low-income communities. Such agencies encourage a perception of the community as an entity isolated from the rest of the city in which it is located, with problems and solutions particular to that community. Commenting on the activities of "community development" and social service workers in squatter settlements of Lima, Peru, one investigator notes that by relating the settlements to a larger social reality, such workers might enable the residents to understand that the problems experienced by their community (e.g., insecurity of land tenure) are caused by basic problems at the city or national level (e.g., inequities in the land tenure system). Instead, they tend to define the problems confronting a settlement in highly parochial terms, thus "insuring . . . that the residents' organization will be limited to demand making focused only on immediate [community-specific] interests" (Rodríguez, "De invasores a invadidos," pp. 113, 130–131).

[58] Although we lack specific data on this point, it could be argued that their behavior is conditioned importantly by recognition of the low receptivity of Mexican political leaders to influence attempts on the "input" side of the decision-making process. As Robert Scott has recently observed, "Mexico's present political system is based on a petitioning pattern and not a bargaining process. Most citizens still see government (or more precisely, some individual, particularly the President) as dispensing favors to a petitioner on a personal and paternalistic basis rather than as a mechanism that implements policy decisions reached by a ritualized legal process of give and take among competing interests. So do most party and government functionaries." See Robert E. Scott, "Politics in Mexico," in *Comparative Politics Today: A World View*, ed. Gabriel A. Almond (Boston: Little, Brown, 1974).

[59] See, for example, Arthur Hillman and Frank Seever, "Elements of Community Organization," in Fred M. Cox et al., eds., *Strategies of Community Organization* (Itasca, Ill.: Peacock, 1970), pp. 284–285; Ralph M. Kramer, *Participation of the Poor* (Englewood Cliffs, N.J.: Prentice-Hall, 1969), pp. 235–237; and John H. Mollenkopf, "On the Causes and Consequences of Neighborhood Political Mobilization," paper presented at the Annual Meeting of the American Political Science Association, New Orleans, La., September, 1973. The present study does indicate, however, that low-income migrants in Mexico City possess a greater capability for organizing themselves for demand making with regard to *community-specific* needs and problems than has often been observed among the urban poor in the United States. For example, contrast the preceding discussion of demand-making skills and experience among residents of the communities included in this study with the following description, drawn from a study of neighborhood self-improvement associations in U.S. cities during the 1960s: "They had not developed clear goals. Interviews with club members, as well as those with the community organizers, indicated that *most of the club members had no con-*

During the administration of President Salvador Allende, delegations of residents from the *campamento* settlements in Santiago and other Chilean cities carried out numerous *tomas* (temporary seizures or occupations) of government buildings (including ministries), streets, and other public facilities to force officials to receive and act upon demands for assistance.[61] While many—perhaps a majority—of these instances of direct action were motivated by the needs of the settlements for water and sewerage systems, schools, health care facilities, and other local improvements, other *tomas* were staged to protest high prices, shortages of food and other commodities, and unemployment. Some *campamentos* in Santiago also organized committees to seek jobs for unemployed residents through conventional contacting of politicians and government bureaucrats. In sum, the organizers of these communities drew the attention of the residents to a variety of large-scale problems which were manifested concretely at the community level, while encouraging them to view these problems as national in scope, origin, and import. Perhaps even more importantly, the Allende regime took a generally permissive stance toward demand making by the poor, regardless of the subject matter of the demands or the tactics employed in articulating them. Occasionally local authorities retaliated against squatters involved in a *toma* with tear gas and arrests, but in general this type of demand-making activity was tolerated under the Unidad Popular regime, and the resulting negotiations between squatters and government officials often produced prompt and acceptable solutions to the problems at issue, or at least their local manifestations.

The case of Chile under Allende suggests that if migrants and other elements of the urban poor in Latin American cities are to be involved substantially in any kind of demand-making activity

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ception of the type of activities necessary to put pressure upon governmental or commercial agencies, to say nothing of knowing how to organize and implement such activities. . . . They apparently lack knowledge of how to deal effectively with the sources of dissatisfaction. Even very small and limited goals are rarely accomplished. . . . Drop-outs are frequent. . . . [The attempt] to develop local neighborhood self-improvement associations through the efforts of an indigenous community organizer . . . was a failure." See Walter Gove and Herbert Costner, "Organizing the Poor: An Evaluation of a Strategy," *Social Science Quarterly*, 50 (December, 1969), 643–656.

[60] On the Chilean *campamentos*, see James F. Petras, "Chile: Nationalization, Socio-Economic Change, and Popular Participation," in *Latin America: From Dependence to Revolution*, ed. J. F. Petras (New York: Wiley, 1973), pp. 58–60; Vanderschueren, "Political Significance of Neighborhood Committees," pp. 281–282; Joaquin Duque and Ernesto Pastrana, "La movilización reivindicativa de los sectores populares en Chile, 1964–1972," *Revista Uruguaya de Ciencias Sociales*, 1, No. 2 (August-October, 1972); Equipo de Estudios Poblacionales del CIDU, "Pobladores y administración de justicia," *Revista Latinoamericana de Estudios Urbano Regionales*, 3, No. 5 (July, 1972), 135–148; Equipo de Estudios Poblacionales del CIDU, "Reivindicación urbana y lucha política: Los campamentos de pobladores en Santiago de Chile," *Revista Latinoamericana de Estudios Urbano Regionales*, 3, No. 6 (November, 1972), 55–81; Manuel Castells, Luis Alvarado, Santiago Quevedo, Eder Sader, Jorge Fiori, and Ignacio Santa María, *Revista Latinoamericana de Estudios Urbano Regionales*, 3, No. 7 (April, 1973), 9–112 (special issue on low-income urban settlements in Chile). The following discussion of demand making by *campamento* residents is based primarily on these sources and supplemental information provided by Richard R. Fagen. For a detailed analysis of demand making by squatter settlement residents in Santiago during the Frei administration (1965–1970), which stresses the nearly total preoccupation of the demand makers with land tenure problems, see Peter S. Cleaves, "Bureaucratic Politics and Administration in Chile" (Ph.D. dissertation, University of California, Berkeley, 1972), pp. 548–606. On political organization of low-income neighborhoods in Havana, Cuba, see Douglas Butterworth, "Grass Roots Political Organizations in Cuba: The Case of the Committees for the Defense of the Revolution," in Cornelius and Trueblood, eds., *Latin American Urban Research, Volume IV*, chap. 8.

[61] In Chile the word *"toma"* is also used to refer to illegal seizures of land for permanent occupation by urban squatters. These *tomas* do not represent political demand making as defined in this paper, although they may *lead* to such behavior, with the object of securing land titles, basic urban services, and other improvements for the invaded land.

beyond that related to immediate, community-specific problems, it will come only as a result of intensive, sustained mobilization by leaders of political groups external to the low-income communities themselves. Moreover, this mobilization must occur in the context of a national political environment which is supportive of aggressive demand making by low-income groups, in a wide range of issue areas. Demands concerning non-parochial needs are likely to be made by residents of low-income communities only when they feel that the risk of violent repression by the authorities is minimal and that the likelihood of getting some redress of grievances is quite high. The amount of time and energy involved in petitioning public officials is too great to be squandered upon matters which are not regarded by the government as legitimate objects of demand making by the poor. In the absence of both external mobilization efforts and perceptions of governmental receptivity to non-parochial, "class-based" or cross-community demand making, the potential for such activity among the migrant poor will remain very low.[62]

The evidence presented above indicates that low-income migrants to the city should not be viewed as a "non-demanding" sector of the population, even though the demand making in which they engage is considerably less frequent than has often been assumed. However, the demands which they make upon the political system remain highly parochial and limited in scope. They are *the kinds of demands which can be satisfied most easily by incumbent authorities* without fundamental changes in government priorities or patterns of resource allocation. A political system in which most participation by the urban poor is confined to contacting officials about community-related needs is one which tends toward preservation of the basic sociopolitical order:

> Pressures which might otherwise take collective form, and be directed to earlier stages of policy formation or to the composition of the government itself, are diverted into discrete, separable, and small-scale demands which can be met in full or in part or rejected one by one. . . . Even where the incidence of neighborhood organization is high, . . . the sporadic nature of small-scale independent collective action means that only a fraction of all associations are likely to press for assistance at one time.[63]

Rapid urbanization has undoubtedly increased the number of demands made upon governments in Latin American and other Third World countries. But apocalyptic visions of mushrooming popular demands threatening to overwhelm local or national political systems as urbanization proceeds are clearly premature, if not altogether unfounded. Most of the demands on Latin American governments stemming from large-scale redistribution of population from rural to urban areas are, regrettably, those which are least likely to threaten an inequitable distribution of power and privilege in a society.

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[62] The obstacles to demand making by low-income migrants based on perceived class interests are legion, but perhaps the most important derive from the extreme occupational heterogeneity of the migrant population and their concentration within the tertiary sector of the urban economy. For discussions of these and other factors inhibiting class-based demand making by the poor in contemporary Latin American cities, see Daniel Goldrich, "Toward the Comparative Study of Politicization in Latin America," in *Contemporary Cultures and Societies of Latin America,* ed. Dwight B. Heath and Richard N. Adams (New York: Random House, 1965), pp. 368–371; Alaine Touraine and Daniel Pécaut, "Working-Class Consciousness and Economic Development in Latin America," *Studies in Comparative International Development,* 3, No. 4 (1967–68), 71–84; Pablo González Casanova, "Dynamics of the Class Structure," in *Comparative Perspectives on Stratification,* ed. Joseph A. Kahl (Boston: Little, Brown, 1968), pp. 76–80; James F. Petras, *Politics and Social Structure in Latin America* (New York: Monthly Review Press, 1970), pp. 13–23; Roger Hamburg, "Urbanization, Industrialization, and Modernization in Latin America: Soviet Views," *Studies in Comparative Communism,* 5 (Spring, 1972), 18–19; Huntington and Nelson, "Socio-Economic Change and Political Participation," pp. 5-21—5-29; and Richard Sandbrook, "The Working Class in the Future of the Third World," *World Politics,* 25 (July, 1973), pp. 469–478.

[63] Huntington and Nelson, "Socio-Economic Change and Political Participation," p. 5—51. Bryan Roberts, on the basis of field work in Guatemala City and Lima, Peru, concludes that even though residents of low-income neighborhoods in these cities are often able to take advantage of ambiguities in the urban power structure to obtain needed services, these manipulative strategies "have little lasting impact on the existing structure of power or on the distribution of resources. . . . The major impact of these local-level political activities . . . is that of creating a state of political uncertainty for those who control political and economic resources." See Roberts, "The Interrelationships of City and Provinces in Peru and Guatemala," and *idem, Organizing Strangers: Poor Families in Guatemala City* (Austin: University of Texas Press, 1973), pp. 307–337.

Clientelist Politics in the Philippines: Integration or Instability?*

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Introduction

The central question about political instability is not so much whether it occurs, but how it occurs. While the causes of political instability have been widely discussed,[1] it is less clear how these causes interact in particular contexts. Nor is it always clear what forms such instability will take. In this paper we examine the causes and consequences of political change in the Philippines, and suggest that certain nonviolent forms of competition may reflect conditions conducive to political instability.

Changes in nonviolent political competition (e.g., more competitive elections) are seldom viewed as an erosion of legitimacy in cases such as the Philippines, where elections were regularly conducted within a stable party system.[2] Yet whenever political competition reflects intra-elite rivalries within a clientelist structure of political mobilization,[3] it is crucial to determine whether or not changes in the pattern of competition are indicative of changes in the integrative capacity of clientelist structures.

Electoral institutions, when imposed on a feudal social structure like that found in the Philippines, relied on pre-existing patron-client bonds and incorporated them into a party organization. Under such conditions the legitimacy of the government derives in large part from the legitimacy accorded patrons by clients. Whether the disruption of traditional relationships reflects a decline in the legitimacy of government depends in large part on how rapidly and effectively alternative structures are provided for channeling sufficient resources to clients.

A deterioration in conditions supporting clients' claims for economic and social security is usefully analyzed from a class perspective in societies such as the Philippines. In the Philippines, the emergence of a cohesive landed class set the basis for further accumulation by such a class in industry and commerce.[4] To accumulate surplus more effectively and shift capital into these increasingly profitable activities, national elites desire to structure exchange relationships in specialized contractual ways.[5] In so doing national elites become increasingly freed from the informal distributive pressures common in more diffuse forms of patron-client relationships.

The free enterprise ideology absorbed by the Filipino elite through the American colonial experience assigns a prominent role in economic development to the private sector, both local and foreign. When a free enterprise system is imposed on a feudal social structure and bonds between patron and clients weaken, inequality is likely to increase since the restraints on the increased expropriation of surplus by patrons are lessened. Because the Philippines has a labor surplus economy, allowing relatively free play of supply and demand keeps wages low and allows capital to

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* This is a revised version of a paper presented to the Western Conference for Asian Studies in Salt Lake City, November 10, 1972. Field work for the project of which this paper is a part was supported by a Fulbright-Hays and a London-Cornell Project Fellowship. We would like to thank the Local Government Center of the University of the Philippines through Raul de Guzman for use of their facilities; the Cornell Southeast Asia Program, the Cornell Government Department through Doug Van Houweling, and the Cornell Department of Sociology for subsequent research support; and anonymous referees of the *American Political Science Review* for helpful comments.

[1] See, for example, Samuel P. Huntington, *Political Order in Changing Societies* (New Haven: Yale University Press, 1968), pp. 32–92, and Ted Robert Gurr, *Why Men Rebel* (Princeton: Princeton University Press, 1970).

[2] Increased support for extremist parties is often acknowledged as a sign of discontent and potential erosion of legitimacy. Other than the Democratic Alliance (a coalition of peasant groups in central Luzon, urban middle-class elements and some Communists, whose representatives were barred from holding office after the 1946 election), there have been no extremist parties participating in Philippine elections.

[3] There is a rapidly growing body of literature dealing with patron-client relationships. The literature most applicable to our analysis includes: Carl Landé, "Networks and Groups in Southeast Asia," *American Political Science Review*, 67 (March, 1973), 103–127; James C. Scott, "Patron-Client Politics and Political Change in Southeast Asia," *American Political Science Review*, 66 (March, 1972), 91–113; and by the same author, "Corruption, Machine Politics, and Political Change," *American Political Science Review*, 63 (December, 1969), 1142–1158.

[4] Harry Benda, "Political Elites in Colonial Southeast Asia: An Historical analysis," *Comparative Studies in Society and History*, 7 (April, 1965), 233–251. See also Robert Reed, "Hispanic Urbanization in the Philippines," *Journal of East Asiatic Studies* (March, 1967), 1–213.

[5] See Mary Hollnsteiner, "Reciprocity in the Lowland Philippines," in *Four Readings on Philippine Values*, ed. Frank Lynch and Alfonso De Guzman (Quezon City: Ateneo de Manila Press, 1970), p. 85.

accumulate. If such capital accumulation is part of a dynamic, self-sustaining pattern of economic growth, market forces will ultimately raise the cost of labor. If on the other hand such capital accumulation is not part of a dynamic process of growth, deteriorating "terms of trade" between patron and client are likely to persist and to continue favoring the patron.

Compensatory mechanisms can alleviate the deterioration in relative position of clients which frequently occurs during early industrialization. As the economy becomes differentiated and national political and economic power is centralized, integration of local clientelist systems into the national political system occurs through a pyramiding of clientelist structures. Such a vertical integration makes possible the provision of national resources to clients. In electoral clientelist systems such as existed in the Philippines, an extension of the exchange network provided the conduit through which national patronage resources could be bartered for electoral support.[6]

Whether or not the change in patron-client structures to a more specialized political machine proceeds smoothly depends in large part on the capacity of the government to extract and distribute resources. If sufficient resources are allowed to move from the center to local areas, political machines should prove capable of integrating and mobilizing large groups of people.[7] We shall argue that such a pattern of resource capability and distribution does not exist in the Philippines. In so doing we shall examine the nature of capital concentration, economic change, and the capacity of the public sector to extract and distribute resources. To further explore the conditions leading to the declining strength of political machines, we offer hypotheses which focus on: (1) the relationship of capital concentration to economic diversification and industrialization; (2) the relationship of economic structure and sociodemographic structure; and (3) the relationship of economic and socio-demographic structure to the strength of political machines.

Social Structure and Capital Concentration

The historic role of the bourgeoisie in Western Europe as a capitalist class antagonistic to old feudal elites has no counterpart in the Philippines. While the process of industrialization in Western Europe and the United States marked the triumph of industrial elites over feudal agrarian ones,[8] no such conflicts among elites appear forthcoming in the Philippines. Philippine industrial entrepreneurs had not developed into a distinct group, but rather tend to come from families of the older agrarian and commercial elites who increasingly diversify wealth into a variety of advanced as well as traditional enterprises.[9] While there is occasional mobility into the national elite by persons of more modest means, the basic trend in the Philippine elite has been toward consolidation and concentration. That is, a dispersed tribal nobility evolved into a largely Spanish and Chinese Mestizo landed and commercial elite, joined after the turn of the century (once political power became available and educational opportunities expanded) by some indigenous Malay elements. A basic homogeneity of interest among this emergent national elite has been facilitated by increased intermarriage among the different ethnic strands (e.g., Chinese-Malay and Spanish-Malay).[10]

By stimulating a particular pattern of economic diversification through easy credit and import controls, monetary and fiscal policy in recent years has further increased the economic power of the national elite. It was feared that the devaluation and abandonment of controls in 1962 would threaten the existence of Filipino enterprises long nurtured by protection and subsidization, yet most Filipino enterprises managed to survive.[11] Diversification of familial economic power decreases susceptibility to fluctuations in world prices, potential loss of privileges in the U.S. market, and local policy changes such as devaluation, which hurt some sectors more than others. We therefore hypothesize that:

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[6] See Carl H. Landé, *Leaders, Factions, and Parties: The Structure of Philippine Politics*, Yale University Southeast Asia Studies Monograph Series No. 6 (New Haven: Yale University, Southeast Asia Studies, 1966).

[7] See René Lemarchand, "Political Clientelism and Ethnicity in Tropical Africa: Competing Solidarities in Nation Building," *American Political Science Review*, 66 (March, 1972), 86.

[8] Barrington Moore, Jr., *Social Origins of Dictatorship and Democracy* (Boston: Beacon, 1966), chapter 3.

[9] John Carroll's data on the socioeconomic origins of Filipino entrepreneurs leads him to conclude that "the old elite did not precisely 'withdraw . . .' it remains, in proportion to its size, the most fruitful single source of manufacturing entrepreneurs." See John J. Carroll, *The Filipino Manufacturing Entrepreneur* (Ithaca: Cornell University Press, 1965), p. 100. Laurence Davis Stifle, *The Textile Industry —A Case Study of Industrial Development in the Philippines, Data Paper No. 49* (Ithaca: Cornell Southeast Asia Program, 1963), p. 93, notes that for textile entrepreneurs ". . . entrepreneurship has been a means of preserving or consolidating a realized upper stratum position." Dante C. Simbulan, *A Study of the Socio-Economic Elite in Philippine Politics and Government 1946–1963*, (Ph.D. dissertation, Australia National University, 1965) notes the fused nature of the Philippine social, political and economic elite.

[10] See Simbulan, pp. 107–110.

[11] See Frank Golay, "The Philippines," in *Underdevelopment and Economic Nationalism in Southeast Asia*, ed. Frank H. Golay, Ralph Anspach, M. Ruth Pfanner, and Eliezer B. Ayal (Ithaca: Cornell University Press, 1969), p. 103.

H1: Capital concentration increases with economic diversification and industrialization.

Economic Stagnation and Government Capacity

It is important to point out not only the existence but also the nature of capital concentration. Filipino entrepreneurs continue to invest heavily in the profitable primary sector and also in urban real estate and services—investments which do not generate extensive "backward" linkages and employment.[12] While reliance on the protected American sugar market is no longer as crucial a source of elite surplus as in previous years, it continues to be highly lucrative. Additionally, although certain government policies encourage economic diversification, the most extensive foreign and local credit continues to be available to dollar- or yen-earning export industries such as lumber and mining. The removal of subsidies for capital equipment in the early 1960s raised the cost of capital, yet did not significantly stimulate the growth of an indigenous capital-producing sector. Consequently, in recent years the growth in manufacturing has slowed as investors turn to more profitable extractive activities such as logging. Hicks and McNicoll have found that a significant proportion of recent economic growth derives from rapid expansion of activity in the primary sector (e.g., mining and lumber), which has limited long-term growth potential as natural resources are increasingly depleted.[13]

Since a dynamic pattern of economic growth has not occurred in the Philippines, considerable government intervention is necessary to attenuate an increasing loss of security for broad groups of people.[14] Yet it is questionable whether the Philippine government is capable of such intervention. The public sector's share in the total supply of final goods and services has remained virtually constant since 1950 and is low among Southeast Asian countries.[15] One reason is that the government is heavily dependent on indirect excise taxes which are relatively regressive and inelastic.[16] Patronage resources have become increasingly scarce in relation to the obligations incurred. In the last twenty years, for example, the proportion of public works appropriations which were actually released to agencies has fallen approximately 15 per cent.[17] The negative correlation between the rate of population growth from 1960 to 1970 in chartered cities and the rate of growth in total government expenditures per capita from 1961 to 1969 ($r = -.36$) suggest that local government capabilities are not expanding rapidly enough. Time lag correlations, while producing slightly more optimistic results, are not significant. The rate of population growth from 1948 to 1960 correlates $+.15$ with the rate of growth in total government expenditures per capita from 1961 to 1969.[18]

The inevitable recourse to deficit financing characteristic of Philippine election years raises further questions about the capability of the government. In each of the six election years between 1957 and 1968, the level of net government receipts declined.[19] This pattern of deficit financing has both aggravated inflationary pressures and diminished net international reserves. Between 1956 and 1962 net reserves fluctuated but were always positive. By contrast, net reserves from 1963 to 1968 were negative for three of the six years under consideration.[20]

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[12] The concept "backward linkages" is explicated in Albert O. Hirschman, *The Strategy of Economic Development* (New Haven: Yale University Press, 1958).

[13] George L. Hicks and Geoffrey McNicoll, *Trade and Growth in the Philippines* (Ithaca: Cornell University Press, 1971), p. 105.

[14] The real wage of wage earners in selected non-agricultural industries in the Philippines dropped 1.1 per cent from 1957 to 1969. Calculated from Central Bank of the Philippines, *The Statistical Bulletin*, 21 (December, 1969), Tables 24 and 127. Between 1956 and 1971 the income concentration ratio in the Philippines increased from .452 to .455. Calculated from Bureau of Census and Statistics, *Survey of Households, Family Income and Expenditures: 1957* and Bureau of Census and Statistics, *Survey of Households, Family Income and Expenditures: 1971*. The concentration ratio is obtained by dividing the area bounded by the line of equal distribution and the Lorenz curve of income distribution by the total area under the line of equal distribution. The ratio therefore varies from 0 to 1, and the higher the value the greater will be the inequality in the distribution of income.

[15] See United Nations, Economic Commission for Asia and The Far East, *Economic Survey of Asia and the Far East 1961* (Bangkok: United Nations, 1962), p. 77 and *Economic Survey of Asia and The Far East 1966*, p. 24.

[16] Jesus P. Estanislao, *Economics of a Nation* (Manila: Center for Research and Communication, 1970), p. 122 and 151.

[17] Calculated from the Philippine Department of Public Works and Communications, *Annual Report, 1969–1970*, p. 38. Included in the early years (1954–1958) are Public Works Acts Nos. 1200, 1411, 1613 and 1900. Included in the later years (1959–1965) are Public Works Act Nos. 2093, 2301, 2701 and 3101. To include more recent acts would artificially accentuate these differences since releases for such acts are still occurring.

[18] The rate of population growth between 1960 and 1970, and between 1948 and 1960, is computed from the Republic of the Philippines Bureau of Census and Statistics, *Census of the Philippines, Population and Housing* for 1948, 1960 and 1970. The rate of growth in total government expenditures per capita between 1961 and 1969 is computed from statements on expenditures at the General Auditing Office of the Philippines.

[19] Harvey A. Averch, Frank Denton, and John Koehler, *The Matrix of Policy in the Philippines* (Princeton: Princeton University Press, 1971), p. 101.

Economic Structure and Sociodemographic Structure

The relationships of economic complexity and limited forms of industrialization to increased social mobilization and urbanization in the Philippines differ in important respects from the relationships among such variables in the West. In the Western model urbanization is strongly related to industrialization—although it is recognized that in some instances manufacturing may follow as well as "pull" workers, and urban areas may develop around commerce and service as well as manufacturing.[21] Certain forms of urbanization in non-Western societies are more likely to occur separated from industrialization.[22] That is, major sources of elite surplus are derived from investment in primary industries in the more rural areas, while cities serve as major commercial and administrative centers (and also the primary places of residence for the national elite). The rural brokers for such an increasingly absentee national elite (e.g., *hacienda* managers) enable the elite to retain their control over rural surplus and facilitate a net transfer of resources from rural into urban areas through a variety of mechanisms.[23] Such a transfer in turn encourages further urban migration in excess of the cities' real industrial base—for example, an oversized service sector develops which caters largely to the indigenous elite and middle classes, and foreign businessmen.

Increased population density and the emergence of groups facilitating the economic activities of the national elite in more urbanized areas of the Philippines (e.g., professionals and middle level bureaucrats) create a domestic market which, under particular conditions such as import controls or tariffs, encourages foreigners and the indigenous elite to invest for this market. Still, it is clear that strong incentives remain to invest in the rural primary sector.[24]

The emergence of many types of petty establishments in the Philippines provides an indication of the vast supply of under- and unemployed laborers who must resort to marginal employment for their livelihood. Such forms of economic activity —coexisting side by side with large commercial establishments (and to a lesser extent, large manufacturing concerns)—constitute part of a dual socioeconomic system in urban areas involving pronounced class and cultural cleavages between an ever proliferating lower class population and the elite, middle-level professionals and bureaucrats living in the suburbs. Another manifestation of the dual economy occurs in the more rural areas, where large agro-business corporations such as sugar centrals are accompanied by little other industrial or large commercial activity. While such agro-businesses tend to be fairly traditional in terms of the methods of social control they employ and in not being very disruptive of traditional social structure, they do tend to be *in* but not *of* the rural areas in which they are found. That is, they are geared to serving the national elite in the primate city and a largely foreign market.

Even though urban affluence may be functionally related to the extraction of rural surplus, the benefits of this surplus are less apparent in the rural areas. That is, there is a net transfer of resources from rural to urban areas. It is not clear, however, whether the existence of an urban market (e.g., arising from population density or high levels of social mobilization) is related in linear fashion to investment in manufacturing for such a market. The process by which cities with a largely commercial and administrative base acquire some degree of industrialization is likely to result from certain economies of scale which concentrate manufacturing for the internal market in large cities. While detached in spatial terms from the extraction of rural surplus, we anticipate that urban wealth is related to measures of service-commercial development which cater largely to the upper and middle classes (e.g., banks, supermarkets, nightclubs).

Sociodemographic Structure and Political Structure

Changes in traditional Philippine status and economic arrangements have increased the separation between private and public wealth. As private wealth continues to be concentrated within an economy unable to absorb the growing labor surplus, public income is increasingly scarce relative to what is needed to provide economic security for broad groups of people. The impact of such scarcity upon the strength and structure of patron-client ties faced with increased urbanization and social mobilization can be observed through a number of processes, as follows: (a) differentiation among the local elite increases the probability that any local area will have more potential centers for patron-client alliances; (b) growing proportions of economically insecure clients and also middle class groups decrease the coverage of patrons unable to procure sufficient relevant public resources; (c) increased specialization in the structure of patron-client exchange decreases the reliability of political mobilization and increases the demand for public patronage resources suitable for such exchange (e.g., money).

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[20] *Ibid.*, p. 97.

[21] See Gideon Sjoberg, "Cities in Developing and in Industrial Societies: A Cross Cultural Analysis" and Nathan Keyfitz, "Political-Economic Aspects of Urbanization in South and Southeast Asia," in *The Study of Urbanization*, ed. Philip Moris Hauser and Leo F. Schnore (New York: Wiley, 1965). See also Simon Kuznets, *Economic Growth of Nations* (Cambridge: Harvard University Press, 1971), pp. 80 ff.

[22] Sjoberg. See also T. G. McGee, *The Southeast Asian City* (New York: Praeger, 1967).

[23] Absente landlords in the Philippines invariably bank in major urban areas and often pay the majority of their taxes there. Sugar centrals and other large corporations in the more rural cities have offices in the dominant cities and often pay their corporate income taxes through these main offices. Since chartered cities which collect greater amounts of national revenues than they did in the past are apportioned a certain amount of the excess, the location in which corporations and individuals pay their taxes is important.

[24] When the Philippines did not receive its anticipated share of the U.S. sugar quota in 1970, six new sugar centrals were being constructed and eight more had been built since 1965. Information compiled from *Atlantic Gulf and Pacific Progress, Second Quarter Report*, Vol. 11, No. 2 (Manila, 1970), p. 5.

Proliferation of Candidates

A Philippine middle class which performs important administrative and political functions for the national elite emerged as economic differentiation and centralization of government increased. At the local political level such individuals are brokers for an elite increasingly distant from the masses, yet seeking to extend national administrative and economic control. These brokers occasionally expand their influence beyond the local level, yet their bases of power are limited by the increasing concentration of private capital and the growing importance of national public resources.[25] To maximize control over local brokers, it is in the interest of the national elite to make such brokers as dependent upon them as possible. Since the capitalization required for business enterprises has risen, local elites with limited capital are less able to concentrate wealth.[26] The growing penetration of national bureaucratic institutions into local areas heightens the dependence of the local elite upon office-based resources. Results of an Ateneo de Manila survey are indicative of such differential control over the means of production by the national as compared with the local elite—that is, higher proportions of the national elite are reputed to have primary influence in the areas of business and economics than do local elites (41 per cent of the national elite as contrasted with 18 per cent of the local elite).[27]

While the particular pattern of Philippine economic development gave rise to a largely service and commercial middle class, the mobility opportunities for such a class have been much smaller than its aspirations. Such a middle class has been heavily exposed to Western consumerism and is vulnerable to inflation, and thus is particularly eager to increase its income when possible (e.g., through extensive moonlighting). Under these circumstances, the growing absolute size of the middle class has increased the number of individuals interested in holding a potentially lucrative local office. Additionally, the existence of a middle class has provided a social base for the emergence of occasional "reform" candidates more sensitive to its needs.

The specialization of clientelist structures that accompanies economic and social differentiation, together with the weakness of any single structure, provides particularly strong incentives for patrons to expand the range of clientelist groupings under their domination. This is likely since the appropriate boundaries of influence among institutions are not clearly defined, yet sizable rewards may be derived from any one of them. Labor union bosses may find themselves competing with political bosses, or vice versa.[28]

Increased ethnic diversity further expands the social bases necessary to support multiple local candidacies. This should be particularly true in ethnically plural areas of the Philippines (such as Cotabato City) which are not developed enough to sustain an urban culture that might weaken the strength of ethnicity. While ethnicity in the Philippines is not generally reinforced by religious cleavages (the primary exception being the Mindanao and Sulu Moslem groups), it does remain an important basis for political participation.

Since the number of political positions in any given Philippine city remains fairly constant over time, an expanded number of office seekers will simply increase the number of persons competing for a particular office. Such an inflationary cycle might be offset by a number of different conditions, but none of these seems evident in the Philippines presently. Rapid economic growth of the sort that occurred in the United States at the turn of the century, for example, could result in sufficient mobility through other channels so that political office would be less sought after as an avenue to success. Second, the emergence of strong political machines might restrict the number of local political aspirants in any given area. To understand why such strong machines have not developed in more urbanized areas of the Philippines, we must note the important differences between the Philippine machine and its turn-of-the-century American counterpart.

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[25] In her study of the municipality of Hulo, Hollnsteiner notes that the increasing incorporation of Hulo into the national-metropolitan arena has led to a proliferation of and differentiation among the local elite. Whereas previously the traditional land- and fish pond-owning elite had been strong enough to carry completely the dominant political faction and its auxiliaries, at present "non-elite professionals were trying to gain mastery over the faction but though fairly rich, they did not have the seemingly inexhaustible funds which their elite predecessors had had." Mary R. Hollnsteiner, *The Dynamics of Power in a Philippine Municipality* (Quezon City: Community Development Research Council, University of the Philippines, 1963), p. 51.

[26] From 1958 to 1961 the real paid-up mean capital investment per newly registered business was 214,900 pesos, and by 1961 to 1964 it had increased to 233,800 pesos. Calculated from Bureau of Census and Statistics, *Yearbook of Philippine Statistics, 1966* (Manila, 1966),Table 18, p. 117. The wholesale price index used as deflater appears on page 99 of the same source.

[27] Perla Makil, *IPC/PAASCU Study of Schools and Influentials* (Manila: Institute of Philippine Culture, 1971), Table A-3.

[28] For examples of this phenomenon in the Philippines, see Thomas Nowak and Kay Snyder, "Urbanization and Clientelist Systems in the Philippines," *Philippine Journal of Public Administration*, 14 (July, 1970), 267.

American machines evolved in response to some of the same conditions which render Philippine machines functional—the presence of a large, economically insecure population[29] within an electoral system in which votes could be traded for jobs and patronages. The rapid economic growth occurring in the United States at the turn of the century provided important resources for machine politicians. In addition to the exchange of corporate and finance capital by businessmen for local "advantages," the expansion of skilled and unskilled wage employment in the private sector offered the well-connected political boss the opportunity to procure employment in the private sector for his clients. Further patronage resources resulted from the rapid growth in local government services, particularly before the passage of the Civil Service Act.[30]

Resources for urban machines in the Philippines have not been as plentiful. A variety of factors contribute to this scarcity: wage employment opportunities are limited; systems of local taxation are inefficient; the national government preempts major sources of excise revenues; national patronage resources are insufficient. The limited patronage available may suffice for those poor first-generation migrants whose relative standard of living has increased, but is unlikely to satisfy later generations of the lower class whose expectations are rising[31] or to be particularly relevant for middle-class groups.

Since social mobility has not been extensive in the Philippines and since strong urban machines have failed to emerge, various processes of social change (population diversity, economic complexity, social differentiation, and ethnic diversity) should increase the numbers of individuals seeking access to scarce resources such as local political office. National political parties are too weak to limit the rise of local candidacies. Philippine local elections often have several candidates competing for the same office under the same party label.

> H2: Population density, economic complexity, social differentiation, and ethnic diversity increase local elite differentiation, which increases the number of candidates competing for local office.

Specialization in Clientelist Structures

In the Philippines a feudal elite with origins in colonial centers of commerce and estate agriculture has gradually transformed itself into a national elite as political and economic power became more centralized. With the diversification of its power the Philippine national elite appeared increasingly unwilling to play the part of the traditional patron, both in the home areas of such elites and in the frontier areas opened to various forms of investment. Such elites sought to put their relations with clients on a specialized, commercial footing which could produce both surplus and political support but would otherwise free the elites from traditional obligations which could hinder their ability to pursue an increasingly national set of interests. Hollnsteiner notes that those with extra-local interests or those who reside in urban areas find that entanglement in diffuse forms of creditor-debtor relationships (*utang-na-loob*) places severe constraints on their ability to accumulate surplus. Such individuals prefer more explicit contractual or quasi-contractual forms of exchange.[32]

As the economy became differentiated, specialized patron-client relationships have emerged in more functionally specific forms such as political machines and boss-dominated labor unions. Specialized patron-client clusters are likely to be unstable, with a small core and large unreliable periphery.[33] The exchange between patron and client becomes both more instrumental and intermittent. Factionalism is intensified by the greater ease with which clients can attach themselves to and break attachments with patrons. Since local political or union leadership provides local patrons with one attractive avenue of mobility, competition among elites for such positions has been intensive. This competition helped prevent the emergence of a more stable clientelist network by offering clients some choice between patrons. Increased lateral mobility by clients does not, however, represent any increase in their net resource position. To the contrary, the leverage which clients gained with competing patrons through the vote appears offset by the weakening of distributive pressures which were inherent in more diffuse forms of patron-client exchange.

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[29] The mean per cent of individuals with an elementary education or less in Philippine chartered cities in 1960 was 71 per cent. Calculated from the Bureau of Census and Statistics, *Census of the Philippines, 1960, Population and Housing*. In Chicago first generation immigrants comprised 52 per cent of the population by 1850. See Donald S. Bradley and Mayer N. Zald, "From Commercial Elite to Political Administrator: The Recruitment of the Mayors of Chicago," *The American Journal of Sociology*, 71 (September, 1965), 153–167.

[30] Between 1849 and 1868 municipal expenditures in Chicago increased from approximately $45,000 to more than $6,000,000. See Bradley and Zald, pp. 156–159.

[31] See the discussion by William Mangin, "Introduction," in *Peasants in Cities*, ed. William Mangin (Boston: Houghton Mifflin, 1970), pp. xxvii ff.

[32] Mary Hollnsteiner, "Reciprocity in the Lowland Philippines," p. 85.

[33] James Scott, "Patron-Client Politics and Political Change in Southeast Asia," pp. 91–113, does an extensive analysis of patron-client structures in which he suggests a number of dimensions along which such structures can be contrasted.

Specialization in patron-client structures means that there is an increase in the number and type of such structures in any given area. This further complicates the coalition-building process since these specialized structures are more likely to have autonomous institutional bases of power. Additionally, the growth of middle-class groups located within specialized clientelist structures marked the emergence of clients whose interests could not be met within the local political arena.

Increasing reliance on monetary forms of exchange by political machines facing a growing shortage of patronage resources decreases the reliability of support for any one faction. Since both the economic and psychic sanctions which patrons can apply to the urban lower classes are more limited than those available to a landlord with his tenants, clients have more incentive to maximize their resources by hedging on commitments—e.g., collecting money from or being courted by more than one faction.

The deteriorating resource position of clients in more urbanized areas is underscored by the fact that the electorate increasingly demands explicit and immediate resources for their votes. If the resources "lost" from decreased services provided by patrons were compensated through adequate wage labor or provision of economic security by the government, voters would be unlikely to have such a short-range mercenary attitude toward the worth of a vote. As economic insecurity increases, however, the vote comes more and more to represent a resource exchanged for ready money to the highest bidder. To summarize:

H3: The strength of political machines weakens as general sociodemographic processes such as social mobilization and urbanization result in specialized clientelist forms of exchange.

H4: The strength of political machines weakens as intra-elite competition intensifies (evident in the proliferation of local political candidates).

Electoral Participation

In the Philippines limited forms of political mobilization may occur more effectively in areas where traditional forms of clientelist bonds are more fully intact. Mobilization theory, in assuming that the power of the local elite which benefits from parochial community loyalties must be broken before regional and national elites can gain support, ignores the often strong incentives for local elites to deliver exactly such loyalties to higher levels in exchange for resources which help them maintain their position in the community.[34] The increasing access of local elites to a variety of external resources (e.g., financial credit and the coercive power of the state to back up domination of local resources) allows escape from the distributive pressures normally existing in closed community systems.[35] At the same time such access increases the brokerage functions of local elites, which mediate between demands of the national government for resources such as votes and those of local clients for services and security. As long as the clients' positions do not substantially deteriorate, the patron remains able to increase political participation either for himself or on behalf of regional and national allies given the strength of obligations owed him for past and present services rendered.

Greater electoral participation in less urbanized areas should result not only from the previously noted characteristics of such areas, but also from circumstances in more urbanized cities which militate against participation. Nie et al. suggest that while city residents are more likely to have the status and organizational involvement which leads to political participation, the facilitating effect of small-town life is lost with increased urbanization and social mobilization.[36] Local political participation appears particularly likely to deteriorate as government becomes more complex. Whether such a decline in participation will be reflected in voting is not clear, however, since voting appears as one of the most simple and effortless acts of political participation.

[34] This is both cogently argued and demonstrated by Steven Dodge Norris, *Political Behavior and Social Change: Causes of the Growth of the Indian Electorate in the Last Half Century* (Ph.D. dissertation, Cornell University, 1971). What mobilization theory does alert us to is that in systems such as the Philippines which have literacy requirements for voting (albeit lax), social mobilization increases the proportion of adults registered to vote. Our data show that the level of social mobilization correlates +.46 with the level of voter registration. The concept of social mobilization is described in Karl W. Deutsch, "Social Mobilization and Political Development," *American Political Science Review*, 55 (September, 1961), 498–499.

[35] Lenski, for example, argues that in the simplest societies, or those which are technologically most primitive, the goods and services available will be largely distributed on the basis of need. See Gerhard E. Lenski, *Power and Privilege* (New York: McGraw-Hill, 1966).

[36] Norman H. Nie, G. B. Powell, Jr., and Kenneth Prewitt, "Social Structure and Political Participation: Developmental Relationships, I," *American Political Science Review*, 63 (June, 1969), 366.

While decreased local political participation might be attributed to the nationalization of politics, reduced national participation requires other explanations—e.g., the lessened effectiveness of clientelist machines. Such a reduction in effectiveness does not necessarily imply a more underlying process of institutional deterioration, however. A changing class structure could alter the arenas through which individuals press demands. In the United States at the turn of the century, for example, declining participation signified the weakening of machines as middle-class reformers mounted political and legislative attacks against such machines.[37] To argue then that decreased participation measures a decline in the effectiveness of clientelist machines for *all* sectors of the population requires some demonstration that general processes such as social mobilization explain more variance in turnout or decline in turnout than does the size of the middle class.

The institutionalization of particular patterns of competition may also lead to declining turnout. Gordon suggests that in the United States the struggle by ethnic groups to gain access to the political system was characterized both by intense competition and high turnout. Once ethnic conflict is built into a partisan party framework, both political competition and turnout may decline.[38] Whether this might be the case in the Philippines depends on the extent to which factional conflict has been stabilized.

Institutionalization of clientelist forms of mobilization could prevent turnout from declining, however, if important welfare needs of lower-class groups were met. The integration of peasants into national clientelist parties in Venezuela and in Italy was prompted by the need of such parties to increase their electoral support among peasants. By providing peasants with agrarian reform and other types of patronage, the clientelist system in fact stabilized electoral participation.[39] What did diminish was the recourse of such groups to violent means of expressing demands. Improving the quality of the urban environment has higher costs, and thus the state or party would seem less able to stabilize clientelist participation in more urbanized areas.

Since the Philippine government lacks the resources to finance reform and patronage available in a more industrialized country such as Italy or an oil-rich developing country such as Venezuela, lack of political participation could be linked to the general resource scarcity of machines. As the resources available to local brokers decline relative to the needs of lower-class individuals, incentives decrease for both elites and masses to participate in electoral politics. Individuals who expect to be paid off or otherwise rewarded for voting have little incentive to vote unless local political brokers reach them with the actual payoff. If the content of the payoff is increasingly less able to provide the lower-class voter with a margin of economic security, the potential increases for a shift to other arenas through which demands are pressed (for example, the streets).

The general scarcity of resources should be particularly pronounced in areas which have a minimum amount of access to national patronage. The relative frequency and the ease with which local officials in the Philippines switch to the party of the president indicate both the weakness of parties in the Philippines and the importance of access to national patronage resources. For a variety of reasons (such as long-term personal feuds) there are some consistently oppositionist areas which would be likely to receive few patronage resources.

H5: The effectiveness of machines in mobilizing broad groups of people declines with greater social mobilization, ethnic diversity, urbanization and other factors increasing the scarcity of patronage resources.

Data and Measures

The degree of indigenous capital concentration among the economic elite is measured by a Gini Index. This index measures the degree of inequality in familial control over corporate assets in each chartered city.[40] Construction of the index involved a number of steps. Following compilation of a master list of all large corporations filing with the Philippines Securities and Exchange Commission, stockholders were included for each corporation until the pool of the largest corporate stockholders owned at least 70 per cent of the corporate stock in that corporation. Subsequently, stockholders were organized by family or by company (in cases where companies owned stock) and the percentage of stock out of total paid-up capital was computed for each family or company.[41] Since individual corporations vary in size and therefore in their importance to an area, family corporate control is standardized by multiplying the percentage of stockholdings owned by one family by the total assets of the corporation.[42] Following computation of total corporate assets in a given chartered city, each family's share in this total was computed. The Gini Index, used to measure the degree of indigenous capital concentration, is constructed as follows:

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[37] William Nisbet Chambers, "Party Development and the American Mainstream," in *The American Party Systems,* ed. William Nisbet Chambers and Walter Dean Burnham (New York: Oxford University Press, 1967), p. 14. See also Walter Dean Burnham, *Critical Elections and the Mainstream of American Politics* (New York: W. W. Norton, 1970), pp. 21 and 71–90. In Mexico, the access of the middle and upper classes to resources and policy makers through nonelectoral channels helps explain lower turnout among these sectors. See Jose Luis Reyna, *An Empirical Analysis of Political Mobilization: The Case of Mexico,* Latin American Studies Program Dissertation Series No. 26 (Ithaca: Cornell University Press, 1971), chapters 5 and 6.

[38] Daniel N. Gordon, "Immigrants and Municipal Voting Turnout: Implications for the Changing Ethnic Impact on Urban Politics," *American Sociological Review,* 35 (August, 1970), 665–681.

[39] John Duncan Powell, "Peasant Society and Clientelist Politics," *American Political Science Review,* 64 (June, 1970), 420.

[40] Our unit of analysis is the chartered city. Chartered cities are legal entities which may or may not be true cities, since they display considerable variance in density, industrialization and the level of social mobilization. Such cities either alone or in clusters, as in the Greater Manila complex, form labor market areas, which makes them a useful unit in analyzing relationships between socioeconomic and political structure.

$$\text{Gini Index} = 2\sum_{i=1}^{N}(X_i - Y_i)\Delta X_i$$

$$X = \frac{i}{N}$$

$$Y_i = \sum v_k/V$$

$$k \geqq i$$

N = Total number of families in a given chartered city

V = Total amount of corporate assets in a given city

v = Each family's share of city corporate assets

i = The i'th family in assets, from lowest to highest

The Gini Index sums for each family in the population the difference between its location on a Lorenz Curve and its expected location in the case of perfect equality in the ownership of corporate assets.[43]

Since patterns of industrialization and economic growth in countries such as the Philippines differ in important respects from Western models, we cannot assume that commonly used measures of industrialization are equivalent across systems. It is unlikely that variables such as the amount of value-added in manufacturing per capita and the mean size of manufacturing firms cluster together in economies where investment in giant, capital-intensive agro-business is such an important source of elite surplus. Generally, large agro-businesses such as sugar centrals and copra processing plants do not create as much value-added as do secondary basic industries like steel production.

Under the assumption that the meaning of specific indicators is best interpreted in the context of the particular system in which they are observed, we predict that indicators of various aspects of economic structure in the Philippines do not cluster together in the same manner as in the West.[44] Therefore measures of economic structure should distinguish, for example, between manufacturing investment for the largely internal, urban market and investment in the primary or rural sectors. They also should illustrate the commercial-service character of many Third World urban areas, as well as distinguish between petty forms of economic activity that absorb surplus urban labor and larger manufacturing firms which provide more stable employment.

Two sets of factor analyses were employed to extract the hypothesized dimensions.[45] Table 1 indicates the types of measures which tap agro-business forms of rural investment as against those measuring more urban-linked manufacturing investment for the internal market. High loadings on the factor labeled urban industrialization were noted for the following variables: percentage of total employment represented by mining and manufacturing; number of manufacturing production employees, number of manufacturing employees (as measured by two different sources); and per capita manufacturing fixed assets. Mining is included with manufacturing in the first measure for two reasons: (1) mines as well as agro-business forms of manufacturing are important sources of foreign exchange and investment in less urban areas; (2) since there are relatively few mines as compared with manufacturing firms, analyzing mines alone creates cumbersome distributions among the chartered cities.

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[41] The information on stockholders and their stockholdings was obtained from records of the Philippine Securities and Exchange Commission. Only 16 per cent of the corporations were controlled by other corporations rather than individual families ($N = 1511$).

[42] Since the total corporate assets of a city accounted for under the 70 per cent rule varied from 77 per cent to 92 per cent, each family's control over corporate assets is further standardized by dividing it by the total assets accounted for by all the families enumerated under the 70 per cent rule for a city.

[43] See Hayward R. Alker, Jr., *Mathematics and Politics* (New York: Macmillan, 1965), pp. 30–42. A Lorenz Curve is a plot of values held by cumulative proportions of a population.

[44] Richard I. Hofferbert, "Socioeconomic Dimensions of the American States: 1890–1960," *Midwest Journal of Political Science*, 12 (August, 1968), 401–418, finds measures of urbanization and industrialization loading on the same factor which he calls "industrialization."

[45] In order to derive factors uncorrelated with each other, we used orthogonal rotation. While oblique factor solutions are often a closer approximation to reality in producing intercorrelated factor solutions, use of factor scores in regression equations influenced our decision to derive (when necessary) uncorrelated ecological variables. Entering highly intercorrelated variables into a regression equation may produce unreliable results. For a discussion of the assumptions behind different forms of rotation see Norman Nie, Dale H. Bent, and C. Hadlai Hull, *SPSS: Statistical Package for the Social Sciences* (New York: McGraw-Hill, 1970), chapter 17.

Table 1. Factor Analysis of Industrial Structure[a]

	Factor 1 Loadings (Urban Industrialization)	Factor 2 Loadings (Agro-Business)	Communality
Mean Employment			
Mining-Mfg.	.07	.82	.67
Mining, Utilities, Const. & Mfg.	.23	.87	.81
% Total Employment			
Mining-Mfg.	.92	.24	.90
Mfg. Production Employees	.87	−.05	.76
Mfg.	.92	−.01	.84
Mfg. 2	.90	.16	.84
Per Capita			
Mining-Mfg. Assets	.44	.55	.49
Mfg. Fixed Assets	.59	.41	.51
Mean Fixed Assets			
Mining-Mfg.	.01	.68	.46
Mfg.	.08	.67	.76
Variance Explained	65.7	34.3	

[a] Data are from the Bureau of Census and Statistics, *Economic Census of the Philippines: 1967*, except for statistics on mfg. 2, which are taken from the Bureau of Census and Statistics, *Directory of Key Establishments of the Philippines, 1967*. Since both sources had yet to be published, the variables are computed from raw data. There were fewer firms in the Economic Census than the Directory because the former requires quite detailed and extensive information from firms and thus received a lower response rate.

Table 2 illustrates clearly why common measures cannot be assumed equivalent across systems. The degree of urbanization appears to be part of a dimension depicting the commercial-service nature of Philippine urban areas, and is not related to industrialization, as Hofferbert and others have found it to be in the United States.[46]

The fact that per capita small firms in manufacturing appear in a dimension measuring small-scale economic activity rather than one measuring urban industrialization again should caution us against assuming common measures equivalent. The relatively high intercorrelations shown in Table 3 among small and medium-sized establishments per capita, regardless of economic classification, suggest in part that establishments employing less than twenty persons do not so much "pull" employment into urban areas as represent the effort by labor to survive given the scarcity of stable employment in the cities. Chinese participation in the petty economic sector in the Philippines may also help account for the intercorrelations among different types of small-scale firms.[47] In both cases petty economic activity appears to be a response by class or ethnic groups to a lack of opportunity in other economic sectors.

Factor-analytic data-reduction techniques are also used to create an index of social mobilization. As defined by Deutsch, social mobilization is a general process "in which major clusters of old social, economic and psychological commitments are eroded or broken and people become available for new patterns of socialization and behavior."[48] Deutsch suggests that a number of indicators measure the mobilization process representing the general availability of persons for nonagricultural occupations, the percentage of the population residing in urban areas, and the proportion of the population which is literate.[49] To ascertain whether such indicators measure an equivalent process across systems, an orthogonal factor analysis was performed on a group of nine variables; the analysis produced two dimensions—(1) social mobilization and (2) city size and diversity (see Table 4). A number of common social mobilization indicators indeed appear to be equivalent across systems. Some of the variables loading highly on the mobilization factor are: percentage of the population engaged in farming, literate, urban; and the percentage of households with electricity. Economic complexity, population density, and population size combine to form the

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[46] Hofferbert.

[47] Economic census figures (while probably understating actual Chinese control) indicate, however, that alien control is limited in the smallest firms. Among establishments employing 10 or fewer individuals, Filipinos owned 90.0 per cent of those in commerce, 92.1 per cent of those in manufacturing, and 92.2 per cent of those in services. Bureau of Census and Statistics, *Economic Census of the Philippines: 1961*, Vols. 3, 6, 8 (Manila, 1965).

[48] Deutsch, p. 494.

[49] *Ibid.*, pp. 501–503.

Table 2. Factor Analysis of Economic Structure[a]

	Factor 1 Loadings (Small-Scale Economic Activity)	Factor 2 Loadings (Development of Commerce and Service)	Factor 3 Loadings (Urban Industrialization)	Communality
Degree of Urbanization	.29	.54	−.12	.40
Economic Complexity	.19	.87	.40	.95
Per Capita Small Firms				
Mfg.	.85	.40	−.08	.89
Commerce	.94	.26	.06	.96
Service	.84	.32	.04	.82
Mean Employment				
Service	.20	.36	−.18	.20
Commerce	.22	.74	.21	.64
% Total Employment				
Commerce	.42	.73	.23	.76
Service	.43	.54	−.24	.54
Mining-Mfg.	.01	.27	.80	.71
Per Capita				
Mining-Mfg. Assets	−.17	−.18	.65	.49
Value-Added Mfg.	.10	.06	.61	.39
Variance Explained	64.8	24.6	10.6	

[a] Sources: Degree of Urbanization (per cent of the population that is urban)—advanced reports for the Bureau of Census and Statistics, *Census of the Philippines, 1970, Population and Housing*, economic complexity (number of different industry types in a given city, utilizing a 4-digit classification)—from data collected for the Bureau of Census and Statistics, *Directory of Key Establishments of the Philippines, 1967;* per capita small firms (5 to 9 employees)—Department of Labor, *Directory of Key Establishments in the Philippines, 1959–1960;* other variables—data collected for the Bureau of Census and Statistics, *Economic Census of the Philippines: 1967.*

second dimension. The two factors intercorrelate +.53 after oblique rotation.[50]

Ethnic uniformity is measured by the standard deviation from the mean of the percentage voting-age population that is in each of the eight major language groups within a given chartered city. Cities with the greatest ethnic uniformity (a high concentration of persons in one language group such as Tagalog) have large variances, while those with more diversity and a more even distribution of language groups have smaller variances.

Measures of the strength of political machines utilize local rather than regional or national electoral statistics. As Landé has argued, Philippine political parties

> emerged from an upward rather than downward organization of forces . . . interests of modern Filipino voters . . . are primarily local interests and their first loyalties are to their local leaders.[51]

Parties are little more than agglomerations of vertically linked patron-client sets of alliances, segments of which may switch parties when expedient. Thus, local machine strength is viewed as the degree of control which influential patrons have in an area over time, irrespective of the party affiliation of the patron. Such control is measured by the mean plurality achieved by the winning mayor(s) in the three local elections held between 1959 and 1967.[52]

To confirm the validity of using local rather than national electoral statistics for measures of machine strength, we predict that the degree of machine strength at the local level is more stable for purely local elections than for national ones. Delivering support to national and regional allies is more complex than generating local support for oneself. Patron-client tiers extending from the *barrio* level through the city level and on up to the national level are more complex than tiers extending only from the *barrio* to the city level, and therefore one would expect greater "leakage" to occur in alliance structures leading to the national level than in those leading to the city level. Multiple regression analyses confirm the greater stability of local than of national electoral support

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[50] Since oblique rotation does not necessarily produce factors uncorrelated with each other, its factors are often a closer approximation of reality than orthogonal solutions. In this case, however, the factors derived from oblique rotation were virtually identical to those derived from the orthogonal solution.

[51] Carl Landé, *Leaders, Factions and Parties: The Structure of Philippine Politics,* p. 4.

[52] While some cities had elections for certain local offices before 1959, all mayoral and city councilor positions became elective for all cities with the passage of the Local Autonomy Act in 1959 (R.A. 2264). City residents participated in national elections, however, even before Philippine independence. The earliest national election data available to us for all chartered cities was for the 1957 election.

Table 3. Intercorrelations of Different Forms of Petty Economic Activity[a]

Item	No. of Establishments Per Capita					
No. of Establishments Per Capita	Small Mfg.	Med. Mfg.	Small Service	Med. Service	Small Commerce	Med. Commerce
Small Mfg.	1.00	.67	.87	.56	.91	.80
Med. Mfg.		1.00	.58	.50	.59	.72
Small Service			1.00	.56	.87	.76
Med. Service				1.00	.57	.58
Small Commerce					1.00	.87
Med. Commerce						1.00

[a] All coefficients are significant beyond the .01 level. Small establishments employ 5–9 people, and medium establishments 10–19 people. Source: Department of Labor, *Directory of Key Establishments in the Philippines, 1959–1960.*

over time. There are only two previous local elections to enter into the equation to predict the level of support for the winning mayor in 1967, as compared with three previous national elections to predict the level of support for the strongest congressional candidate or the strongest presidential candidate in 1969. In spite of the inclusion of fewer variables, slightly more variance is explained in the level of mayoralty support than in the level of either congressional or presidential support (.50 as compared with .48 for both of the latter).

The validity of mean plurality of votes for the winning mayor as a measure of local machine strength is further confirmed by its high intercorrelations with other measures of local dominance. Such measures include the mean percentage of city councilors belonging to the same party as the mayor, and turnover among all elected city officials.[53] While parties have scant existence apart from cliques and factions, a councilor affiliated with the same party as the mayor is more likely to be an ally or perhaps a client of the mayor than a councilor belonging to the opposing party.

The number of local political factions is measured by the mean number of "excess" candidates running for local political office. Since the norm for local political competition in a two-party system such as exists in the Philippines is that for each local office there will be two candidates on the slate, any deviation from this norm is an ex-

Table 4. Factors Utilized to Construct Social Mobilization Scale and City Size and Diversity Scale[a]

Item	Factor 1 Loadings (Social Mobilization)	Factor 2 Loadings (City Size & Diversity)	Communality
% Population[b]			
In Farming	−.66	−.38	.69
Literate	.83	.11	.85
High School Ed.	.89	.17	.88
Degree of Urbanization	.73	.23	.65
% Households with			
Electricity	.85	.43	.96
No Radio	−.79	−.46	.95
Economic Complexity	.33	.90	.87
Population			
Density	.23	.69	.60
Log to Base 10	.16	.85	.83
% Variance Explained	81.8	18.2	

[a] Sources: Degree of Urbanization, economic complexity—see Table 2; all other variables are from the Bureau of Census and Statistics, *Census of the Philippines, Population and Housing;* population density is from the 1970 census, others from the 1960 census.

[b] Population bases for these variables are as follows: farm population (economically active population), literacy (population over 10 years of age), high school educated (population over 6 years of age).

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[53] Mean vote for winning mayor is strongly correlated with the mean percentage of city councilors belonging to the same party as the mayor (+ .62) and with turnover among all elected city officials over the three terms of office for which election data are included (−.59). With an N of 46 both correlation coefficients are significant beyond the .001 level.

cess or deficit. The number of local political factions is a mean of the number of excess or deficit candidates in three years for which local election data were available.

Voting turnout is used to measure the strength of political mobilization. A grand mean as well as separate means for both local and national elections are computed.[54] While Philippine cities do not as a group experience a change in level of turnout, individual cities do have increases and decreases.[55]

Change in national turnout is measured by the deviation of mean turnout in the 1965 and 1969 elections from the regression line computed by the regression of mean late turnout (1965 and 1969) on mean early turnout (1957 and 1961). This procedure controls for any variation among initial levels of turnout which might affect changes in future levels of turnout.[56] Negative residuals imply then that turnout has decreased while positive residuals suggest an increased turnout. Change in local turnout is computed in a similar fashion, with the exception that early and late turnout are measured by single years (1959 and 1967).

Instability in pluralities is measured by the standard deviation of pluralities in the local and national elections between 1957 and 1969. Cities with fluctuating pluralities will have large standard deviations, while those with stable pluralities will have small standard deviations.

Stable party affiliation is measured by a dummy variable. Cities with either a Liberal mayor over all election years or a Nacionalista mayor over all election years are scored 1, while those with both Nacionalista and Liberal Party mayors are scored 0. Stable opposition is measured in a similar way. Since Nacionalista presidents were elected for three of the four national election periods, opposition is defined by Liberal Party membership. Cities with Liberal mayors over all election periods are scored 1, while other cities are scored 0.

Capital Concentration and Economic Diversification

As shown in Table 5, the degree of urbanization appears to be the most important determinant of economic complexity. Both the percentage of foreign ownership and the indigenous capital concentration are also significant elements in the equation.

As evident in Table 6, the indigenous capital concentration and, to a lesser extent, the degree of urbanization are determinants of urban industrialization. The relationship between urban industrialization and the percentage of manufacturing that is foreign is considerably weaker. This beta coefficient rises, however, when the percentage of manufacturing that is American is substituted for the percentage that is foreign.

The pattern and scope of foreign investment in the Philippines provides clues as to why the percentage of manufacturing that is American explains more variance in urban industrialization than does the percentage of manufacturing that is

Table 5. Multiple Regression Equations for Determinants of Economic Complexity[a]

Dependent Variable	Independent Variables	*B*	Beta	Standard Error of *B*
Economic Complexity[b]	% Foreign Ownership	2.6527	.26*	1.2846
	Indigenous Capital Concentration	0.0035	.45**	0.0010
	Degree of Urbanization	0.0221	.71**	0.0041
	(Constant) −0.672	$F = 12.6$ with 3,25 d.f.	$R^2 = .60^{**}$	

[a] Sources: % Foreign ownership (percentage of total stockholdings that are controlled by foreigners)—data collected for the Bureau of Census and Statistics, *Economic Census of the Philippines: 1967;* indigenous capital concentration—see pp. 1154–1155; degree of urbanization, economic complexity—see Table 2.

[b] The natural log of economic complexity is utilized in order to lessen the skew of this variable (2.501) given the great differences between cities in the Greater Manila Area and smaller ones in the number of different types of economic establishments found in each.

* $p \leq .10$ on a one-tailed test. Significance tests are reported for the interested reader even though we are analyzing the universe of Philippine chartered cities rather than a sample.

** $p \leq .05$ on a one-tailed test.

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[54] The mean begins with the 1957 election and includes elections for 2 year intervals through the 1969 election. Turnout is the proportion of registrants voting.

[55] Mean voting turnout in these national elections fluctuates between 75 per cent and 77 per cent for the chartered cities as a group, and in local elections it fluctuates between 76 per cent and 79 per cent.

[56] See George W. Bohrnstedt, "Observations of The Measurement of Change," in *Sociological Methodology,* ed. Edgar F. Borgatta and George W. Bohrnstedt (San Francisco: Jossey-Bass, 1969). See also Norris, *Political Behavior,* pp. 22–24. For our data the use of residuals produces a solution similar to that obtained by subtracting turnout in the early years from turnout in the late years. The intercorrelations between the two alternative measures of change in local and national turnout are all over +.98.

Table 6. Multiple Regression Equations for Determinants of Urban Industrialization[a]

Dependent Variable	Independent Variables	B	Beta	Standard Error of B
	Equation 1			
Urban Industrialization	Indigenous Capital Concentration	.00369	.56**	.00107
	Degree of Urbanization	.00775	.29	.00430
	(Constant) −2.9361	F=6.5 with 2,27 d.f.	R^2 = .32*	
	Variables with F less than 1.0 not in the equation			
	Variable		Partial	
	% Mfg. Foreign		.04	
	Equation 2			
Urban Industrialization	Indigenous Capital Concentration	.00343	.52**	.00104
	Degree of Urbanization	.00913	.34*	.00420
	% Mfg. American	1.14007	.29	.62567
	(Constant) −2.96248	F=5.8 with 3,26 d.f.	R^2 = .40**	

[a] Sources: Indigenous capital concentration—see pp. 1154–1155; degree of urbanization—see Table 2; % mfg. foreign and % mfg. American—from data collected for the Bureau of Census and Statistics, *Economic Census of the Philippines: 1967;* urban industrialization—factor score derived from Table 1.

* $p \leq .10$ on a one-tailed test.

** $p \leq .05$ on a one-tailed test.

foreign. The largest manufacturing firms are American- rather than Chinese- or European-controlled. American investments constitute the biggest block (about 83.7 per cent) of the 67 foreign-owned firms among the top two hundred corporations in the Philippines.[57] American investment is more balanced between heavy and light manufacturing than is investment by the second largest foreign group, the Chinese. Approximately 47 per cent of American-owned manufacturing firms are classified as engaging in heavy manufacturing (e.g., refining of petroleum products) rather than light manufacturing (e.g., textiles and food processing), while heavy manufacturing accounts for only 14 per cent of Chinese-controlled firms.[58]

Greater diversification of American than Chinese investment in manufacturing does not mean extensive American participation in building up basic Philippine industries. On the contrary, realms of American domination suggest a high degree of dependence on capital imports—e.g., in petroleum refining and distribution (Caltex, Shell, Esso, and Mobil Oil), assembly and manufacture of appliances (USIPHIL, Aircon), assembly of transport equipment (Ford Philippines, Chrysler Philippines), assembly of office equipment (IBM), and manufacture of tire and rubber products (B. F. Goodrich and Goodyear).[59] Much recent growth of manufacturing in the Philippines is largely a function of growth in capital goods imports, with only a small growth in domestic output of capital equipment.[60]

The commercial nature of much American investment in the Philippines is illustrated by comparing Table 7 with Table 6. There is a stronger relationship between the percentage of commerce that is American and commercial development (as measured by both the mean employment in commercial establishments and the percentage employed in commercial establishments) than be-

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[57] "The Top Two Hundred Foreign Firms," *Manila Chronicle Magazine Supplement,* July 7, 1971, p. 1.

[58] Computed from Bureau of Census and Statistics, *Economic Census of the Philippines, 1961,* Vol. III, Table 25.

[59] See "The Top Two Hundred Foreign Firms."

[60] See Hicks and McNicoll, *Trade and Growth in the Philippines,* p. 72. While the commercial, processing and assembly nature of much American investment in the Philippines helps perpetuate dependence on capital imports, equally significant for the pattern of economic growth in the Philippines is the amount of capital outflow. For the six years following 1957, earnings accruing to American foreign investors were more than twice the amount of direct foreign investment in the Philippines. See Frank H. Golay, "Economic Collaboration: The Role of American Investment," in *The United States and the Philippines,* ed. Frank H. Golay (Englewood Cliffs: Prentice-Hall, 1966), p. 110. From 1965 to 1968, new American investment amounted to $30.2 million while withdrawals were more than $59 million. A. B. Santos, "The Philippines—Country with an Image Problem," *Manila Chronicle,* January 6, 1971, p. 9. These figures overstate the amount of net capital inflow since many foreign firms raise considerable amounts of their capital requirements from Philippine banks. In 1968 the National Economic Council found that 108 American firms over the period from 1956 to 1965 raised 84 per cent of their capital requirements from Philippine Banks, and only 16 per cent from foreign sources. Cited in the *Manila Chronicle,* May 4, 1971, p. 9.

Table 7. Multiple Regressions for Determinants of Commercial Development[a]

Dependent Variable	Independent Variables	B	Beta	Standard Error of B
	Equation 1			
Mean Employment Commercial Est.	% Commerce American	86.4342	.51**	23.9531
	Indigenous Capital Concentration	0.0261	.42**	0.0083
	Degree of Urbanization	0.1220	.49**	0.0357
	% Commercial Chinese	8.9524	.21	5.8854
	(Constant) −11.076	$F=8.8$ with 4,24 d.f.	$R^2=.58$**	
	Equation 2			
% Employed Commercial Est.	% Commerce American	952.3636	.58**	200.3931
	Indigenous Capital Concentration	0.2840	.48**	0.0715
	Degree of Urbanization	1.0123	.43**	0.3119
	% Commercial Chinese	31.7998	.08	51.3823
	(Constant) 192.1564	$F=11.72$ with 4,24 d.f.	$R^2=.65$**	

[a] Sources: Indigenous capital concentration—see pp. 1154–1155; degree of urbanization—see Table 2; other variables—data collected for the Bureau of Census and Statistics, *Economic Census of the Philippines: 1967.*
** $p \leq .05$ on a one-tailed test.

tween the percentage of manufacturing that is American and urban industrialization. While American investment in the Philippines has been slightly greater in manufacturing than in large-scale commerce, American-controlled manufacturing is dispersed over a wider geographic base. Manufacturing investment in food processing linked to the primary sector (e.g., Dole Philippines and Franklin Baker) is found in certain less developed cities. Such a dispersion helps account for the weaker relationship between the percentage of manufacturing that is American and urban industrialization.

Economic and Sociodemographic Structure

As anticipated, agro-business is not positively correlated with social mobilization or the degree of urbanization (see Table 8). Urban industrialization (investment in manufacturing for the internal market) is positively correlated with both population density and economic complexity. While investment in various types of manufacturing increases the level of economic complexity in a city, such investment is in turn stimulated by the existence of sufficient numbers of people to constitute a market (as measured by population density). The spatial separation which we argued exists between the level of community wealth supportive of higher levels of social mobilization and one important source of this wealth in rural areas is also apparent in the more urbanized areas. That is, urban industrialization is only weakly related to social mobilization. For many of the more urbanized areas in the Philippines, colonial heritage dictated a substantial commercial and administrative base.[61] Additionally, Southeast Asian cities have tended to develop distinguishable patterns of land use. While the commercial-business district often constitutes the center of the larger Philippine urban areas, factory zones appear in outlying areas with the creation of new industrial estates (or appeared adjacent to the Port as a result of the emergence of processing industries during the colonial era).[62]

The absence of clearly demarcated city types of the kind found in the West is suggested by the positive relationships between commercial and service development (mean employed in commerce, in service; per cent employed in commerce, service), and forms of petty commerce and industry (per capita small manufacturing, commerce). As McGee notes in discussing land-use patterns in Southeast Asian cities:

> The mixed economic structure of the cities in which highly-developed forms of Western capitalism, such as banks and trading firms, coexist with pre-industrial and semi-capitalistic forms of economic organization like the Chinese loan association and the mobile street markets of the indigenous populations, inhibits large areas of homogeneous land use from emerging.[63]

While residentially exclusive areas in the suburbs with their large banks and supermarkets resist both the intrusion of squatters and open-air markets whenever possible, squatters still move into these areas—often because of the under-utilization of land and the desire to be as close as

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[61] See McGee, *The Southeast Asian City,* chapter 3.
[62] *Ibid.,* p. 133.
[63] *Ibid.,* p. 126.

Table 8. Intercorrelations of Measures of Economic and Sociodemographic Structure[a]

	Agro-Busin.	Urban Indus.	Soc. Mobil.	Per Cap. Small Mfg.	Per Cap. Small Commerce	Mean Employed Commerce	Mean Employed Service	% Employed Commerce	% Employed Service	Degree of Urbaniz.	Pop. Density	Econ. Complexity
Agro-Business	1.00	.26	−.25	−.32	−.39	−.23	−.24	−.16	−.26	−.20	−.00	−.13
Urban Indust.		1.00	.12	.20	.32	.41	.10	.48	.14	.16	.32	.64
Soc. Mobil.			1.00	.49	.37	.46	.29	.29	.59	.75	.23	.35
Per Cap. Small Mfg.				1.00	.91	.48	.38	.49	.61	.48	.23	.48
Per Cap. Small Commerce					1.00	.39	.29	.63	.55	.40	.21	.46
Mean Employed Commerce						1.00	.31	.73	.40	.45	.58	.77
Mean Employed Service							1.00	.22	.52	.15	.03	.28
% Employed Commerce								1.00	.49	.47	.73	.86
% Employed Service									1.00	.53	.20	.40
Degree of Urbanization										1.00	.31	.47
Population Density											1.00	.72
Economic Complexity												1.00

[a] Sources: Agro-business and urban industrialization—factor scores generated in Table 1; social mobilization—factor score generated in Table 4; population density—see Table 4; other variables—see Table 2. Correlations of .24 or above are significant beyond the .05 level.

possible to their place of work. In the Greater Manila area, squatters have moved both into the inner city (in areas such as Intramuros which suffered extensive war damage) and into encampments near suburban villages for the middle and upper class in some of the more outlying districts in Quezon City or Pasay City.

The Strength of Machines

As evident in Table 9, both ethnically defined social bases (a lack of ethnic uniformity) and the size of the class from which local candidates tend to come (the percentage of the population that is college-educated) are determinants of the number of local political factions. Since the percentage of the population that is college-educated is highly correlated with social mobilization ($r = +.80$), it is also likely that that percentage reflects the extent of social differentiation. In order for increased factionalism to be meaningful, there must be both an increase in the number of competing patrons and in the diverse groups of clients willing to support such patrons. The strong impact of the number of local factions on local machine strength (Table 10) suggests that the proliferation of political factions is indicative of a real diffusion of power rather than a proliferation of totally powerless local candidates.

The second major determinant of local machine strength in Table 10 is the prevalence of agro-business. Company towns arising from the presence of large sugar centrals or mines are conducive to maximum political control by a local elite with close ties to managers or owners of the corporation. While such settings do imply the commercialization of exchange relationships, any loss of control incurred from the increasingly monetary nature of exchange is more than compensated for by the corporation's monopolization of employment opportunities and sources of credit. Workers in sugar centrals and mines tend to be chronically in debt to the company store, for example, and extremely vulnerable to manipulation for the employer's or other purposes.[64] Additionally, since agro-business so dominates the economy of the area in which it is located, it can provide an important source of revenue to lubricate local machines—through such formal channels as taxes and also more informal payoffs or contributions.

City size and diversity is a weaker but positive determinant of local machine strength. This relationship may be partially explained by the high correlations between city size and diversity, on the one hand, and urban industrialization ($r = .61$) and the percentage employed in large commercial

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[64] See for example Nanette Garcia Duñgo, *A Southern Industrial Complex* (Quezon City: Community Development Research Council, University of the Philippines, 1969).

Table 9. Multiple Regression Equations for Determinants of Number of Local Political Factions[a]

Dependent Variable	Independent Variable	B	Beta	Standard Error of B
No. of Local Political Factions	Ethnic Uniformity	−.0440	−.27*	.02395
	% Population College Educated	.0399	.27	.02211
	(Constant) 3.3515	$F=5.23$ with 2,43 d.f.	$R^2=.20$*	
	Variables with F less than 1.0 not in the equation			
	Variable	Partial		
	Urban Industrialization	.11		
	Agro-Business	.14		
	City Size and Diversity	.12		

[a] Sources: Ethnic uniformity—from a 1960 Philippine Census Sample obtained through John Koehler of the Rand Corporation; % population college educated from the Bureau of Census and Statistics, *Census of the Philippines, 1960, Population and Housing;* number of local political factions—data from the Philippine Commission on Elections (the natural log is utilized since the number of local political factions increases exponentially in primate area cities); other variables—see Tables 1 and 4.

* $p\leq.10$ on a one-tailed test.

establishments ($r=.80$), on the other. Machines able to tap local sources of finance and industrial capital in exchange for favors are likely to be stronger than those lacking access to such wealth.

While the simple correlation between social mobilization and local machine strength ($r=-.37$) becomes weaker in the regression equation, the correlation is in the anticipated direction. This finding supports our argument that alliance structures become more unstable as the form of exchange between patron and client becomes more specialized. The process may not be a perfectly linear one, however, and it may be more complex than we had anticipated. Our findings linking social mobilization to weakness in machines appear contradicted by those of Machado, who, in his comparative study of municipal mayors from Batangas and Capiz provinces, finds less turnover among mayors from municipalities with higher levels of social mobilization and with less farm tenancy (where mayors less often tend to be upper class) than from those less disturbed by social changes.[65]

While Machado cites longer tenure in office by those mayors from more humble class backgrounds as evidence for the increased professionalization of politics, there is no indication that such professionalization fundamentally changes the bases upon which political factions are organized. The movement away from diffuse landlord-tenant control to more specialized patterns of exchange suggests that political alliances become more unstable, *unless* the local politician

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[65] Kit G. Machado, "Changing Patterns of Leadership Recruitment and the Emergence of the Professional Politician in Philippine Local Politics," *Philippine Journal of Public Administration,* 16 (April, 1972), 147–169.

Table 10. Multiple Regression Equations for Determinants of Local Machine Strength[a]

Dependent Variables	Independent Variables	B	Beta	Standard Error of B
Local Machine Strength	No. of Local Political Factions	−6.4549	−.62**	1.3024
	Agro-Business	3.0106	.29**	1.2288
	Social Mobilization	−1.5878	−.14	1.1385
	City Size and Diversity	1.4949	.16	1.3699
	(Constant) 69.4344	$F=9.70$ with 4,41 d.f.	$R^2=.49$**	
	Variables with F less than 1.0 not in the equation			
	Variable	Partial		
	Urban Industrialization	−.09		
	Ethnic Uniformity	.11		

[a] Sources: Local machine strength—from data of the Philippine Commission on Elections; other variables—see Table 9.

** $p\leq.05$ on a one-tailed test.

Table 11. Level of City Development By Number of Times Mayor Re-elected[a]

	Number of Times Mayor Re-elected			
	1	2	3	N
Level of City Development				
Most	7.1%	71.4%	21.4%	14
Medium	9.1%	50.0%	40.9%	22
Least	20.0%	20.0%	60.0%	10
Chi Square = 6.35 with 4 d.f., $p \leq .17$.				
Medium Level Cities (High vs. Low)				
	1	2	3	N
Medium High	8.3%	50.0%	41.7%	12
Medium Low	10.0%	50.0%	40.0%	10
Chi Square = .03 with 2 d.f., *N.S.*				

[a] Sources: Number of times mayor re-elected—from data of the Philippine Commission of Elections; level of city development—classifications developed by the Philippine Department of Finance, as discussed in Republic Act 4477, based on a city's average annual income for four years preceding their classification (Class Special, 1A and 1 are most developed; Class 2 and 3 are medium developed; and Class 4 through 8 are least developed. Medium high cities are Class 2 and medium low cities are Class 3.)

has access to sufficient resources to "buy off" enough support over long periods of time.[66] Since public resources are most scarce in highly urbanized areas, there may be certain thresholds that affect the relationship between the length of mayoral incumbency (which is one measure of local machine strength) and the level of city development. That is, in areas with intermediate levels of development, the professional politician may compensate for resources he has "lost" in his not coming from a prominent local family by cultivating (or being cultivated by) national sources of patronage. Since such politicians assume important brokerage roles between local clients and the national bureaucracy or national politicians, local political professionals may be able to mobilize sufficient office-based and patronage resources to enable them to stay in power. At even greater levels of urbanization (which are not included in Machado's sample, but are in ours) the effectiveness of local political machines decreases as it becomes impossible to marshall sufficient resources to meet the rising demands of increasingly diverse social groupings. Table 11 indicates that while the level of city development is inversely related to the number of times the mayor has been re-elected, there is no relationship between the two variables for cities with medium-high and medium-low levels of city development.

The weakness of machines in more developed cities suggests that there are determinate limits to exchanging local political support for national patronage resources without both the emergence of a more cohesive set of political structures such as a single or mass party and an increased flow of patronage resources to broad groups of clients. The important brokerage functions which local machines perform for national elites and clients is confirmed by the correlation between the strength of pluralities gained by the strongest presidential candidate in a city and the strength of local machines ($r = .63$). The instability of nationally linked coalitions in more developed cities is especially apparent in the intercorrelations between local and national pluralities over time. Areas in which local machines have considerable control should show greater stability in the pluralities such elites achieve for themselves and are able to deliver to national allies. Tables 12 and 13 indicate that pluralities for mayor tend to be most highly intercorrelated with pluralities for president in the least developed cities and in cities with the strongest local machines.

The limited effectiveness of patronage resources for building coalitions is apparent in the difference between the mean pluralities generated for local as compared with national candidates. It is anticipated that the strongest voting coalitions will be built at the time of national elections (since the national government has more patronage resources and national candidates run at large); this means that local candidates are motivated to deliver maximum pluralities to national candidates with whom they are affiliated. As shown in Table 14, national candidates receive higher mean electoral pluralities than local candidates do regardless of the level of city development. The smaller difference between national and local pluralities in more developed cities indicates limits to expanding coalitions in such areas during national elections. These differential pluralities cannot be attributed to any bias toward allocating patronage resources to less urban areas. If anything, the data indicate that more urban areas receive more patronage. The amount of large public works expenditures per capita from 1966–1969 is weakly but positively correlated with the level of urban industrialization ($r = .27$) and city size and diversity ($r = .25$). The amount of small public works expenditures per capita is not correlated with urban industrialization, but it is negatively correlated with city size and diversity ($r = -.29$). Correlations between the level of social mobiliza-

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[66] In his study of local leaders in Camarines Norte, Benson notes that "jobs, projects and money were shown to be the most important considerations for a *lider* to support a candidate for governor or congressman." See Lewis Benson, *Political Leadership Through Political Liders: A New Approach for the Analysis of Philippine Provincial Leadership Positions* (Ph.D. dissertation, University of Hawaii, 1970), pp. V–VI.

Table 12. Intercorrelations Between Local and National Pluralities Over Time, by Level of City Development[a]

	Plurality for Presidential Candidate				
	1957	1961	1965	1969	
Most Developed Cities					
Plurality for Mayor					
1959	.10	.77*	−.07	−.20	
1963	.14	−.14	−.05	.11	
1967	−.04	.28	−.19	.44	($N=14$)
Moderately Developed Cities					
Plurality for Mayor					
1959	.65*	.26	.52	.42	
1963	.50*	.03	.60*	.03	
1967	.36*	−.15	.62*	.41*	($N=22$)
Least Developed Cities					
Plurality for Mayor					
1959	.42	.59*	.52	.42	
1963	.35	.80*	.60*	.66*	
1967	.63*	.77*	.57*	.57*	($N=10$)

[a] Sources: Electoral pluralities—from data of the Philippine Commission on Elections; level of city development—see Table 11.

* $p \leq .05$ on a one-tailed test.

tion and either small or large public works expenditures per capita are all under .24, and therefore not significant at the .05 level.

The Stability of Local Machine Strength

The weakening of clientelist alliance structures can be further illustrated by the inability of dominant patrons to create organizations able to mobilize stable support over time. A decline in the integrative capacity of political machines requires more than a change from a pattern of competition in which a single machine has a wide range of power to one in which two machines have stable clienteles. Since we argue that our measure of machine strength does *not* measure a simple transition from a stable unifactional system in which pluralities are high to a stable bifactional system in which pluralities would be lower, we should be able to demonstrate that the strength of pluralities achieved by winning mayors in the more developed areas is not stable over time. By contrast, a stable bifactional system with a small

Table 13. Intercorrelations Between Local and National Pluralities Over Time, by Local Machine Strength[a]

	Plurality for Presidential Candidate				
	1957	1961	1965	1969	
Least Strong Local Machines					
Plurality for Mayor					
1959	.24	.17	.19	−.00	
1963	.20	−.37	.34	.00	
1967	.36	.11	−.21	.07	($N=16$)
Moderately Strong Local Machines					
Plurality for Mayor					
1959	.20	−.45*	−.15	−.27	
1963	−.30	.02	.19	.16	
1967	−.05	.57*	−.05	.19	($N=16$)
Most Strong Local Machines					
Plurality for Mayor					
1959	.68*	.50*	.57*	.62*	
1963	.60*	.55*	.50*	.31	
1967	.40	.21	.81*	.74*	($N=14$)

[a] Sources: see Tables 10 and 12. Local machine strength is measured by mean mayoralty pluralities—least strong local machines have pluralities of 50% or less; moderately strong have pluralities between 50.1% and 55.0%; and most strong have pluralities greater than 55.1%.

* $p \leq .05$ on a one-tailed test.

Table 14. Level of City Development by Mean Pluralities in Local and National Elections[a]

	Mean Electoral Plurality (%) Local	National	N
Level of City Development			
Most	50.4	53.1	14
Medium	53.2	56.9	22
Least	54.2	64.5	10

[a] Sources: See Tables 11 and 13.

swing vote should produce relatively equal pluralities for winners over time. Electoral data indicate that stable bifactionalism has not emerged in the most developed areas. For the most developed cities, the strength of early pluralities for mayor (1959 and 1963) predict very little variance in the strength of the plurality in the 1967 mayoral election ($R^2=.01$). The amount of variance which early pluralities predict for the later one is greater in moderately developed cities ($R^2=.65$) and is highest for the least developed cities ($R^2=.70$). The plurality for mayor in 1967 is better predicted by earlier pluralities in cities with strong local machines than in cities with weak machines ($R^2=.45$ as compared with $R^2=.02$).[67]

Political Participation

Central to our argument that machines become weaker with urbanization and social mobilization is the notion that lower- as well as middle-class groups become less firmly integrated into local clientelist structures. For middle-class groups, greater exposure to national media and the emergence of class interests sensitive to national government policies on such issues as inflation decrease interest in local issues.[68] If the weakening of machines demonstrates a broader process of change apart from the growth of the size of the middle class in urban areas, then a decrease in coverage provided by machines cannot be specific to a particular class.

Table 15 shows that the determinants of both local and national turnout are ethnic uniformity and, to a lesser extent, city size and diversity. Ethnic appeals are powerful stimuli for mobilization in societies such as the Philippines where primordial loyalities remain salient and ethnic cleavages exist among the national elite seeking support. For example, the mean percentage vote in the 1965 and 1969 elections for President Marcos, an Ilocano, is positively correlated with the percentage of the voting population that is Ilocano (+.44). In these two elections, one of Marcos's opponents was a Pampangan and the other Visayan. Since Marcos had a Pampangan opponent in the 1965 election, it is hardly surprising that the vote for Marcos is negatively correlated with the percentage of the population that is Pampangan ($r=-.38$). For the 1957 election the simple correlation between the percentage voting for President Garcia (a Visayan) and the percentage of the voting population that is Visayan is +.66.

To the degree that a lack of ethnic uniformity implies at the same time less salient ethnic group identification, lower community cohesiveness makes it more difficult for political brokers to mobilize people to vote. Such a situation is particularly likely when sufficient cross-cutting cleavages exist to counter potential ethnic conflict and competition—for example, when exposure to the national Tagalog culture gradually weakens ethnic bonds. While ethnic diversification may under certain conditions intensify and politicize primordial loyalties (e.g., when it is reinforced by religious cleavages as in Mindanao), it may also, if initial cleavages are not too deep, speed the process of accommodation if patronage rewards result from such accommodations and various ethnic leaders have incentives to create interethnic coalitions. The innate instability of factions in systems such as the Philippines, combined with the often intense competition between rivals from the same ethnic group, would seem conducive to the creation of interethnic coalitions. Only 10.9 per cent of our chartered cities have ethnic cleavages reinforced by religious ones, and most of these cities have clear Christian majorities which either force Moslem leaders into electoral coalitions with Christians or encourage insurgency.[69]

Machine strength is another determinant of mean national turnout. Strong machines can mobilize higher proportions of the electorate for presidential candidates with which they are affiliated than can weak machines. The low community cohesiveness common to large cities may make the mobilization of votes more difficult, and helps account for the negative Beta between city size and both national and local turnout (Table 15).

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[67] High levels of machine strength are measured by electoral pluralities of 52.1 per cent or higher, and low levels of machine strength by a mean plurality of 52.0 per cent or lower.

[68] See the discussion by Nie et al., "Social Structure and Political Participation," pp. 366–368.

[69] Basilan and Marawi are the only two of our cities with Moslem majorities (73 per cent and 86 per cent respectively). In the former the cleavages between the hill dwelling, nominally Moslem Yakan peoples and the coastal Tausog peoples are probably as intensive as those between Christian settlers and Tausogs. Such cleavages, combined with the high illiteracy among both Tausogs and Yakans, have made possible the election of a Christian mayor.

Table 15. Multiple Regression Equations for Determinants of Voting Turnout[a]

Dependent Variable	Independent Variables	B	Beta	Standard Error of B
	Equation 1			
Mean National Turnout	Ethnic Uniformity	.6711	.68**	.1004
	City Size and Diversity	−.8543	−.16*	.5576
	Local Machine Strength	.1853	.23**	.0834
	Social Mobilization	.8839	.13	.6659
	(Constant) 47.733	$F=19.7$ with 4,40 d.f.	$R^2=.66$**	
	Variables with F less than 1.0 not in the equation			
	Variable	Partial		
	Urban Industrialization	−.00		
	Agro-Business	−.06		
	Number of Local Political Factions	−.05		
	Equation 2			
Mean Local Turnout	Ethnic Uniformity	.6168	.65**	.0984
	City Size and Diversity	−1.5372	−.30**	.5292
	Agro-Business	− .6537	−.11	.5664
	(Constant) 62.025	$F=23.3$ with 3,41 d.f.	$R^2=.63$**	
	Variables with F less than 1.0 not in the equation			
	Variable	Partial		
	Social Mobilization	−.03		
	Urban Industrialization	−.04		
	Local Machine Strength	−.08		
	Number of Local Political Factions	−.02		

[a] Sources: Mean national and local turnout—data from the Philippine Commission on Elections; ethnic uniformity—see Table 9, local machine strength—see Table 10; other variables—from factor scores derived from Tables 1 and 4.

* $p \leq .10$ on a one-tailed test.

** $p \leq .05$ on a one-tailed test.

The weak positive Beta ($p > .10$) between mean national turnout and social mobilization makes it difficult to interpret the unexpected direction of the sign with any confidence.

For local turnout, machine strength is not a determinant. The incentive for strong local machines to maximize their potential to mobilize support is less strong in local than in national elections. It was mentioned previously that presidential candidates run at large and need a simple plurality to win, and consequently it is to their advantage to induce strong local machines to deliver a maximum of support to them. A local boss, by contrast, can win with a simple plurality in his city. Machines certain of victory can achieve savings by mobilizing only a portion of their clientele. The savings possible for elites with strong local power bases also helps explain the negative relationship between agro-business (often indicative of company towns) and local turnout.

Using a measure which approximates socioeconomic class (the level of education), we examined the differential influence of level of education on mean national and local voting turnout. Controlling for the three major determinants of turnout (ethnic uniformity, machine strength and city size and diversity) results in the following partial correlations with turnout: the proportion of the population with no education (−.15), with an elementary school education (+.23), with a high school education (+.02), with a college education (+.07). The fact that the crucial threshold occurs between "no education" and "elementary education" suggests that in a system of free mass elementary education a minimum amount of social mobilization is a much more important determinant of voting turnout than is middle class status.

Clientelist political mobilization means that mayoralty aspirants or local brokers have the loyalty of *barrio* leaders who can apply both the sanctions and rewards necessary to get voters to the polls. Areas with high proportions of less educated individuals only marginally participative in community economic life (e.g., agricultural workers or the unemployed) are likely to have low levels of mobilization because local leaders lack contact with and control over those only partially integrated into community activities. Anderson—

Table 16. Multiple Regression Equations for Determinants of Change in National and Local Turnout[a]

Dependent Variable	Independent Variables	B	Beta	Standard Error of B
	Equation 1			
Change in National Turnout	Social Mobilization	−1.0631	−.25*	.5735
	Ethnic Uniformity	.1856	.29**	.0823
	Stable Opposition	−3.4141	−.29**	1.5325
	Agro-Business	1.0980	.28*	.5203
	(Constant) −4.3588	$F=7.5$ with 4,40 d.f.		$R^2=43$**
	Variables with F less than 1.5 not in the equation			
	Variable	Partial		
	Instability in Pluralities	−.17		
	City Size and Diversity	−.18		
	Urban Industrialization	.00		
	Local Machine Strength	.13		
	Equation 2			
Change in Local Turnout	Social Mobilization	−1.9868	−.26*	1.0560
	City Size and Diversity	−2.2553	−.35**	.8698
	Stable Opposition	−5.2144	−.24*	2.9134
	(Constant) .6793	$F=6.52$ with 3,42 d.f.		$R^2=32$**
	Variables with F less than 1.5 not in the equation			
	Variable	Partial		
	Urban Industrialization	.03		
	Agro-Business	−.04		
	Local Machine Strength	−.06		
	Ethnic Uniformity	.05		
	Instability in Pluralities	−.07		

[a] Sources: stable opposition, instability in pluralities—from data of the Philippine Commission on Elections; other variables—same as Table 15.
* $p \leq .10$ on a one-tailed test.
** $p \leq .05$ on a one-tailed test.

in analyzing the situation of laborers, agricultural workers and the unemployed in the province of Pangasinan—notes that such individuals are

> economically very insecure . . . they participate to a more limited degree than any other category in the social, political, and religious affairs of the barrio.[70]

Similarly, Marxists have long been skeptical about the organizational potential of the lumpen-proletariat.[71] The weakness of social mobilization in explaining variance in voting turnout thus appears to be due to the curvilinear relationship between the two variables.

In Table 16 social mobilization is a determinant of change in both national and local turnout. To examine whether such drops in turnout are linked more specifically to the size of the middle and upper middle class, we recalculated the regression equations by substituting a more class-specific measure (the level of education) for social mobilization.

For local elections, substituting class-specific measures for social mobilization strengthens the Betas and weakens the influence of city size and diversity as another determinant of change in local turnout.[72] It appears then that declining local turnout can in part be explained by the lower interest in local affairs likely among the middle class. Since they are integrated into more specialized clientelist or nonclientelist structures such as the national bureaucracy or corporations, the middle sectors can only intermittently be served by local political machines.

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[70] James N. Anderson, "Land and Society in a Pangasinan Community," in *Social Foundations of Community Development*, ed. Socorro C. Espiritu and Chester L. Hunt (Manila: R. M. Garcia, 1964), p. 179.

[71] See the discussion by David Matza, "The Disreputable Poor," in *Class, Status and Power*, 2nd ed., ed. Seymour Martin Lipset and Reinhard Bendix (New York: The Free Press, 1966), pp. 289–302.

[72] For change in local turnout, substituting the percentage of the population that is either high-school or college-educated for social mobilization raised the absolute value of the Beta slightly to −.29. When only the percentage of the population college-educated alone is substituted, the absolute value of the Beta is raised even further to −.33.

More class-specific measures are weaker determinants of change in national turnout than is the general measure of social mobilization, however.[73] For drops in national turnout, our argument that relates measures such as social mobilization to the general deterioration in clientelist structures for mobilizing broad groups of people appears plausible.

While changes in national and in local turnout are intercorrelated ($r=.52$), the change in national turnout appears to be much more than solely a consequence of changes in local turnout. That is, there are significant partial correlations between changes in national turnout and the following variables, controlling for changes in local turnout: social mobilization ($r=-.41$); ethnic uniformity ($r=.40$); local machine strength ($r=.31$). More class-specific measures such as the percentage of the population that is college-educated, by contrast, are only weakly correlated with changes in national turnout ($r=-.19$). Both ethnic uniformity and local machine strength appear to support continued clientelist mobilization, as evident in their positive correlations with changes in turnout.

Stable opposition is a significant determinant of change in national and, to a lesser extent, in local turnout. Cities with Liberal Party mayors, given their opposition to Nacionalista presidents (elected in 1957, 1965, and 1969), are more likely to be deprived of important national patronage resources needed to mobilize clients. Since the cost of elections has been rising, declining turnout in Liberal Party areas may result from the economics necessary to achieve victory at the lowest expense. Declining turnout in Liberal Party areas does not appear to be also a function of the institutionalization of competition in such areas. Assuming that institutionalized competition is reflected in stable pluralities over time, the low correlation between stable opposition and the instability of pluralities ($r=.09$) suggests that oppositionist areas do not have particularly stable voting blocs. Furthermore, if stable party affiliation of mayors over time were a good indicator of institutionalized competition, one would expect some correlation between stable party affiliation (regardless of the party) and declining turnout. Change in national turnout is insignificantly correlated with stable party affiliation ($r=-.19$) and also with stable Nacionalista Party affiliation ($r=.02$).

The negative partial correlation in Table 16 between change in turnout and instability in local and national pluralities suggests, if anything, that factional instability over time is related to declining turnout. Our data provide no evidence that declining turnout is the result of a stabilization in factional competition.

A number of conditions help explain why agrobusiness is a determinant of increased turnout in national but not in local elections. We have already suggested that in areas where local elite power is strong, machines have more incentive to mobilize all their clientele in national as against local elections. The dependence of mines and agro-business on national sources of capital and credit as well as on favorable export-import policies provides further incentives for managerial personnel to collaborate with local politicians to deliver a maximum of support to favorably disposed national candidates.

Many of our previous results have indicated that deterioration in the integrative ability of political machines appears in a number of instances to be both a cause and a consequence of conditions which have decreased the proportion of income received by the bottom 40 per cent of the population between 1956 and 1971.[74] While such a deterioration is either irrelevant to or enhances the economic position of the middle and upper class, it means that urban workers and peasants are more fully exposed to market forces increasing the concentration of wealth. The dramatic rise in violent and mass urban activities, precisely during the period in which turnout declined, suggests that alternative structures capable of integrating and providing security for broad groups of people have yet to emerge on the Philippine scene.[75]

Much of the recent Moslem insurgency in the South of the Philippines can be traced to a similar failure of clientelist structures to provide economic security. The tendency to examine the insurgency in the South as an encroachment by a Christian majority on the land and customs of Moslems ignores the role of the old Moslem feudal elite in the process of economic deterioration. Co-opted into the national political and economic arena, this elite lost touch with its clientele in the pursuit of its increasingly national and personal interests. Rebel leaders have charged Moslem leaders with corruption and selling communal land for personal gain, while ignoring the needs of the poor Moslems.[76] The class as well as ethnic basis of this insurgency should not be ignored. While interethnic conflict suggests very little about a change in the viability of clientelist structures as such, emerging class conflict between lower- and upper-class Moslems emphasizes the susceptibility of insecure clients to the more explicit class appeals utilized by the Rebel forces.

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[73] For change in national turnout, substituting the percentage of the population either high-school or college-educated for social mobilization decreased the absolute value of the Beta to −.21. When the percentage of the population college-educated alone is substituted, the absolute value of the Beta is decreased even further to −.18.

[74] Figures on economic concentration are from the Bureau of Census and Statistics, *Survey of Households, Family Income and Expenditures: 1957* and Bureau of Census and Statistics, *Survey of Households, Family Income and Expenditures: 1971.*

[75] Clarence Abercrombie III (counting front page stories, letters to the editors and political cartoons dealing with the subject of revolution in a sample of *Manila Times* in 1951, 1960, and 1970) found a decline in the mean items per day dealing with revolution from 1951 to 1960 (from 2.62 to .31). Between 1960 and 1970 the mean rose to 7.03. Clarence Abercrombie III, "Vietnam and the Philippines: A Comparative Study of Revolution in Southeast Asia" (unpublished manuscript, Yale University, 1970).

Conclusion

Capital accumulation by the indigenous elite and foreign investment help cause sociodemographic changes, which in turn encourage rising political factionalism and declining voting participation. As national elites become more powerful through the accumulation of capital and expropriation of increasingly large portions of social income, local political machines are confronted by structural changes that lessen their integrative capacity. The weakening of machines is not, as in the United States, the result of anti-machine activities by a rapidly growing middle class; consequently, a significant proportion of the Philippine population (especially the lower classes) becomes more vulnerable to market forces which weak machines cannot assuage. Since alternative integrative mechanisms such as a mass-based party or a dominant, partially clientelist party (e.g., the PRI in Mexico) have yet to emerge on the Philippine scene, the increasing weakness of machines intensifies the potential for more violent forms of mass activity.

The widening gap between the masses and the national elite that arises from the commercialization of clientelist exchange relationships may ultimately provide the stimulus for the reintegration and participation of individuals through class- or mass-based parties. In the short run, however, the response to increased factionalism and social unrest appears more a recognition by sectors of the national elite that control through increasingly expensive elections is both unwieldly and inefficient. The declaration of martial law, representing a shift to more bureaucratic and coercive control, will no doubt provide short-run stability.[77]

There is certainly no guarantee, however, that the change to a more hierarchical clientelist structure will provide long-run political stability. While the curtailment of public elections places fewer strains on public resources, it is unclear whether the savings inherent in such a curtailment either are sufficient or will be used to provide security for broad masses of people. The vote provided a minimal amount of leverage, however small, which forced the distribution of limited resources to the masses. If such individuals are further ignored, the concentration of income will accelerate. Both Marcos's renewed encouragement of foreign investment and his effort to curb inflationary pressures by curtailing expensive elections suggest a posture particularly responsive to much of the middle and upper class.

Both continued capital concentration by the elite and encouragement of foreign investment are unlikely to rectify certain of the structural imbalances in the Philippine economy. The consequences of such imbalances include an increased gap in the standard of living of different population sectors and an increase in unemployment. The government's move to more direct methods of bureaucratic control further limits the ability of lower class groups to press demands. Neither the provision of economic security nor the generation of adequate wage employment through government efforts appears probable in the near future.

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[76] James P. Sterba, "Filipino Rebels Urge Ouster of Chiefs," *New York Times*, March 24, 1973, p. 3.

[77] Under martial law declared on September 23, 1972, the military and executive sectors are given extraordinary power.

An Organizational Approach to the Study of Political Culture in Marxist-Leninist Systems*

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Considerations of a Theoretical and Methodological Order

For at least some students of Marxist-Leninist regimes, political culture is becoming a salient analytic and research perspective.[1] There are several reasons for this development. First, there is the question of intellectual and theoretical stance. Those students of revolutionary change who define revolution as involving decisive but not absolute change tend to be sensitive to the role of cultural elements in contributing to the character of political systems. This understanding of revolutionary change is one which accepts the assertion that revolution involves "fundamental and even abrupt ruptures with the past in many areas of life . . . ," but that "for good or ill, revolutions never shut the door to the past."[2] Political culture also recommends itself to those who approach the phenomenon of social change by focusing on personality factors.

Second, recognition of diversity seems to be closely related to an appreciation of political culture as a research focus. It is interesting to note that during the period of Stalinism and the hegemony of the totalitarian thesis in the West, the major analytic approach to Marxist-Leninist regimes was a rather crude form of social-systems analysis.[3] Approaches of this order tended to discount or neglect the role of culture, largely because the relationship between regime and society was viewed simply as a pattern of domination-subordination. Similarly, during the reign of the "end of ideology" thesis in the West, structural-functional models dominated the analytic field. Only with the recognition of diversity does the analytic focus tend to expand and include cultural considerations.[4] The particular expression of diversity which has prompted consideration of foci that are complementary to a systems-approach has to do with the visible and systematic impact society has on the character, quality, and style of political life. One expression of this impact is the systematic discrepancy that occurs between statements about the ideal operation of a political system and its actual operation.

Third, political culture assumes greater salience as an analytic and research perspective as: (a) Marxist-Leninist regimes themselves redefine their task priorities from transformation and consolidation to modernization, (b) the inter-regime community of ruling parties loses its cohesiveness and power as a membership group and identity referent,[5] (c) the social composition of the parties

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* The author wishes to thank the Romanian Academy of Social and Political Science, IREX, the Institute of International Studies, and the Department of Political Science at the University of California, Berkeley.

[1] I have in mind Frederick Barghoorn's *Politics in the USSR* (Boston: Little, Brown & Co., 1966); Zbigniew Brzezinski and Samuel Huntington's chapter on "The Political System and the Individual," in *Political Power: USA/USSR* (New York: Viking Press, 1963); Richard Fagen's, *The Transformation of Political Culture in Cuba* (Stanford: Stanford University Press, 1969); Richard Solomon's, *Mao's Revolution and the Chinese Political Culture* (Berkeley: University of California Press, 1971); Lucian Pye's, *The Spirit of Chinese Politics* (Cambridge, Mass.: The M.I.T. Press, 1968); and Robert Tucker's "Culture, Political Culture, Communism" published in the *Newsletter on Comparative Studies of Communism*, 4 (May, 1971), 3–12.

[2] Benjamin I. Schwartz, "Continuity and Discontinuity in Contemporary China: Some Methodological Questions," *Bucknell Review*, 19 (Spring, 1971), 115–124, at p. 121.

[3] An exception to this was the study by Bauer, Inkeles, and Kluckhohn, *How the Soviet System Works* (New York: Vintage Books, 1956). In certain respects the argument to be presented in this article consciously builds on the work initiated in that volume.

[4] It is not surprising that the first formulations of the political culture approach preserved the existing and rather crude paradigmatic distinctions made between totalitarian and liberal-democratic regimes. Just as the "system theorists" responded to the changes after Stalin with the fallback concept of "rational totalitarianism," so students of political culture came up with the distinction of *pragmatic and ideological cultures.* (See Brzezinski and Huntington's, contrast of Soviet *ideology* and American *political beliefs;* and S. Verba, "Conclusion: Comparative Political Culture," in *Political Culture and Political Development*, ed., Lucian W. Pye and Sydney Verba (Princeton: Princeton University Press, 1965), pp. 544–550. There are a number of problems connected with this type of distinction: (a) it is ethnocentric and obstructs through definition a concern with the complexities of all cultures; (b) it is unnecessarily static (it might for example prove more useful to consider both the United States in 1776 and the Soviet Union in 1917 as ideological and both in 1972 as pragmatic); (c) it fails to sensitize the analyst adequately to comparison of different ideological cultures, i.e., the U.S. in 1776 and in 1948, the Soviet Union in 1917, 1928, and 1957.

[5] On this point see Kenneth Jowitt, "Political Integration and Political Identity in Eastern Europe," in *East Central Europe in the Seventies*, ed. Sylvia Sinanian, Istvan Deak, and Peter C. Ludz (New York: Praeger Publishers, 1972), pp. 180–184.

themselves changes, and (d) their posture towards the non-Marxist-Leninist world changes.

For a variety of reasons, then, political culture is currently becoming a salient analytic-research focus for certain students and a research-policy focus for certain Marxist-Leninist regimes.[6]

What is Political Culture and How to Study It. As conceived in this study political culture is a complementary perspective, not a substitute for other approaches or a single-factor explanation. This understanding of its role is related to the assumption that social reality is complex and that the function of theory is not to deny this complexity but to make it more intelligible. The definition of political culture used here ideally sensitizes the observer to certain relationships between regime and society and allows him to approach these relationships in a way that contributes to our understanding of the multifaceted reality characterizing these social systems.

Drawing an analogy with organizational theories, one can note at least three ways of studying Marxist-Leninist social systems. Both in the study of American factories and in the study of the Soviet Union the first major analytic statements were primarily structural. In the first case this took the form of Taylorism and in the second totalitarianism. In each instance the range of phenomena generated by structural relationships but not clearly consistent with them was perceived on the one hand by executives of Western business organizations as "anomalous," and on the other by Leninist elites as "bourgeois remnants." Analysts viewed such phenomena as either inconsequential and humorous or, when it came to Soviet type regimes as indicative that the efforts to change these societies had been superficial. Whether in an American factory or a Soviet-type regime, the informal, cultural, and covert aspects of the system were inadequately integrated into the theory that was used to explain the unit's character.

In organization theory the reaction against this structural, formal organization bias came with Mayo's human relations school. Mayo shifted the emphasis to an appreciation of human sentiment, of the factory culture, and of the significance of informal groupings. In recognizing the significance of the human factor, Mayo failed, however, to attend adequately to critical considerations such as the power structure of the factory. I would argue that within the field of comparative communism a development that parallels the Mayo school in organization theory is Solomon's work on China. Solomon also emphasizes the cultural dimension at the expense of the structural dimension. At least two major costs are associated with this failure. First, the terms of his analysis do not readily lend themselves to a discriminating comparative analysis of other Marxist-Leninist systems, largely because of his pursuit of the "unique" elements of Chinese political culture. Second, rather ironically much of what Solomon perceives as uniquely Chinese turns out to be something else. For example, the "uniquely" Chinese ambivalence toward authority and tendency to avoid conflict are, according to Crozier, distinctive and basic features of contemporary French political culture.[7] The outcome of this "exceptionalist" approach to political culture may very well create more obstacles in the way of comparative analysis and consequently in the way of establishing what is truly distinctive about individual Marxist-Leninist political structures and cultures.

A third approach to the study of organizations is that suggested by Michel Crozier. Crozier suggests that organizations should be analyzed in terms of the interaction of formal structure and informal relationships. In his terms these two elements should not be opposed. "They interpenetrate and complete each other. If one wants to understand them, one must study them together along with the system of power relationships that helps integrate them."[8] It is in this vein that I approach the study of political culture in Marxist-Leninist systems. Political culture will be studied in conjunction with political structure; structure and culture will be viewed as establishing mutual, though not necessarily equal, limits for one another. Approaching the concept of political culture from this perspective allows us to (a) take explicit account of the cognitive element of political culture and to see political culture largely as a response to a regime with a given organizational format,[9] (b) avoid limiting the notion of political culture to the level of individual psychology as most students of the concept have done, (c) perceive the systemic significance of what is currently seen as either anomalous or unique in certain Marxist-Leninist political cultures, and (d) generate hypotheses dealing with questions of conflict, change, and system identity. In short, the attempt here is to devise an analytic understanding of political culture that facilitates comparison of Marxist-Leninist regimes in light of the interaction between formal-structural and informal-cultural elements, thereby avoiding on the one hand the exceptionalist or uniqueness pitfall and on the other the tendency to assert through definition that structure alone is necessarily decisive in shaping the political character of Marxist-Leninist regimes.

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[6] In recent years the Romanian, Bulgarian, Chinese, Hungarian, and Albanian regimes have all directed their attention, in the form of Central Committee discussions and resolutions, to the realms of education, ideology, and culture. The Chinese case is clear to all in the form of the Great Proletarian Cultural Revolution; for the Romanians see *Scinteia* July 7, 13, November 4, 1971; for Bulgaria see the article by Marin V. Pundeff, "Bulgaria Under Zhivkov," and for Albania the article by Nicholas C. Pano, "Albania in the Sixties," both in *The Changing Face of Communism in Eastern Europe*, ed. Peter A. Toma (Tucson, Arizona: University of Arizona Press, 1970), pp. 89–121 and pp. 243–281. For Hungary see the comments of Barnabas Racz in "Political Changes in Hungary After the Soviet Invasion of Czechoslovakia," *Slavic Review*, 29 (December 1970), 638–639; and Bennett Kovrig, *The Hungarian People's Republic* (Baltimore: The Johns Hopkins Press, 1970), pp. 131–183.

[7] See Richard Solomon, *Mao's Revolution*, also Solomon, "Mao's Effort to Reintegrate the Chinese Polity: Problems of Authority and Conflict in Chinese Social Processes," in *Chinese Communist Politics in Action*, ed. A. Doak Barnett, (Seattle: University of Washington Press, 1969), pp. 271–365; and Michel Crozier, *The Bureaucratic Phenomenon* (Chicago: University of Chicago Press, 1964), chapters 8 and 9.

[8] Crozier, *Bureaucratic Phenomenon*, p. 164.

The relationship of political culture and political structure may be compared to the relationship that exists between the formal and informal organization of a factory. According to Barnard, "when formal organizations come into operation, they create and require informal organizations . . . they are interdependent aspects of the same phenomena—a society is structured by formal organizations, formal organizations are vitalized and conditioned by informal organization . . . there cannot be one without the other."[10] Barnard argues that informal organization may be "regarded as a shapeless mass of quite varied densities, the variations in density being a result . . . of formal purposes which bring [people] specially into contact for conscious joint accomplishments. . . . Thus there is an informal organization of a community, of a state."[11] It is the informal organization of the state that we shall refer to as political culture. More precisely, *political culture refers to the set of informal, adaptive postures—behavioral and attitudinal—that emerge in response to and interact with the set of formal definitions—ideological, policy, and institutional—that characterize a given level of society.*

Perceiving the different levels of society one can differentiate three types of political culture: elite, regime, and community political culture.[12] *Elite political culture* refers to the set of informal-adaptive (behavioral and attitudinal) postures that emerge as a response to and consequence of a given elite's identity-forming experiences. One might for example analyze the Romanian and Chinese elite political cultures in terms of the difference between a conspiratorial-prison experience and a guerrilla-partisan experience.

Regime political culture refers to the set of informal-adaptive (behavioral and attitudinal) postures that emerge in response to the institutional definition of social, economic, and political life. One would expect Marxist-Leninist regime cultures to vary according to whether a given regime structure was predicated on a command or semi-market principle and whether it emphasized the participatory or command component of democratic centralism.

Community political culture refers to the set of informal-adaptive (behavioral and attitudinal) postures that emerge in response to the historical relationships between regime and community. For example, one would expect very different community political cultures to emerge from a society in which organized religion stresses ritual and a mediated relation to God and one in which organized religion minimizes ritual and argues for a direct relation to God; similarly, the community political culture of a society in which state and society are related on the basis of an effectively institutionalized citizen-role should differ considerably from a society in which the mass of society has been historically excluded from political recognition and participation.

Having dealt with the basic orientations, concerns, and definitions, I should outline the basic analytic framework we shall employ to study political culture in Marxist-Leninist regimes. Reflecting the contention that the study of political culture is most fruitful in connection with the study of political structure, our two independent variables shall be structural. It is my thesis that all Marxist-Leninist regimes are oriented to certain core tasks that are crucial in shaping the organizational character of the regime and its relationship to society.[13] These tasks include: (1) *transformation*—the attempt to alter decisively or destroy values, structures, and behaviors which a revolutionary elite perceives as comprising or contributing to the actual or potential existence of alternative centers of political power;[14] (2) *consolidation*—the attempt to create the nucleus of a new political community in a setting that ideally prevents existing social forces from exercising any uncontrolled and undesired influence over the development and definition of the new community; and (3) *modernization*—the regime's attempt to develop more empirical and less dogmatic definitions of problems and policy, a formal, procedural approach rather than a substantive, arbitrary approach to the solution of problems, and an understanding of the executive function that stresses leadership rather than command competences.[15] Each of these tasks has organizational or structural corollaries. Transformation involves a confrontation between the regime and the "unreconstructed" society. Consolidation yields a structure of domination as the politically defeated but "hostile" society must be prevented from "contaminating" the nuclei of the new socialist society. Modernization, however, requires a rather significant redefinition of the relationship between regime and society from mutual hostility and avoidance to the regime's selective recognition and managed acceptance of society.[16] Depending on the core task, then, the regime will be structured differently both internally and in relation to society. It is to be expected therefore that (a) the character of political culture will vary with changes in tasks or task-mixes and (b) the introduction of a new task and regime structure will confront an already existing set (elite, regime, and community) of political cultures.

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[9] With regard to (a) see the very important article by Carole Pateman, "Political Culture, Political Structure and Political Change," in the *British Journal of Political Science*, 1 (July, 1971), 291-305.

[10] Chester Barnard, *The Functions of the Executive* (Cambridge, Mass.: Harvard University Press, 1962), p. 120.

[11] *Ibid.*, p. 115.

[12] To date most studies of political culture have been made at the community level. Recently Robert Dahl has commented that "although students of political culture call attention to differences between elite and mass political cultures . . . up to now they have paid a good deal less attention to the beliefs of political elites." See *Polyarchy* (New Haven: Yale University Press, 1971), p. 167. For a perceptive analysis of elite culture, see Robert Putnam's "Studying Elite Political Culture: The Case of Ideology," in the *American Political Science Review*, 65 (September, 1971), 651–682. To date, however, no one has argued for or analyzed a third arena, that of the regime in contrast to the elite and community levels. Among system oriented studies there is of course Easton's differentiation of the regime from the elite and community arenas. See David Easton, *A Systems Analysis of Political Life* (New York: John Wiley & Sons, Inc., 1965), pp. 154–247.

The second independent variable we shall work with is central ideological tenets. Briefly stated, all Marxist-Leninist regimes define themselves in terms of their commitment to certain ideological principles. We are interested in two: the dictatorship of the proletariat and democratic centralism. These are of particular significance precisely because they have explicit and direct structural implications for the organizational definition of the regime itself and for the organizational or structural relationship between regime and society. Core task and central ideological tenets with their structural correlates provide the boundary conditions for the organizational definition of the regime and its relations with society. It is assumed that they directly contribute to the type of political culture that develops at the elite, regime, and community levels. The framework, however, is still not complete. Organizations do not occupy vacuums but are defined in opposition to already existing organizations that have themselves generated corresponding cultures. Consequently, the process of specifying and acting on the organizational correlates of core tasks and ideological tenets is in part shaped by existing cultural postures. Furthermore, once a Marxist-Leninist regime has come to power and established itself organizationally, the political cultures it generates assume a certain integrity of their own and begin to reinforce the regime's structure. For both these reasons it is useful to consider political culture in the terms suggested by Harry Eckstein, namely, as an intervening variable.[17] In this formulation political culture provides some substance for the "black box" between the stimuli (independent variables) such as core task and central ideological tenet and the responses (dependent variables) such as the ease with which certain tasks are accomplished in contrast to others, the manner in which they are acted upon, and the ease or difficulty a regime has in redefining its core task (i.e., from consolidation to modernization).

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[13] An argument and analysis of changes in Marxist-Leninist systems based on different tasks is currently being written by the author with the title "Stages of Development in Marxist-Leninist Systems: A Comparative Analysis."

[14] See Kenneth Jowitt, *Revolutionary Breakthroughs and National Development: The Case of Romania, 1944–65* (Berkeley: University of California Press, 1971), pp. 8–9, 107–108.

[15] Leadership competences refer to an elite's ideological acceptance of the political membership credentials of the mass of society. Nonparty members are considered bona fide participants rather than unreconstructed inhabitants of the national community. Tenets such as "state of the whole people" and "party of the whole people" are illustrative of this shift (which of course can be reversed or modified). Structurally, the shift from command to leadership competences involves the shift from coercive to manipulative skills from a pattern of regime exclusivity to one of greater complementarity vis-à-vis society. Such a shift is illustrated by the decreased role of the secret police and the at least formal attempts to upgrade party activists in comparison to full time party functionaries (apparatchiks). Behaviorally, the shift in competence is reflected in new styles of leadership, greater emphasis on leadership visibility (i.e., Khrushchev vs. Stalin, Gierek vs. Gomulka, Dubcek vs. Novotny), direct contact with diverse social constituencies, and attempts to elicit social response to and confirmation of policy initiatives before the latter are given official status by the party.

[16] Selective recognition refers to the regime's willingness to expand power by allowing greater functional autonomy in various organizational settings; managed acceptance refers to the regime's organizational pre-emption of social strata with recognition aspirations or demands. An instance of managed acceptance in Romania is the recently formed Front for Socialist Unity, an instance of selective recognition is the emphasis on collegial decision making in factories.

[17] Harry Eckstein, "Memorandum to Participants in Conference on Political Culture and Communist Studies."

We must now ascertain whether the Marxist-Leninist international subsystem provides a recognizable and widespread structural format for dealing with the tasks of revolutionary transformation and political consolidation. Is there in the historical experience of some of these regimes a shared definition, a similar formal-organizational approach to the tasks of transformation and consolidation? And if there is, what are the elements that define the character of this format, what are the consequences of this regime format for the political culture of these systems, and what is the significance of the interaction of this culture with regime structure for political change?

System Building and Political Culture. Although there are fourteen Marxist-Leninist or Communist states, it is an historical fact that the Soviet Union's mode of development under Stalin has usually been the authoritative model, especially for most East European regimes. In addition, for regimes such as the Cuban, Chinese, and Yugoslav, the Stalinist model was either adopted and then later rejected or continues to be espoused as a model by certain elite members. The Stalinist mode of transformation and consolidation may be termed a system-building approach. It is this approach that has shaped the overall character of the Soviet regime and the majority of Marxist-Leninist regimes. And it is the legacy of this approach which these regimes must deal with in their current efforts to modernize.

System building contains three major related components. First, the dictatorship of the proletariat becomes the defining relationship for interactions both between regime and society and within the regime. One can specify three structurally significant aspects of this political principle: (a) the explicit and sustained policy of separating the elite and regime sectors from and opposing them to the rest of society, (b) the explicit utilization of widespread coercion and violence and the subordinate role of persuasion in the initial phases of "constructing socialism," (c) the tendency of Leninist parties to monopolize the public sector through their assumption of comprehensive and direct responsibility for social developments and the corresponding concentration of decision-making powers within the Party. What is involved here is the denial of any integrity to the public realm as distinct from the official realm.

Second, system-building regimes are concerned with the rapid development of their societies and the sustained mobilization of resources. What is structurally significant is that relations between regime and society are organized along elite designated priorities. In Marxist-Leninist terms this is a "commanding heights" strategy, an approach to development that I choose to label "revolutionary laissez faire." In such a system the emphasis is on controlling decisive points and sectors in a given social subsystem and on structuring a society on the basis of tacit trade-offs or exchanges. In return for performance in priority sectors, and with respect to priority items, members of society are "allowed" to manipulate nonpriority sectors for their private benefit.[18] A variety of causal factors underlie this type of development strategy; some, such as the inadequate number of cadres are historical while others, such as the concern with rate of development,[19] are more ideological. Of interest here, however, is the type of regime organization and regime-society relations produced by this "commanding heights" or priority strategy. As Simon has noted, any formal organization "will always differ from the organization as it actually operates in several important respects. First, there will be many omissions in it."[20] As Simon goes on to note, what is decisive is whether or not the formal organization sets limits to the informal relations that develop within it. Mobilization in a system-building regime means that while all social areas are provided with political limits, several areas do not receive the kind of attention that characterizes priority areas. To emphasize the distinction, nonpriority areas are controlled rather than transformed. In this sense they are "omitted" by the regime.

Third, system-building regimes are led by elites with "production mentalities." The Stalinist approach to socialist construction has been one of "walking on one leg,"—i.e., of defining success in constructing socialism as a function of political, economic, and social breakthroughs while viewing cultural transformation as basically a derivative accomplishment. The predominance of the production mentality in system-building regimes manifests itself in the following ways: (a) as an "island-hopping strategy" whose premise is that with the redefinition of the economic, political, and social domains the cultural domain can be effectively circumscribed, transformed, and the few "remnants" of bourgeois origin gradually

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[18] For a vivid description of this exchange relationship see Merle Fainsod, *Smolensk Under Soviet Rule* (New York: Vintage Books, 1963), pp. 85 and 151. While there are variations in the format over time and between countries the essentials of this relationship persist in many Soviet-type systems.

[19] See Kenneth Jowitt, "Time and Development Under Communism: The Case of the Soviet Union," in *Temporal Dimensions of Development Administration*, ed. Dwight Waldo (Durham, North Carolina: Duke University Press, 1970), pp. 233–264.

[20] Herbert Simon, *Administrative Behavior* (New York: The Macmillan Company, 1961), p. 148.

"mopped up,"; and (b) as an instrumental approach to culture and individuals that attends to such areas when they directly affect the regime's ability to act effectively on its priorities—social and economic change.

The significance of these major aspects of the system-building approach is twofold. First, a system-building approach to the tasks of transformation and consolidation *unintentionally* reinforces certain features of the existing (non-Leninist) community political culture that are considered negative by Marxist-Leninist parties. This reinforcement occurs largely because of the congruence between major elements in the ideological and organizational format of system-building regimes and major elements in the community political cultures of the various societies that have become Marxist-Leninist. Second, as these societies develop under Marxist-Leninist auspices, they characteristically generate challenges to existing structural and *cultural* definitions and postures at the elite, regime, and community levels.

One implication of this analysis is that the current challenges faced by most Soviet-type regimes are not simply threats to power and efficiency. Rather, the current crises in these systems involve the confrontation of different structural-cultural sets, and what is at stake is not only power and efficiency but the *character of political authority and social relations.* This dimension of current developments has not been adequately examined and the study of political culture can greatly enhance our understanding of the complexity and relatedness of developments within these regimes.

The Paradoxical Character of System Building

It is paradoxical that in their attempt to critically redefine society, Marxist-Leninist regimes simultaneously achieve basic, far-reaching, and decisive change in certain areas, allow for the maintenance of pre-revolutionary behavioral and attitudinal political postures in others, and unintentionally strengthen many traditional postures in what for the regime are often priority areas. How and why are the obvious questions. In order to answer them, to deal with the substantive as well as formal dimensions of political culture in Marxist-Leninist systems, an empirical referent is required. For that purpose we shall focus primarily on a single political culture—that of the Romanian Socialist Republic—and on the interaction between its regime and society. The analytic results of this exercise should ideally be a greater appreciation of the partially contradictory character of Romanian development since 1948 and a heightened appreciation of the character of development and developmental problems in other system-building regimes.

The Dictatorship of the Proletariat and Political Culture. We have already stated that as a structural or organizational principle the dictatorship of the proletariat involves the separation of the elite and regime sectors from the rest of society. This separation has occurred in Romania and has also occurred in similar regimes. Its rationale was twofold. Initially, it was predicated on the need to minimize regime commitments to a politically unreconstructed and hostile society. After the achievement of a decisive political breakthrough, the posture of avoidance was still maintained, adhering to the Soviet thesis that even after the political defeat of the old regime, its influence still threatens the construction and consolidation of new institutions, roles, and values.[21]

In at least two respects the separation or dichotomization of regime and society had a critical impact on the cultural orientation of both elite and non-elite segments of the Romanian population towards the political realm. The first is the reinforcement of a status ordering of regime and society. In an article signaling a campaign against *bakshish,* its author noted that such a practice properly belonged to a period in the past, to a time when "our (Romanian) society experienced domination by a stratum of rulers or boyars," and he asked how a practice associated with such a political structure could survive in a socialist society.[22] The answer to this question can be found in the structural similarity between the dictatorship of the proletariat and the historic relationship of state and society in Romania. During the period of the dictatorship of the proletariat,[23] the Romanian Workers Party was in effect a corporate, privileged, status group that asserted its identity and perquisites *in opposition to* the rest of society. Our purpose here is not to deny the significant social, economic, and cultural transformations initiated during the Gheorghiu-Dej period, but rather to focus on a feature of this period that has not been examined by Western scholars or understood by the Romanian party itself.

Given its goals of transformation and consolidation and its perception of how to achieve those goals, the Romanian elite created a political structure that in certain basic respects not only allowed for maintaining, but even reinforced, certain traditional political attitudes and behavior within both elite and non-elite members of the population. Just as *boiar* (nobleman) and subject had historically been mutually exclusive statuses, so cadre and citizen became mutually exclusive statuses rather than complementary roles. The structure of the regime made it possible for cadres to "consider a public office a source of personal income and those in their charge as serfs."[24] The *nomenclatura*[25] became the equivalent of the legal underpinnings of nobility, an organizational statement that a certain category of the population was (is) subject to special considerations. Thus, Mihai Gere, a member of the Romanian party's secretariat, has noted that "sometimes those (cadres) who damage (*aduc prejudicii*) our society (i.e., through corrupt behavior) are shifted to another position or even promoted instead of being removed from their work."[26] Such behavior is not unnoticed by those who are lower in the political and social hierarchy. In contrast to the ideal regime criterion of worth based on social discipline, responsibility, and political maturity, the popular understanding of what is actually the criterion of worth reflects the dichotomous status ordering of regime-society relations. Reporting on an organization's failure to punish an accountant for proved offenses, a correspondent for the party newspaper *Scinteia* concluded that the incident involved "the removal of our principle criteria for appreciating men and their deeds (and their replacement) with subjective criteria." What were these subjective criteria? It seems that the prosecution of offenders depended on whether or not the individuals involved were *cei cu functie sau fara functie* (those with official positions or those without), in short the effective criterion was membership in the *categorie de privilegiati* (privileged category).[27]

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[21] See J. V. Stalin, *The Foundations of Leninism* (Peking: Foreign Languages Press, 1965), pp. 42–43. "The bourgeoisie has its grounds for making attempts at restoration, because for a long time after its overthrow it remains stronger than the proletariat which has overthrown it." See also the passages from Lenin that Stalin quotes.

[22] *Scinteia,* July 23, 1971. The title of this article was, "Let us wipe out Bakshish: A practice incompatible with the ethical climate of our society."

[23] While the dictatorship of the proletariat is still an operative concept in Romania, the current regime's structure and policy in many respects depart from our working definition of this principle. When I use the term dictatorship of the proletariat I am referring primarily to the time period 1948–1964.

There is some evidence that the posture of non-elite members of society toward the political realm is based on this dichotomous structure of privileged versus nonprivileged. In another report about the use of public resources for private ends, the party correspondent noted that when the culprits were faced with their offenses without exception they adopted a lamentable position, admitting only to the crime of not occupying a prominent enough position (*suficient nivel*).[28] Another example of the understanding which elite members have of their position, perquisites, role, and relation to political life is found in an article that discusses the phenomenon of *razgindirii* or rethinking of a verdict.[29] Although the reasons behind this phenomenon vary, depending on the case, it appears that local party units often act much like other corporate-privileged groups in trying to protect "their own."

It is significant that the tendency to dichotomize elite and non-elite membership during the dictatorship of the proletariat has reinforced the political culture that existed prior to the rule of the Communist party, a political culture in which the elite sector was distinct in character and prerogative, not simply in role. We have examined some instances of this perspective at the elite level; let us now turn again to the mass level.

It is striking that system-building regimes based on the dictatorship of the proletariat have reinforced many pre-Leninist political cultural dispositions (attitudinal and behavioral) at the community level. By responding adaptively to the political reality of a status-based relationship between regime and society, many members of Romanian society continue to behave in partially traditional modes. For example, it is often necessary for the ordinary citizen to *stimula atentia*, or gain the attention of those with a regime position, a doctor, bureaucrat, or party cadre, and many individuals with such positions demand *mica atentia*, a little attention (i.e., money) before the performance of a public service.[30] The terms themselves are revealing. In a society based on explicit and authoritative status differences it is rational for non-elite members to act on the premises enforced by the elite sector. As one of the "mass," the ordinary citizen must distinguish himself from other non-elite subjects if he is to get a response; he must "stimulate" the attention of a superior. Doing this is simultaneously a means of gaining the superior's attention, recognizing the superior's elite status, and often a means of decreasing the uncertainty and anxiety of the encounter.[31]

The widespread phenomenon of *pile* or "pull," "connections," may also be viewed as a continuation of pre-Leninist political culture and as an adaptive-informal response to a political structure that devalues adherence to legal-rational rules. The dictatorship of the proletariat is, after all, defined by Lenin as the "rule—unrestricted by law and based on force—of the proletariat over the bourgeoisie. . . . "[32] *Pile* at the community level and in the interaction between community and regime is the informal, covert equivalent of the *nomenclatura* at the official level; both are mechanisms for dealing with a structure based on status, not on rule considerations.

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[24] *Scinteia,* July 23, 1971.

[25] The *nomenclatura* is a party listing of politically critical posts and the personnel eligible for them.

[26] *Scinteia,* November 5, 1971. See in this connection the article by Steven J. Staats, "Corruption in the Soviet System," *Problems of Communism,* 21 (Jan.-Feb. 1972), 40–48.

[27] *Scinteia,* November 13, 1971.

[28] *Scinteia,* November 30, 1971.

[29] *Scinteia,* November 10, 1971.

[30] *Scinteia,* November 17, 1971.

[31] In this connection see the arguments presented by Robert Price in "The Social Basis of Administrative Behavior in a Transitional Polity: The Case of Ghana" (doctoral dissertation, University of California, Berkeley, 1971), particularly chapters 1 and 5.

The most important point, then, is that from the perspective of the structural dichotomization it generated, the dictatorship of the proletariat supported certain pre-Leninist political cultural dispositions at both the elite and non-elite level of the new Marxist-Leninist society. Ironically, these dispositions are essentially antithetical to the appearance of a Marxist-Leninist nation-state in which legal-rational (though not necessarily liberal-democratic) norms and institutions have a major role.[33]

It was asserted earlier that in at least two ways the dichotomous and oppositional relationship of regime and society contributes to the maintenance of a pre-Leninist political culture. We have examined the first, the reinforcement of a status ordering of regime and society. The second pertains to the basic cultural ethos of a peasant-traditional society, an ethos captured in Foster's brilliant notion of "limited good." According to Foster,

> broad areas of peasant behavior are patterned in such fashion as to suggest that peasants view their social, economic, and natural universes—their total environment—as one in which all of the desired things in life such as . . . friendship . . . honor, respect, status, power, influence, security, and safety, exist in finite *quantity* and *are always in short supply* . . . hence it follows that *an individual or a family can improve a position only at the expense of others*[34] [emphasis added].

In short, peasants tend to define relationships in zero-sum terms and to conceive of values rigidly and concretely. What is so striking in this instance is the high degree of congruence between the traditional cultural ethos of a peasant society and several Leninist-Stalinist precepts. Just as for the peasant, social relationships are zero-sum, so for Lenin and Stalin many of the relationships between the Party and society are also zero-sum. Examples include the basic political question raised by Leninists, *kto-Kovo* or who-whom—which political force will triumph? The outcome is seen as precluding long-term sharing of power: that is, either the bourgeoisie or the proletariat gains power—not both.

Similarly, the rigid Stalinist conception of the Party's leading role and of the unitary nature of leadership has made those whose political identities were formed during the Stalinist period fearful that a shift to collegial leadership would "dilute" the Party's power, place, and effectiveness. Finally, there is the Leninist-Stalinist aversion to faction; differences of opinion are often seen as a threat, as "weakening" the regime's unity.[35] It is remarkable how formally compatible these Leninist-Stalinist "limited good" interpretations of basic structural considerations have been with the "limited good" interpretations of value and structural considerations in a peasant society. Consequently, it is not surprising that the "limited good" conception of the dictatorship of the proletariat held by Lenin, Stalin, and their emulators in Eastern Europe, could enhance the traditional cultural postures of a peasant society and in turn be reinforced by them. As in the case of the hierarchical status relationship between regime and society, a major link between the regime's conception and definition of its organizational character and the society's response to it was that society's social composition. Romania was approximately eighty per cent peasant in the late 1940s. Peasants not only provided the regime with its major constituency but also with a large proportion of its membership. If one realizes that the urban population of Romania, particularly the industrial population, was basically peasants recently turned workers, one can better appreciate how easy it was for the large numbers of party cadres drawn from the ranks of the peasantry or peasant-worker stratum to understand, accept, and operate with the Party's "limited good" conception of its own identity, tasks, and prerogatives.[36] Here was a classic instance of the Party partially defeating its own purposes and not recognizing it. In its effort to recruit and socialize cadres, to create "new men," its very ideological images and organizational definitions acted in part to reinforce unintentionally the traditional view of the relationship between regime and society, a view that stressed the antagonistic, zero-sum character of that relationship. Once again the Party's attempts at transformation and consolidation produced a mixed set of outcomes.

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[32] Stalin, *Foundations of Leninism*, pp. 45–46.

[33] One can of course argue that a Marxist-Leninist regime cannot simultaneously maintain its political identity and incorporate legal-rational norms and institutions. In fact this assertion is one of the "proverbs" in the field of Communist studies. It may be a correct one but has not been demonstrated convincingly. Arguments about the incompatibility of Marxism-Leninism and Modernity often fail to recognize that all modern political communities are to varying degrees characterized by a conflict between several of the imperatives associated with modernity and the imperatives associated with the particular ideology of a given regime. Viewed in this light, Liberal as well as Leninist communities are characterized by persistent conflict between ideological commitments and commitment to a legal-rational ethos. One can identify one ideology (i.e., liberalism) with a particular ethos (modernity) and then conclude by definition that because Leninists aren't liberal they cannot be modern, or can only be modern if they become liberal; but it would be more productive analytically (a) to establish the range and type of conflicts that are typical of modern or modernizing societies with different ideologies, (b) to scrutinize more critically the varying dimensions of modernity and then attempt to establish what aspects of modernity different regimes are likely to stress and what institutional definitions of modernity they are likely to posit.

[34] George M. Foster, "Peasant Society and the Image of Limited Good," *American Anthropologist,* 65 (April, 1965), 293–315, see 296–297.

[35] One index of the modification of a Marxist-Leninist regime's character is its approach to internal party criticism. It is significant that since assuming power Ceausescu has repeatedly stated that criticism is not tantamount to disloyalty; however, the resistance to implementing this nondogmatic (though authoritarian) and nonlimited good notion of criticism is still widespread. See Nicolae Ceausescu, "Adunarea activului de partid al municipiului Bucuresti," in *Romania Pe Drumul Desavirsirii Construcţiei Socialiste,* vol. 3 (Bucuresti: Editura Politica, 1969), pp. 199–200.

Along with the separation or dichotomization of regime and society, an integral part of the dictatorship of the proletariat in Romania and elsewhere has been the regime's explicit and persistent use of coercion in relations between regime and society and within the regime sector itself. This coercion had a major impact on the type of political culture that has developed within these systems.

In a very perceptive article Emanuel Turczynski has pointed out the chasm that historically existed between state and society in Romania. The absence of an indigenous middle class, the "village oriented cultural and social structure," and the experience with foreign domination all contributed to a sharp difference between the private and social realms on the one hand and the public and official realms on the other. Turczynski suggests that this state of affairs was not conducive to the appearance of any effective popular identification with the state.[37] The coercive character of the dictatorship of the proletariat in certain respects (though not all) reinforced this separation of the private and public sectors of society, the antagonism between the two, and the perception of the public arena as a threatening, hostile, and alien sector. The Marxist-Leninist regime's employment of violence resulted in significant sectors of the population adopting a split posture, one of public compliance on the one hand and private avoidance, skepticism, or rejection on the other. A syndrome of this order is perfectly consistent with social psychological studies that have demonstrated, "if threatened punishment [for noncompliance] is very high, then compliance [public dependent influence] will occur without private acceptance. There is no dissonance since the high . . . punishment provides sufficient basis for the inconsistency between private belief or attitude and public behavior.[38]

And, in fact, it seems that the manner in which and the extent to which most Marxist-Leninist regimes employed coercion reinforced a cultural disposition that was highly calculative toward the political realm, a posture of complying publicly in order to protect and preserve one's private domain. Scalapino, for example, in his recent comprehensive study of North Korea notes that "a number of persons appear to adjust to the demands of an authoritarian state by developing a series of external responses including an intricate series of participatory actions that differ from and provide some protection for internal or private responses."[39] Unlike third-world regimes in which coercion and rewards are too low to produce compliance with modern norms, and unlike regimes such as the Chinese, which appear to have employed coercion more judiciously,[40] in their formative stages system-building regimes employed too much coercion and thereby reinforced what in certain respects might be termed a ghetto political culture. Before pursuing this point let me emphasize that the dictatorship of the proletariat in Romania and elsewhere has done more than unintentionally reinforce and enhance pre-Leninist political cultural postures that were and remain antithetical to several of a Marxist-Leninist regime's basic goals. This will become clearer in the final section of the article. One cannot, however, understand the current crises facing these regimes without analyzing the ways in which they did reinforce and enhance such postures.

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[36] See the recent article by Zygmunt Bauman, "Social Dissent in the East European Political System," in the *European Journal of Sociology*, 11 (1971), 25–52, for a much needed and very useful discussion of the importance of the "peasant factor" in the development of Marxist-Leninist regimes.

[37] Emanuel Turczynski, "The Background of Romanian Fascism," in *Native Fascism in the Successor States, 1918–1945*, ed. Peter F. Sugar (Santa Barbara, Calif.: ABC Clio Inc.), p. 109 and *passim*.

[38] Barry E. Collins and Bertram H. Raven, "Group Structure: Attraction, Coalitions, Communication, and Power," in *The Handbook of Social Psychology*, vol. 4, ed., Gardner Lindzey and Elliot Aronson (Reading, Mass.: Addison-Wesley Publishing Co., 1969), p. 181.

[39] Robert Scalapino, and Chong-Sik Lee, *Communism in Korea*, vol. 2 (Berkeley: University of California Press, 1972), p. 847.

[40] In this connection see Thomas Bernstein, "Leadership and Mass Mobilization in the Soviet and Chinese Collectivisation Campaigns of 1929–1930 and 1955–1956: A Comparison," *The China Quarterly*, 31 (July–September 1967), 1–47.

Historically, the Romanian community political culture appears to have stressed the antagonistic relationship between public and private spheres. Experience with the basically coercive, extractive, and status-based rule of Turks, Russians, Austrians, Hungarians, native boiars, and native regimes stimulated the development of what often resembled the political culture of a ghetto. As in the past and as in a ghetto, under the dictatorship of the proletariat the regime or official sphere represented "trouble" in being identified as the locus of demands and sanctions rather than of political support and recognition. The result was the adoption of a calculative, instrumental, and often dissimulative approach to the official or public sphere of life. In this respect the similarity to ghetto culture is quite clear. Rather than identify with regime values, norms, and goals, such as disciplined work, the community developed a set of postures that in many instances were antithetical to the regime's expectations. Certainly from the Romanian party's point of view its social investigations have too often discovered individuals whose highest value is *trai usor* (the easy life) and who place too high a value on *timp liber* (free time).[41] And yet, each of these postures is an instance of an informal-adaptive response to a political structure that has until very recently related to society in extractive and coercive terms, that failed to and in fact feared to identify with its society—a fear that may well be rational during the initial stages of a revolution but that produces mixed, unanticipated, and undesired consequences for a revolutionary elite.

Estrangement is the quality that unites the various postures we have referred to—the calculative, instrumental approach to social tasks and official priorities, the emphasis on free time and the easy life. If, as at least one student of the topic has argued, alienation "refers to an enduring sense of estrangement from salient objects in specified contexts," and denotes "the absence of basic attachments to and identifications with salient objects in a particular context,"[42] then it is plausible to conclude that during the period of the dictatorship of the proletariat (approximately 1948–1964) the Romanian regime partially reinforced the alienated posture of large segments of the Romanian population. The use of coercion and the easily identifiable source of that coercion—easy precisely because of the dichotomy the Party created between itself and society and because of the concentration of decision-making power within its boundaries—in certain respects sustained and increased the community distrust of things official, public, and political, and created a major obstacle for the regime in its current attempts to create a more positive relationship with society.

The third feature of the dictatorship of the proletariat component of system building is the marked tendency of such a regime to fuse the official or elite sector with the public sectors of society.

In one of Petru Dumitriu's novels, *Incognito*, there is a passage describing a party member's reaction to the criticism made of one of the higher party cadres at a social gathering. In response to the news of this occurrence the party member exclaimed, "I can't understand *why it has been allowed*."[43] Phrased more formally, system-building regimes have tended to monopolize political discretion, initiative, and responsibility. Events do not occur, decisions are not made, and facts are often not recognized as facts until they are *allowed* to occur or to be recognized.

For such regimes the absence of a public domain with an integrity separate from that of the official-elite domain and the private-social domain has several consequences: Lacking any substantial control over their own definition and role, public institutions in such a regime often fail to elicit identification from those who work within them. Given the absence of any autonomous public domain, the Party's demands that individuals identify with the public interest rather than with their private interest are unrealistic; the political reality is that only two discernible interests exist: the regime's and the individual's. Without a differentiated public domain capable of eliciting identification from large sectors of society and with a regime that minimizes its commitments to society, it is not surprising to find so many instances of theft, pilfering, and lack of involvement with one's work. One could argue that given the fusion of official and public sectors and the low popular identification with these sectors, the public domain becomes the equivalent of a "treasure find" in a traditional peasant society. In such societies based on the notion of "limited good" one can only afford to appear more prosperous by attributing one's new wealth to a source outside the local community—simply because in a society based on an ethos in which resources are fixed, if one person adds to his well-being, it means someone else has become less well-off. Now, given the political structure of a system-building regime, specifically, its monopoly of all nonprivate domains, the public domain (i.e., the commercial sector of the economy) becomes for many the equivalent of a "treasure find." Prevented from effectively identifying as participants by the structure of the regime itself and often left to their own devices to supplement their income, the incidence of *ciupeli marunte* (petty pilfering) is understandable. While regimes such as the Romanian still do not recognize the causes of such behavior, they seem to be aware of the ethos that informs such behavior. One indication is the idiom that is employed when such practices are criticized. In a recent attack on theft, pilfering, and lack of work discipline the author of an article in *Scinteia* argued that "illicit gains are not *gifts of chance* [i.e., a 'treasure find'], they are the fruit of repeated violations of work discipline and of the law."[44] Apparently certain elite members are sensitive to the persistence of semi-traditional orientations to the public domain.

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[41] See the article by Alexandru Ivasiuc in *Scinteia*, December 30, 1971, "Republica Muncii" (The Republic of Work); see also "Distractii inofensive care afecteaza grav destinele unor tineri," *Scinteia*, September 18, 1971; "Parazitul cu blazon," *Scinteia*, September 13, 1971; and "Acolo unde educatia si echitatea erau doar teme de sedinta . . . ," *Scinteia*, December 14, 1971.

[42] See Jack Citrin, "Political Disaffection in America, 1958–1968," (doctoral dissertation, University of California, Berkeley, 1972), pp. 66 and 73.

[43] Petru Dumitriu, *Incognito* (New York: The Macmillan Company, 1964), p. 53. Stress in the original.

A second consequence of the regime's monopoly of the public domain, of its concentration of decision-making power, has been that those who have positions within the regime but not necessarily elite positions within the Party tend to separate the status and role components of their position. In his recent work on the social basis of administrative behavior in Ghana, Price discovered that Ghanaian bureaucrats tend to separate the status or prestige component from the role or behavioral component of their positions. His explanation is that the role demands of positions in a modern organization are incongruent with fundamental elements of traditional African culture and that consequently one would expect this separation to persist as long as those traditionally prominent elements retain their importance.[45] In Marxist-Leninist countries, particularly in the rural areas, one can find a tendency to separate the prestige from the behavioral component of official positions, and it is often related to the Party's organizational weakness in such areas, the low level of ideological sophistication among the cadres, and the claims made on them by families and friends. Additional and distinctive factors, however, are at work in Marxist-Leninist systems that lead officials to separate the prestige and role components of their positions. Given the sharp distinction made in the dictatorship of the proletariat between elite and non-elite positions, there is a corresponding tendency to emphasize the prestige component of one's position. The fact that historically the position of official was seen in terms of status and power rather than functions and services has only made the bias towards the prestige component easier to act on.

There is yet another reason, however, for the failure of many officials to act consistently with the formal definition of their position. In a system-building regime based on the dictatorship of the proletariat, where the Party places a premium on exercising a monopoly over politically relevant behavior and where the definition of politically relevant behavior is deliberately diffuse rather than specific, an official who bases his behavior primarily on the formal role prescription of his position is in effect posing a challenge to that monopoly. For to base official behavior on the formal role prescriptions of a given position is to have as one's main referent a universalistic, impersonal set of action determinants. In the framework of the dictatorship of the proletariat, however, the Party does not accept the value of such impersonality or neutrality. In fact, the dictatorship of the proletariat is defined explicitly in substantive, not procedural terms. The discrepancy between the concern of regime officials with status and their frequently poor performance in terms of the formal definition of their role is not primarily due, as in third world settings, to the conflict and incongruence between modern role prescriptions (i.e., treat all clients equally) and traditional cultural elements (i.e., favor one's relatives) but to the political penalities that often accompany attention to role-prescriptions rather than to political cues. Because regimes interested in transforming an existing society and consolidating the nuclei of a new one are committed to controlling and defining the premises and substance of politically relevant behavior, any such behavior with a non-party premise as its major referent is a potential challenge. What is routine behavior for an official in a non-revolutionary context becomes an illegitimate initiative for one in a revolutionary setting. The consequence is that in such a regime context one discovers that officials are reluctant to take the formal definitions of their positions seriously. In light of the structure of the regime and its political-ideological orientation, this is a quite rational stance to adopt. The net result of all of this is that in a third-world peasant society there are *societal* forces working against the integration of the status and role components of a position, and in a peasant society subject to the dictatorship of the proletariat there are additional *regime* forces working in the same direction. Both forces work against an effective appreciation of what role- and rule-based behavior implies and support a political culture that views politically relevant behavior as dependent on permission by and familiarity with authoritative figures.

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[44] *Scinteia*, September 15, 1971.

[45] Price, "The Social Basis of Administrative Behavior," pp. 72–86.

A final consequence of the regime's monopoly of the public domain is the premium that is often placed on withholding critical information from the purview of decision makers. Within system-building regimes a particularly large number of events are hushed up (*musamalizat*). It was argued earlier that intra-elite mutual protective behavior is partly a function of the Party's corporate group attitude of protecting "its own." However, mutual protection also occurs because the Party assumes responsibility for so much. Given the scope of the Party's responsibility, it is not surprising that cadres who were unaware of misdeeds in their sectors often work to prevent such deeds from becoming widely known since exposure will result in their own punishment. Such behavior could be documented throughout Eastern Europe and the Soviet Union by a random search of party newspapers. In his study of North Korea, Scalapino has found a classic example of this phenomenon. There it was brought to the attention of a provincial secretary that a "half-won business" was operating. He was unfamiliar with this term and it was explained that this referred to prostitution. The secretary and several other cadres carried out an investigation, identified the individuals involved and concluded, "This was a serious problem for us, because if we reported this matter to higher offices of the Party, we would all be subject to disciplinary treatment, including the *kun* Party secretary and myself, because we had allowed such a thing to develop. It was the season of the annual committee inspection and . . . we did everything to prevent them from reaching the hamlet . . . Later (we) called on those implicated . . . We could not implicate them in any case, however, or we would have been deeply involved."[46] Paradoxically, then, the Party's assumption of total responsibility for the public domain contributes in certain instances to collusion between elite and nonelite actors regarding illegal behavior, and to the withholding of important information since its release may result in negative sanctions against regime members. The consequence of all of this is the development of a regime political culture that is composed in part of dissimulation and complicity. Monopolization of the public domain, the fusing of elite and public spheres, and the corresponding denial of institutional integrity to public institutions (economic, administrative, social, and political) has in some ways reinforced the traditional political culture which viewed all relationships in terms of their hierarchical rather than their complementary character and which defined responsibility as the avoidance of public initiative.

Mobilization and Political Culture. A second defining feature of all Marxist-Leninist regimes is their preoccupation with the rate of social development. Communist governments emphasize the fulfillment of potentiality. "Structurally, the political community is the means of translating potentiality into some sort of reality."[47]

A major feature of this mobilization ethos and the corresponding relationship between regime and society are the targets and priorities that are authoritatively established by the elite and that are used to gauge both the regime's and society's performance. Berliner has analyzed the character and consequences of this political structure in the Soviet regime's economic sectors. Berliner related the existence of extremely ambitious targets, tight time constraints, scarce resources, and authoritative sanctions ("the plan is law") to a series of informal-adaptive responses that included searching for a safety factor, hoarding, and dissimulation.[48] Though to date no study of this magnitude has been undertaken for other system-building regimes, one could in fact find similar informal-adaptive responses in most East European regimes.[49] What is most significant about Berliner's findings is that they can be generalized to other sectors of society.

In their effort to create an ethos of expanding good—an ethos that views resources and values as complementary and dynamic rather than mutually exclusive and static—mobilization regimes often unintentionally generate an understanding of resources and values based on "limited good." For example, just as in traditional peasant societies families jealously guard their own interests rather than cooperate extensively, so factory managers in a mobilization system are judged by their subordinates according to how successfully they defend their factory's interest *in opposition to* that of the ministry or trust. As one of Berliner's respondents commented: "We look to the director to promote our interests, to push through for us at the ministry and to defend the interests of the plant."[50] And just as a factory in such a system is always on the lookout for a safety factor and prone to hoard resources, so individuals in such a system tend to hoard "connections." Connections are as jealously guarded by individuals and families as material resources are by factories, and for similar reasons. Both the ethos and the structure of a mobilization regime are antagonistic to the routinization of tasks and the authority of impersonal-stable rule. Striving to insure that the potential of the society is completely realized, the regime defines targets, goals, and priorities substantively rather than procedurally, and rapidly redefines them. Thus a premium is placed on informal adaptive mechanisms (signified by the Russian term "blat"[51] and the Romanian term "pile" or pull) that allow for some stability and certainty in response to what is often perceived as an arbitrary and threatening regime. Both *blat* and *pile* refer to relationships and dispositions that obstruct the development of a political culture based on overt, public, cooperative, and rule-based relationships. Instead they reinforce the traditional community and regime political cultures with their stress on covert, personalized, hierarchical relationships involving complicity rather than public agreements.[52]

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[46] Scalapino, *Communism in Korea*, vol. 2, p. 777.

[47] David E. Apter, *The Politics of Modernization* (Chicago: University of Chicago Press, 1965), p. 31.

[48] Joseph S. Berliner, *Factory and Manager in the USSR* (Cambridge, Mass.: Harvard University Press, 1957), pp. 318–329 and *passim*.

[49] For a recent analysis which points out differences between the Romanian economic structure and the Soviet economic structure see David Granick, "The Orthodox Model of the Soviet-Type Firm Versus Romanian Experience," (International Development Research Center Working Paper, Indiana University, 1972).

[50] Berliner, p. 230.

The ethos and structure of mobilization regimes influence the political culture of a society in a number of other ways. Along with the generation and reinforcement of a "limited good" perspective, mobilization regimes often unintentionally reinforce traditional conceptions of time. In an interesting book written in 1904, an insightful observer of Romanian society noted that Romanians at all levels of society worked in an "unregulated rhythm," and oscillated between periods of hard intense work and periods of prolonged inactivity. In Radulescu-Motru's words, "We [Romanians] are capable of any virtue so long as it doesn't require too great a persistence on our part."[53] In 1972, Nicolae Ceausescu, leader of a Marxist-Leninist regime that has been in power for twenty-five years, noted that policies initiated several years ago (at the National Party Conference in December 1967), "while leading to positive results are not being carried out consistently." He continued, "As a matter of fact, comrades, this [lack of measured persistence] is a habit of ours. We begin an action with a great deal of noise, obtain a series of results, but after a period of time we lose sight of segments of the policy . . . and we interest ourselves in other things."[54]

One could conclude that the changes in regime in Romania have been superficial and that although the form changes, the substance (i.e., cultural dispositions) remains. Such an argument would itself be superficial. The reality is more complex and intriguing as the character of the industrial production process in system-building regimes with a mobilization structure illustrates. A major characteristic of production in such a setting is the phenomenon of "storming," an uneven attention to tasks, varying intensity of job performance, and "campaign"-like resolution of tasks. These characteristics are a function of *the regime structure itself* and are highly congruent with traditional behavioral and attitudinal postures toward time and antithetical to modern notions of task performance based on scheduled —i.e., measured, continuous—behavior. In this instance one has an example of a regime concerned with and in some major respects succeeding in transforming the character of an entire social system and yet actually reinforcing certain cultural postures that are antithetical to its basic goal.

A third and highly significant consequence of the mobilization structure and ethos of a system-building regime is its connection with dissimulation as a modal behavioral posture in Marxist-Leninist societies. Berliner discovered that managers who wish to maintain their positions systematically engage in the practice of dissimulation. In Russian the term is *ochkovtiratelstvo* which literally refers to cheating at card playing by altering the character of a card. "In economic activity, it [ochkovtiratelstvo] signifies the simulation of a successful performance by some *deceptive manipulation*."[55] As with the case of hoarding, we should like to suggest that dissimulation is a *general* feature of a society and system based on a mobilization ethos and structure.

Several things can be said about dissimulation as a form of behavior. To begin with, it should be differentiated from formalism, which refers to strict adherence to external forms, usually to inadequate and inflexible understanding of a role's meaning. As Price has suggested, formalism is often fostered by the conflict between established societal norms and official or regime norms.[56] Dissimulation refers to something related but quite distinct. What is involved is not simply rigid adherence to rules in order to reduce uncertainty, but also "deceptive manipulation," the conscious adoption of false appearances. This posture does not arise as much from the conflict between established traditional norms and newly initiated modern norms as from (a) the perceived need to deflect the regime's attention from possible or real under-fulfillment of tasks, and (b) from the general desire to minimize the scope of regime interference in one's private and social life. Dissimulation is based on fear and avoidance. It is an adaptive response to a regime that, in its concern for realizing the entire society's potential, periodically attempts to penetrate most areas within the society. As a political posture, dissimulation is hardly something foreign to the experience of the Romanian population, which in its history has had to develop adaptive-informal postures in order to maintain a private zone of unofficial, covert attitudes and practices while publicly complying with the coercive, extractive demands of alien rulers. The demands of a system-building regime have reinforced the Romanian population's tendency to separate sharply the public and private arena, and to perceive them in antagonistic rather than complementary terms. Dissimulation is the posture, response, and strategy that integrates the two arenas. In such a society one often finds a highly calculative and selective recognition of regime authority. This stance takes the form not so much of political opposition as a strong antipolitical privatism in which family and personal interests are emphasized at the expense of regime and societal interests.

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[51] According to Berliner, *blat* "implies the use of personal influence for obtaining certain favors to which a firm or individual is not legally or formally entitled," p. 182.

[52] A number of phrases in contemporary Romanian parlance refer to different forms of complicity. Recently in *Scinteia* an article dealing with certain illegal practices in a factory found "un spirit de 'amabila,' ingaduinto reciproca, de 'binevoitoare' toleranta, de 'delicata' inchidere a ochilor" (November 30, 1971). All of these phrases refer to semilegal or illegal covert cooperation.

[53] C. Radulescu-Motru, *Cultura Romana si Politicianismul* (Bucuresti: Cultura Nationala, 1904), p. 56.

[54] *Scinteia,* February 20, 1972, p. 2.

[55] Berliner, *Factory and Manager in the USSR,* p. 114.

[56] Price, "The Social Basis of Administrative Behavior," pp. 72–86.

Finally, system-building regimes with a mobilization ethos and structure are based on a "commanding heights" principle that entails the attempted *transformation* of certain areas, roles, and values and the attempted *control* of nonpriority areas, roles, and values. What differentiates a revolutionary Marxist-Leninist regime from a radical nationalist regime is *not* the Marxist-Leninist's attempt substantively and simultaneously to transform all areas of a social system but its determination (a) to control and/or transform critical points at *all levels* of the social system and (b) to prevent existing social forces from defining their resentment and hostility in terms of political opposition at any point or level of the society.

This pattern of transformation and control, of revolutionary *laissez faire*, contributes in the short run at least to a highly heterogeneous and often incompatible set of political cultures. One can distinguish at least three types of areas in system-building regimes: (a) poor rural areas experiencing more political control than cultural transformation. In such areas traditional-peasant political culture probably persists to a great extent. A classic illustration is the investigation of a Romanian cooperative in which a number of illegal practices were discovered. Asked about why such practices were not criticized in the assembly, one member noted that "here in this cooperative we are neighbors and relatives. There are too many relatives in the cooperative, and it isn't wise to get on the wrong side of them."[57] In nonpriority arenas, such as the rural, given the scarcity of their resources (i.e., time, cadres, goods) system-building regimes have emphasized political control more than cultural transformation. (b) Another area consists of modern-industrial projects in urban settings. The likelihood is that in these instances there has been a greater transformation of general and political culture reflecting the work-setting, nature of work, and the correlates of an industrial milieu. (c) There is also an intermediate area that combines modern forms and traditional practices.[58] The commercial and service sectors in system-building regimes provide a good deal of evidence for the argument that there is no simple demarcation between traditional and modern political cultures, between conceptions of authority based on hierarchical, personalized, and command considerations, and conceptions of authority based on complementary, rule, and leadership considerations. As the Romanian regime itself has noted the commercial and service sectors in urban as well as rural areas are major sites for the practice of *bakshish.* It is, in fact, noted that many individuals attempt to get jobs in these areas precisely because they are so lucrative. The incidence of corruption (or "behavior which deviates from the formal duties of a public role because of private-regarding [personal, close family, private clique] pecuniary or status gains . . . ")[59] in the service and commercial sectors provides another good example of regime and societal forces combining to produce behavior that is antithetical to official values. It is not accidental that the commercial and service realms are characterized by widespread corruption. In a traditional society services are not rendered on the basis of impersonal rules but on the basis of personal recognition; in the commercial and service realms (in contrast to the industrial realm) personal encounter is the basis of exchange and productivity. If, in addition one realizes that these are low priority realms for system-building regimes, it is not surprising that corruption is so pervasive within them. Actors within these realms, while complying with the externally imposed quotas or targets, concentrate on maximizing their personal, private interests much as cooperative farmers relate organizational tasks to their interest in their private plots. This propensity to deal instrumentally and calculatively with official responsibilities and ingeniously with personal interests characterizes many actors in all realms of the society but given the low-priority status of the commercial and service sectors and their personal-encounter character, this propensity is less constrained here and consequently flourishes.

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[57] *Scinteia,* November 22, 1971.

[58] In this "area" one can find many instances of what Fred Riggs has termed "prismatic behavior." See *Administration in Developing Countries* (Boston: Houghton Mifflin Co., 1964).

[59] J. S. Nye, "Corruption and Political Development: A Cost-Benefit Analysis," *American Political Science Review,* 61 (June, 1967), 417–427, see p. 419.

Production-Mentality and Political Culture. The third and final aspect of a system-building regime to be considered here is its conception of tasks. We are particularly interested in the way in which system-building regimes approach the task of cultural transformation, a task that all Marxist-Leninist regimes assert as a major goal. System-building regimes tend to see the resolution of this task in derivative terms: it is assumed that as the consequence of constructing a new political, economic, and social system a new and congruent culture will emerge. Marxist-Leninist leaders often quote Lenin to the effect that "the transformation of all customs and practices . . . is a work of decades."[60] I am suggesting, however, something slightly more complex: the way in which certain Marxist-Leninist regimes deal with the task of cultural transformation is often contradictory and in part self-defeating. System-building regimes have tended to neglect the cultural realm in favor of other sectors of society and have been selective, often formalistic, in their efforts at transforming culture. The result has been an uneven and mixed developmental profile. The reasons behind this are varied and involve several elements: elements that are shared by certain elites (i.e., a Stalinist or neo-Stalinist conception and approach to the tasks of transformation and consolidation); elements specific to individual elites (i.e., their political identity-defining experiences); and elements specific to individual community cultures (i.e., the type of religious belief and organization that characterizes a given society). An analysis of the Gheorghiu-Dej regime in Romania will illustrate this argument.

Historically, formalism has been a noted and criticized component of the relationship between Romanian institutions and Romanian society. In observing a major Romanian institution, the Orthodox Church, Radulescu-Motru noted that it differed from Protestant churches in the West in being more oriented to ritual and formalistic practices than to practical moral and political lessons.[61] In a similar vein, Adolph Harnack noted that in Orthodoxy, "doctrine comes to be administered in stereotyped formulas accompanied by symbolic acts. . . . For ninety-nine per cent of these Christians, religion exists only as a ceremonious ritual, in which it is *externalised.*"[62] At the community level, then, a major defining experience for the mass of the Romanian population involved uncritical adherence to a belief system that was manifested in terms of symbol and ritual and a private life that remained basically untouched by the formal-ritualistic components of that same belief system. This separation of religious ritual from private life characterized not only the mass of the population but also many of the Orthodox clergy.[63] Such a separation of spheres was not necessarily considered hypocritical, because the integrity of religious belief tended to be identified with the correct application of ritual. In short, ritual was regarded as both substance and form. It was out of this milieu that the faction headed by Gheorghiu-Dej came to power and established the dictatorship of the proletariat. Under Dej the party belief system was in some critical respects as dogmatic and externalized as Orthodoxy's belief system had been.[64] This tendency to externalize and ritualize the structure and ethos of the new regime was not simply a function of Dej's socialization in an Orthodox milieu. One must relate this socialization (Dej's as well as that of his followers and major sectors of Romanian society)to two other elements: the identity-defining experiences of the Romanian Communist Party (R. C. P.), and the adoption of a Stalinist approach to social transformation and political consolidation.

The Romanian party did not have a mass base or following before assuming power and was consequently highly concerned with identity questions. Its adoption of a very rigid interpretation of Marxism-Leninism was a response to its isolation during the interwar period from effective and sustained interaction with a mass following, insecurity over its "proletarian character" in the midst of a peasant society, and conflicts with parties whose ideology was primarily oriented to national culture and the peasantry. The Romanian leadership lacked the historical experience, political confidence, and consequently the ideological sophistication that enabled a party such as the Chinese to approach the basic social force in a traditional society—the peasantry—in a flexible rather than dogmatic, repressive fashion. As suggested, the third element behind the Romanian party's formal and dogmatic approach to cultural transformation was its adoption of a Stalinist strategy in dealing with the tasks of transformation and consolidation. This strategy consists of "walking on one leg," of stressing the organizational and instrumental components of development at the expense of a direct concern with multiple forms of political education and participation.[65] The interaction of these three elements—a community culture shaped by Orthodoxy, a party whose historic experience made it reluctant to engage rather than repress the peasantry and its culture, and a Stalinist conception of its political tasks—contributed to a dogmatic, ritualistic, and formalistic approach to the task of cultural transformation. A striking illustration of the uneven development that has resulted was discussed in the party paper, *Scinteia*. Under the heading, "Any work is clean, only laziness is dirty," the correspondents reported that several high school graduates were demanding office positions and refusing skilled-labor positions in a factory. Interviewing the family of one graduate, the correspondents quickly discovered that the father and mother had successfully socialized their son in a highly traditional fashion. Both they and their son were shocked to think that anyone who had been educated and who "spoke so beautifully" (*Ce frumos vorbeste!*) could be considered for a laborer's position.[66] This is not an isolated incident. The Romanian (and other) regimes are beginning to reap a harvest that is plentiful but unexpected and certainly undesired. While the regime has been quite successful in its attempts to increase the education and skill of many individuals, the corresponding and expected changes in cultural dispositions have not occurred as consistently or as widely as was hoped. Instead, traditional expectations of status defined as prestige have persisted in the face of formalistic demands that they disappear.

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[60] See for example D. Mazilu, "Socialismul—orinduirea cinstei si dreptatii," *Scinteia,* September 3, 1971.

[61] Radulescu-Motru, *Cultura Romana si Politicianismul,* p. 97.

[62] Adolf Harnack, "On Eastern and Western Christianity," in Talcott Parsons et al., *Theories of Society,* vol. 2 (New York: The Free Press, 1961), pp. 1114–1115 and *passim.*

[63] In novels such as Zaharia Stancu's *Barefoot* (New York: Twayne Publishers, Inc., 1971), one can find very insightful portrayals of the clergy's place in the community, and the community's perception of the clergy.

[64] To suggest that Dej was influenced by the Orthodox milieu of his society does not imply that he was religious or accepted the substantive tenets of Orthodoxy; only that certain formal attributes of Orthodoxy were important in shaping the way in which he approached and interpreted his own beliefs.

The restrictive understanding that characterizes a production-mentality approach to cultural transformation appears to complement the selective character of a "commanding heights" approach to social change. Both the commanding heights and the production-mentality orientations neglect critical elements in the units they are dealing with. The commanding heights or "revolutionary laissez faire" approach places a relatively low priority on the rural-agricultural areas and in certain respects the private arenas of society as well; the production-mentality approach fails to appreciate adequately or to come to terms with the expressive dimension of human behavior. The combination of a commanding heights mobilization effort and a production-mentality creates a milieu in societies and regimes such as the Romanian in which individuals are in effect rewarded for attending simply to their own interests both at work and in private settings. Within such a context those individuals who are *pusi pe capatuiala* (stimulated by the prospect of gain) and functionaries who *nu-si vad decit interesele lor* (don't see anything but their own interest) are reacting rationally and predictably to the regime's structure and ethos.

Through their organization and ethos, then, system-building regimes have stimulated a series of informal-adaptive responses—behavioral and attitudinal—that are in many respects consistent with and supportive of certain basic elements of the traditional political culture in these societies. These elements in turn are antithetical to the appearance of a regime and society with an ethos and structure predicated on a complementary relationship between the public and private realms,[67] on the viability of impersonal rules and norms, and on the value of egalitarianism expressed in the role of effective participant.

As the Romanian scholar Radulescu-Motru noted, there is a major difference between a social conglomerate and a viable nation-state. According to Motru, the basic condition for transforming such a conglomerate into a nation is the creation of a political formula that comes to terms with and reflects a conscious, sustained, and direct concern with a society's cultural dimensions.[68] Recently, many Marxist-Leninist regimes have begun to appreciate his general point. There is for example the critical observation made in *Scinteia* that not "all leadership elements, members of party committees [or] basic party bureaus have extended their observation to see how men are working, how they are living, what preoccupies them, how they think. We have been content with the fact that the plan is realized."[69] Political culture has become a salient issue for Marxist-Leninist regimes, including the Romanian. It is to the origins of this increased sensitivity, the type of responses offered by one regime, the Romanian, and the political significance of these responses that we shall now turn.

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[65] The Chinese model of transformation and consolidation differs from the Stalinist precisely in giving lesser weight to the production mentality. Some scholars in effect combine the Leninist-Stalinist models and view Chinese development as a deviant case. I would view both Stalin and Maó as Leninists who have emphasized and partially revised the major elements of the Leninist political-ideological paradigm. As these revisions have drawn on different elements of the paradigm the resulting models do differ in significant respects. One can still argue, however (though for various reasons an argument of that order is not feasible at this point) that both models are in the political tradition of Lenin (just as Bendix and Parsons with all their real differences may be said to be in the intellectual tradition of Weber).

[66] *Scinteia,* September 29, 1971.

[67] I am currently working on a theoretical essay that approaches and interprets the nation-building process from the perspective of the emergence of a public domain that is distinct in character from the state (or official), social and private domains.

[68] C. Radulescu-Motru, *Romanismul* (Bucuresti: Cultura Nationala, 1936). For students of con-

The Conflict and Resolution of Opposing Political Structure-Culture Sets

During the last decade most Marxist-Leninist system-building regimes have indicated their awareness of the growing complexity of their societies. The political significance of this complexity lies not so much in the noted "structural differentiation" that attends the developmental process as in the increase in the number of individuals and groups perceiving themselves as relatively unrecognized politically, economically, socially and ideologically by the regime. Twenty-five years of mobilization and industrialization have created a varied set of social groups with different defining features (partially antagonistic, partially complementary) and have confronted these regimes with the major pressing problem of political integration. The particular form this issue assumes and the particular response it elicits vary according to the leadership, international position, history, and composition of individual communist parties and according to the cultural and social character of each society. What is common to all such regimes, however, is the growing salience of the integration issue as these regimes begin to come to terms with the task of modernization and its imperative of regime-society complementarity rather than regime-society dichotomy.

Recognition of the Problem. In a speech delivered at a leading industrial center, the general secretary of the Romanian Communist Party pointed out to the assembled workers that, "As you can see, we have had an easy time constructing factories. But it is incumbent on us to transform man at the same rate, so that he will be capable of mastering new techniques . . . and new ways of thinking."[70] Taken together, Ceausescu's proposals of early July (1971), and his speech on November 3rd make it quite clear that culture, ideology, and education are now priority issues. There are two interesting and neglected aspects of this new focus. First, there is the continuing, if contradictory,[71] tendency to see the bourgeois past as the single source of a political culture that does not adequately approximate regime expectations. For example, in his November 3rd speech Ceausescu blamed the incidence of "parasitism" in Romania on the mentality generated by the exploiting classes, and he condemned the influence of bourgeois customs for the indifferent, calculating approach of Romanian workers towards socialist property.[72] This tendency to externalize the source of problems, a characteristic response of any organization concerned with minimizing the internal costs of structural and cultural redefinition, limits the regime's ability to see and act on the *multiple* and frequently *regime-generated* causes of such a political culture.

Second, there is the highly instrumental rationale behind the current appreciation of the cultural domain. Thus, in an authoritative article, Presidium member Gh. Pana[73] says that as the regime restructures its relations with Romanian society to allow for greater organizational recognition of various social elements, a cultural "revolution" is necessary to insure the correct interpretation of what this new relationship implies, the competent performance of complex tasks, and the harmonious integration of the regime's multiple goals. Two aspects of Pana's argument warrant attention: (a) culture has not become equal in status with industrialization and social change but has become increasingly significant at this point in time since it affects the regime's ability to enhance its capacity to direct the continued development of Romanian society and (b) at least in Ceausescu and Pana's view the cultural revolution is not considered a retrograde neo-Stalinist

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temporary Africa and Latin America with their interest in neocolonialism, linkage-politics, and fragmented societies, and for students of nation building in general, Motru's work and the debate that took place in Romania in the latter part of the nineteenth and early twentieth century are of real comparative value.

[69] *Scinteia,* December 14, 1971.

[70] *Scinteia,* December 4, 1971.

[71] It is contradictory because on a number of occasions Ceausescu has explicitly argued that defects can no longer be blamed on the "bourgeois past," i.e., Nicolae Ceausescu, *România Pe Drumul Construirii Societatii Socialiste Multilateral Dezvoltate,* vol. 4 (Bucuresti: Editura Politica, 1970), p. 665.

[72] Nicolae Ceausescu, "Expunerea cu privire la Programul P.C.R. . . . ," *Romania Literara,* November 4, 1971, p. 6.

[73] Gheorghe Pana, "Conducerea politica de catre P.C.R. a intregii activitati de faurire a societatii socialiste multilateral dezvoltate," *Lupta de Clasa,* 12 (December 1971), pp. 3–15.

development but the condition for a new non-Stalinist relationship between regime and society.[74]

An Analytic Specification of the Conflict. Currently, the Romanian, along with other Communist parties, faces the politically complex task of reconciling two antagonistic constituencies; constituencies with a preference for opposing organizational formats and with different approaches to authority. More simply, it is a conflict between the Gheorghiu-Dej legacy and the reformist Ceausescu posture. Between the years 1952 and 1964, the Dej regime was based on ritualistic, coercive, arbitrary, and personalistic principles. It is important to note that this kind of regime was not entirely unfamiliar or alien to the experience and social composition of Romanian society. In fact, one might hypothesize that the stability of Dej's regime was partially due to the congruence between the structure and ethos of his rule and the historic experience, social composition, and notion of authority held by large sectors of Romanian society.[75] One need not conclude from this hypothesis that the regime was highly legitimate.[76] One can, however, arrive at a more sophisticated understanding of the relationship that existed between regime and society. If indeed the formal organization of this regime generated a regime political culture that in many respects was congruent with and reinforced the traditional community political culture, then this very congruence contributed to the regime's stability during a period of comprehensive and intense societal dislocation.

The Dej regime, however, did not merely reinforce traditional political culture postures. Through its policies of mobilization and industrialization it created new social groups and corresponding political and cultural postures that in time challenged the very regime that produced them.[77] This dialectical process has been characteristic of all system-building regimes and has generated political crises in both Novotny's Czechoslovakia and Gomulka's Poland. As a consequence of their mobilization and industrialization policies, system-building regimes create strata of skilled workers, educated peasants, and professionals as well as urban-industrial and technological-scientific work settings.

These situational or societal developments accompany changes in the social composition of the Party and an increasing attention within the Party to the task of modernization. As these skilled and more articulate strata become more numerous and as the Party becomes increasingly concerned with modernization, the conflict between many of the current regime's defining features and the type of structure-culture set linked to the task and ethos of modernization becomes increasingly manifest. The conflict may be viewed symbolically as one between rules and patrons at the elite level, between contracts and *blat* (see footnote 51) at the regime level, and between merit and *pile* (connections) at the community level. The conflict is between those who wish to introduce modern—formal, empirical, and leadership—premises into regime-society relationships and those who wish to maintain or extend neo-traditional—substantive, dogmatic, and command—premises. It is a conflict between leaders capable of entertaining a wider range of solutions to problems and leaders with a more restrictive political mentality, between those who wish to maintain a regime-society relationship based on domination and avoidance and those who wish to establish a relationship based on manipulation and selective recognition. Finally, it is a conflict between those who favor a regime-society relationship that generates an "oblique" political culture: one stressing the dichotomization of public and private arenas and placing a premium on the private, covert presentation of demands (i.e., crypto-politics),[78] and those who favor a political culture stressing the complementary character of private and public arenas and recognizing the usefulness of a framework that allows for something more than the private presentation of demands and something less than the public presentation of demands. What is involved then is the ethos and structure of the Romanian regime.

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[74] Of course there are some Stalinists in the party who do see the "cultural revolution" in Stalinist terms. More seriously, given the lack of an alternative political tradition in the party, any move away from the recent emphasis on modernization may necessarily and somewhat unintentionally assume a neo-Stalinist cast.

[75] For the analytic statement that this argument is based on see the excellent piece by Harry Eckstein in his *Division and Cohesion in Democracy* (Princeton: Princeton University Press, 1966), pp. 225–289.

[76] In fact for many sectors of Romanian society, and one may argue for many other societies ruled by Marxist-Leninist parties, the question of legitimacy defined as politically conscious consent was not a major question at all. Defined in this way legitimacy presupposes the existence of a participatory constituency and tradition. The situation was quite different in peasant, patriarchal, and authoritarian Romania. A situation of this order does not remove the issue of regime-society relations; but it should sensitize the analyst to the task of specifying the dimensions of such in light of a specific historic reality.

[77] At another point in his recent work, *Polyarchy,* Dahl notes correctly that political culture studies have focused on "stable and persistent outlooks . . ." and that "investigation of political cultures may easily neglect sources of change . . ." pp. 166–167. In this article we are explicitly concerned with conscious and partially successful efforts at creating a new political culture with the resulting conflict between cultures and the attempts to resolve this conflict.

[78] An illustration of this "oblique culture" is found in an article in *Scinteia* (November 14, 1971). While being interviewed the director of a plant "was interrupted by an associate who tried to whisper something in his ear." "Speak clearly" the director said. But the associate attempted to find a favorable angle to whisper in the director's ear. The director responded, "What you say is of no interest, the law must be applied." The correspondent asked what all

A Political Resolution of the Conflict. The leadership of the R.C.P., specifically, Nicolae Ceausescu, is faced with the task of defining his regime in terms that are at least partially acceptable to those (including himself) whose defining experiences, political perspectives, and operating styles are to varying degrees the product of the Gheorghiu-Dej period: those whose political characters combine traditional peasant and Stalinist political postures. Simultaneously, Ceausescu and his supporters are aware that in twenty-five years the regime has created social groups whose aspirations for recognition require a political response, whose skills require a restructuring of power relations, and whose understanding of authority allows for and requires new institutional formats and a corresponding political-cultural ethos.

The leadership must come to terms with a party and a society in some ways more homogeneous than before 1944 but in others more heterogeneous.[79] Within the Party itself, the number of intellectuals has increased from 11 per cent (93,000) in 1960 to 23 per cent (437,000) in 1970, while the percentage of workers has decreased from 51 per cent to 46.5 per cent in 1972. In addition, it is very likely that an increasing percentage of these workers are recruited from the more skilled stratum.[80]

As for the society, the percentage of peasants has decreased at least 20 per cent in the last twenty years. More significant is the increase in the number of educated, skilled, and professional individuals. In a population of some twenty million the number of nonmanual workers increased by almost 400,000 (from approximately 900,000 to 1.3 million) between 1959 and 1969. In contrast to 1959, when approximately half of these had received higher or middle-level education, in 1969 close to three-quarters had done so. Furthermore, the rate at which such individuals are being educated and trained is increasing. In the eight year period 1960–1968, a greater number of individuals attended advanced study courses than during the period 1945–1959. To return to the Party, both at the center and in the provinces there is a noticeable tendency under Ceausescu to place men within the age group forty to forty-five in leading positions, and at the time of the Tenth Congress in 1969, within the whole Romanian party, 60 per cent of the membership was under forty years of age.[81] While the data are insufficient in this area, there are some grounds for assuming that the leading cadres are better educated than their older comrades and differ importantly (although not completely) in their understanding of authority relations.

These data are provided to illustrate two points. Within Romanian society a relatively new, urban, professional, skilled, educated stratum is growing at a significant rate. This development is not occurring within a vacuum, but rather affects and is affected by a society that is still more than half

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of that was about. The director responded, "*Pile*, illegitimate pressure, to hire someone without regard to their merit." In a very real way the conflict of cultures in Romania today is between the "oblique" presentation of demands and the overt or clear presentation of demands, between those who view the public realm fearfully and try to find "the right angle" to approach it from and those who *spune tare* (speak clearly) who want the public domain to develop a new type of integrity. In connection with the covert presentation of demands see the by now classic statement on crypto-politics in Soviet-type regimes by T. H. Rigby, "Crypto-Politics," in *Survey* 50 (January, 1964), 183–194.

[79] The Romanian party is quite attuned to the issue of social homogeneity-heterogeneity. See for example the discussion in *Lupta de Clasa,* 7 (July 1969), 55–79.

[80] In connection with the social composition of the Soviet and East European parties see the very valuable contributions by Frank Parkin, "Class Stratification in Socialist Societies," *British Journal of Sociology,* 20 (December 1969), 355–374; Frank Parkin, "Yugoslavia," in *Contemporary Europe: Class, Status and Power,* ed., Margaret Scotford Archer and Salvador Giner (New York: St. Martin Press, 1972), pp. 297–318; Mervyn Matthews, *Class and Society in Soviet Russia* (New York: Walker & Co., 1972), pp. 213–255; George Kolankiewicz, "The Working Class," in *Social Groups in Polish Society,* ed. David Lane and George Kolankiewicz (London: The Macmillan Press, Ltd., 1973), pp. 88–152; Peter Ludz, *The Changing Party Elite in East Germany* (Cambridge, Mass.: The M.I.T. Press, 1973), pp. 187–325.

[81] See respectively, Gheorghe Gheorghiu-Dej, *Articles and Speeches:* June 1960–December 1962 (Bucharest: Meridiane Publishing House, 1963), p. 66 and Gheorghe Pana, "Noua structura sociala a Romaniei si cresterea rolului conducator al partidului comunist," *Lupta de Clasa,* 2 (February, 1971), 9; *ibid.,* p. 8; *Anuarul Statistic al Republicii Socialiste Romania 1970* (Bucuresti: Directia Generala de Statistica, 1970), Table 46, p. 122; *Anuarul Statistic Al* R.P.R. (Bucuresti: Directia Generala de Statistica, 1960), Tables 42, 43, 50, 51, pp. 114–115, 122–123; *Anuarul Statistic al Republicii Socialiste Romania 1970* (Bucuresti: Directia Generala de Statistica, 1970), Tables 56, 58, 59, pp. 136–137; *Breviarul Statistic Al Republicii Socialiste Romania 1971* (Bucuresti: Directia Generala de Statistica, 1971), Table 32, pp. 76–77; Virgil Trofin, "Raport Cu Privire La Modificarile Ce Se Propun A Fi Aduse Statutului Partidului Comunist Roman," *Congresul Al X-lea al Partidului Communist Roman* (Bucuresti: Editura Politica, 1969), p. 128.

peasant, a party composed of a good number of old and young cadres with a Stalinist set of experiences and dispositions, and a political community lacking any recent or past experiences with institutions based on effective legal-rational norms. It is hardly an exaggeration, then, to argue that this regime and its leader are faced with an extremely complicated task in responding to distinct and often antagonistic constituencies.[82] What has been emerging (through 1972 at least) is a regime with a *patriarchal-executive* character, one based on what Eckstein has termed balanced disparities.[83] This regime is attempting to manage constituencies whose preferences concerning institutional formats and authority relationships are, if not mutually exclusive, at least highly incongruent and often incompatible. One may look at this regime in terms of Gerschenkron's concept of historical substitutions;[84] namely, as a response to the universal imperatives of modernization (rather than as in Gerschenkron's formulation, industrialization) that reflects the historical legacy of Romanian society, the political character of the Romanian party, and the social, economic, and cultural definition of contemporary Romanian society.

The patriarchal-executive regime is oriented to three goals: authoritarian leadership, welfare, and efficiency. The Plan in Romania is an important example of the merging of modern, Leninist, and traditional premises in a far from harmonious but thus far (1972) workable synthesis. Certainly, one cannot interpret the Party's apparently absolute commitment to the Plan as simply an inevitable Leninist stance; the Hungarian and Yugoslav attitudes contradict such a notion. Rather the intense commitment to the Plan reflects three partially compatible and partially incompatible factors: (a) the Leninist commitment to directed mobilization, (b) the Party's "patriarchal" concern (*grija*) for the population's well-being (*bunastarii*), and (c) the Party's increasing appreciation of managerial and technical expertise.

As a mode of leadership, patriarchalism has historically had several distinguishing features including a stress on orderly rather than charismatic (i.e., highly random and arbitrary) management, direct concern with the economic welfare of the social unit involved, personalized attention to solidarity issues, and emphasis on authoritarian and disciplinary behavior.[85] Defined in these terms a "patriarchal" stance by the Party is consistent with its assumption of authoritarian responsibility for the overall development of Romanian society. It is also compatible with the Party's effort to respond to social dislocation and cleavage in a society that has experienced a prolonged period of mobilization-industrialization and that has no significant experience with institutions based on impersonal instrumental norms. Finally, to the extent that a "patriarchal" stance (in contrast to the "patrimonial" or "sultanist" structure under Gheorghiu-Dej) decreases the incidence of arbitrary behavior, it is compatible with a modern or "executive" structure. Similarly, a modern "executive" structure is compatible with a "patriarchal" stance insofar as executive organizations are authoritarian organizations.

The Romanian leadership is in the process, then, of elaborating a structure and ethos designed to elicit the approval of quite different constituencies within as well as outside the Party. The Ceausescu leadership has attempted to build into the regime a set of balanced disparities that are congruent with the different expectations and orientations of quite different groups of party members and social strata.

A good example of this blending or amalgamating process is the way in which Ceausescu's role is defined. Ceausescu has defined the leadership role as entailing respect for the corporate identity and integrity of the Party as an institution, adherence to Party rules, and enhancement of procedure in the workings of the party organization. He is portrayed as self-disciplined, hard working, tolerant of debate and expert opinion; yet as a leader in touch with the people. But he is also the concerned father of his people visiting the market places; Ceausescu, the proposed authoritative model for the Romanian people presented as a latter day Fat-Frumos, the mythical youthful hero of the Romanians;[86] Ceausescu, the focus of what is often referred to as a personality cult. This amalgamated image appears designed to elicit approval from diverse constituencies: on the one hand, the peasant, the apparatchik, and the "new class," and on the other, the professional, and the managerial-type party cadre.

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[82] Ceausescu himself may be viewed as an amalgam leader, insofar as he not only represents and leads distinct constituencies that are themselves characterized by conflict-accommodative patterns, but also reflects in his own political personality these partially compatible, partially conflicting tendencies.

[83] Eckstein, *Division and Cohesion in Democracy*, pp. 262–269.

[84] Alexander Gerschenkron, "Economic Backwardness in Historical Perspective," in *Economic Backwardness in Historical Perspective: A Book of Essays*, ed. Alexander Gerschenkron (Cambridge, Mass.: Harvard University Press, 1962), pp. 5–31.

[85] On patriarchalism see Max Weber, *Economy and Society*, ed. Guenther Roth and Claus Wittich (New York: Bedminster Press, 1968), pp. 231, 240, 645, 1006–1010, 1112, 1118; Vera St. Erlich, *Family in Transition* (Princeton: Princeton University Press, 1966), pp. 31–60.

[86] Pavel Apostol has written an excellent article dealing with Fat Frumos titled, "Fat-Frumos si tineretea ca dimensiune spirituala," in *Trei Meditatii Asupra Culturii* (Cluj: Editura Dacia, 1970), pp. 19–51. While explicit parallels with Ceausescu are not drawn they are not hard to find. Ceausescu himself in some speeches has identified himself with the youth, while certain of his associates have presented him as a "model" for Romanians to follow.

The political task of eliciting and managing such approval, however, from what are basically antagonistic constituencies with different operating styles is by no means easy. In the course of the last seven years the Party has adopted two distinct strategies for balancing constituencies with different cultural postures and structural preferences. The first strategy was compartmentalism. The strategy consisted of isolating opposing elements from one another by parceling out areas that were in effect equivalent to protected fiefs. To call this pluralism would not be totally incorrect, but it would be more misleading than useful because of the incidence of conscious elite manipulation and the absence of a public arena. For these reasons I prefer the term authoritarian compartmentalism.[87] This strategy has quite recently been supplemented and largely replaced by one that places a greater emphasis on combining representatives with competing orientations in the same institutions. We mention these strategies in order to highlight the crucial fact that the emergence of a patriarchal-executive regime is not an ethereal process. Ceausescu's success will partially depend on the strategy (i.e., authoritarian compartmentalism or fusion) he chooses, and that strategy will in turn reflect the opposition he encounters and his own ambivalence toward cultural and structural change. Although he has given many indications that he is sensitive to the importance of culture, diverse constituencies, and the imperatives associated with modernizing, there is no guarantee that he will successfully provide for the consequences of his various policies. Nor is there any guarantee that he can continue to bridge the gap between his two constituencies within and outside the Party. In short, I am not at all suggesting that Ceausescu and his supporters have developed a perfect regime-amalgam or synthesis. The serious challenge he received in the summer and fall of 1971 and the resulting changes in policy suggest that Ceausescu is still the manager and leader of a coalition (not as yet the "patrimonial leader" of a political retinue as Dej was) and that this coalition is composed of elements that are capable of conflict as well as accommodation.

One might hypothesize that as modernization progresses, the choices before the Romanian Party may become more mutually exclusive, in which case the current patriarchal-executive amalgam may fail.[88] Noting all of this does not, however, minimize or underrate the significance and innovativeness of the current Romanian leadership's attempts to reform the existing character of the nation's political structure and ethos. The patriarchal-executive regime embodies the Romanian attempt to move with control and stability from an established neotraditional/Stalinist political society to a modern Leninist political society.[89]

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[87] This article was written before I came across two recent articles that are of major significance in Communist studies. One is that by Jerry F. Hough, whose notion of institutional pluralism is very congruent with what I am terming authoritarian compartmentalism (see "The Soviet System: Petrification or Pluralism?" *Problems of Communism*, 21 (March-April, 1972), 25–46, and the already cited article by Zygmunt Bauman.

[88] In the period since this article was written one striking development has occurred within Romania. Mobilization has become the defining emphasis. At the leadership level this is seen in the increasingly patrimonial character of Ceausescu's position. With respect to the Weberian categories that have served as the basis for my characterization of the Romanian regime between 1970 and 1972, the regime in the last year and a half (this is being written in August 1973) has moved from an emphasis on neo-traditional (patriarchal) and legal-relational (executive) considerations to a much greater emphasis on the charismatic qualities of the party.

[89] I have attempted to deal with some of the questions connected with the compatibility of Leninism and modernity in an article titled "Inclusion and Mobilization in Marxist-Leninist Systems," which will be part of a volume edited by Mark Field titled *The Social Consequences of Modernization in Socialist Countries.*

Foreign Policy and Empirical Democratic Theory*

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The studies of political and economic determinants of policy outcomes in the American states have been extensive, and the findings have generally been cumulative. V. O. Key's[1] original hypothesis about the importance of interparty competition on policy outcomes was subsequently supported and elaborated by Lockard.[2] The first serious challenge to Key's theory was presented by Dawson and Robinson,[3] who expanded the concern with policy determinants to include a wider range of political and economic variables. Subsequent studies by Jacob,[4] Dye,[5] and Hofferbert,[6] among others, also questioned the importance of political determinants, suggesting instead the greater importance of economic variables as correlates of policy outcomes. A comprehensive study by Dye[7] of the American states provided additional evidence that "differences in the policy choices of states with different types of political systems turn out to be largely a product of differing socioeconomic levels rather than a direct product of political variables." In reviewing this effort, Robinson was able to conclude that "thanks to Dye's extensive efforts, I believe judgment [about the relative importance of politics and economics to policy outcomes] may now be rendered with persuasiveness if not conclusiveness."[8]

But the challenge to the challengers was almost immediate and was provided in part by the challengers themselves. Dye[9] noted the greater importance of political variables in accounting for the distribution of wealth among the populations in the states, a point later elaborated and supported by Fry and Winters.[10] Sharkansky and Hofferbert,[11] Cnudde and McCrone,[12] and Clarke,[13] among others, concluded that for certain policies, particularly those that do not depend primarily upon monetary resources for implementation, political characteristics of state systems are more important than the state's economic wealth. Thus, one important hypothesis drawn from this most recent research is that political variables *are* important determinants of policy outcomes, but only (or at least primarily) when those outcomes are essentially political rather than economic.

A second hypothesis drawn from this research was offered by Cnudde and Neubauer,[14] who point out that despite some variation in electoral competition, all of the American states are basically democratic. Among democratic polities, the authors hypothesize, variation in democracy is slight and thus may not be as important as environmental or socioeconomic influences, while in undemocratic nations these latter influences will decrease, and variations in political characteristics will be more important. To test this hypothesis, cross-national studies would have to be conducted.

A third hypothesis is offered by Cutright,[15] who

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* I am grateful to John R. Kayser and Michael K. O'Leary for comments on an earlier draft.

[1] V. O. Key, Jr., *Southern Politics* (New York: Vintage Books, 1949).

[2] Duane Lockard, *New England State Politics* (Princeton, New Jersey; Princeton University Press, 1959).

[3] Richard E. Dawson and James A. Robinson, "Inter-Party Competition, Economic Variables and Welfare Policies in American States," *Journal of Politics*, 25 (May, 1963), 265–89.

[4] H. Jacob, "The Consequences of Malapportionment: A Note of Caution," *Social Forces*, 43 (December, 1964), 256–61.

[5] Thomas R. Dye, "Malapportionment and Public Policy in the States," *Journal of Politics*, 27 (August, 1965), 586–601.

[6] R. I. Hofferbert, "The Relation Between Public Policy and Some Structural and Environmental Variables in the American States," *American Political Science Review*, 60 (March, 1966), 73–82.

[7] Thomas R. Dye, *Politics, Economics, and the Public: Policy Outcomes in the American States* (Chicago: Rand, McNally, 1966).

[8] James A. Robinson, "A Review of Dye, *Politics, Economics, and the Public*," in the *American Political Science Review*, 61 (December, 1967), 113–14.

[9] Thomas R. Dye, "Income Inequality and American State Politics," *American Political Science Review*, 63 (March, 1970), 157–62.

[10] Brian R. Fry and Richard F. Winters, "The Politics of Redistribution," *American Political Science Review*, 64 (June, 1970), 508–22.

[11] I. Sharkansky and R. I. Hofferbert, "Dimensions of State Politics, Economics, and Public Policy," *American Political Science Review*, 63 (September, 1969), 867–79.

[12] Charles F. Cnudde and Donald J. McCrone, "Party Competition and Welfare Policies in the American States," *American Political Science Review*, 63 (September, 1969), 858–66.

[13] James W. Clarke, "Environment, Process and Policy: A Reconsideration," *American Political Science Review*, 63 (December, 1969), 1172–82.

[14] Charles F. Cnudde and Deane E. Neubauer, "New Trends in Democratic Theory," in *Empirical Democratic Theory*, ed. Cnudde and Neubauer (Chicago: Markham, 1969).

[15] Phillips Cutright, "Inequality: A Cross-National Analysis," *American Sociological Review*, 32 (August, 1967), 562–78; and "Political Structure, Economic Development and National Social Security Programs," *American Journal of Sociology*, 70 (March, 1965), 537–51.

extended this type of research to the national level. Although he did not specifically test the Cnudde and Neubauer hypothesis mentioned above, he did study policies cross-nationally and discovered that the political representativeness of a nation is indeed an important determinant of policy. The importance, however, seemed to depend on whether the nations were developed or undeveloped. Among underdeveloped nations, he found that social insurance programs had yet to be adopted even though the nations showed considerable variation in representativeness. But among the highly developed nations, he found a positive relationship between governmental representativeness and social security coverage. This difference between developed and undeveloped nations was attributed to the "technological and bureaucratic prerequisites" needed to implement the social insurance programs, prerequisites which only the developed nations possess. The undeveloped nations, no matter how politically representative their national government, do not have the resources to adopt such programs. To generalize from this case, the third hypothesis can be stated as follows: the importance of governmental representativeness to policy outcomes is higher in developed than undeveloped nations.

The following is a restatement of the three hypotheses just mentioned, all drawn from the recent literature analyzing the relationship between democracy and policy outcomes:

(1) Political variables describing the extent of a government's democratic processes (e.g., party competition) *are* important determinants of some policy outcomes, primarily those outcomes which are political rather than economic.

(2) Among democratic polities, socioeconomic characteristics are more important than governmental characteristics in accounting for policy outcomes, while the opposite holds among undemocratic polities.

(3) The importance of governmental representativeness to policy outcomes is higher in developed than in undeveloped nations.

One of the notable characteristics of all of this research is its focus on domestic policy to the exclusion of foreign policy. Most of the research is even more narrowly focused on American state governments, but even Cutright's cross-national studies deal only with domestic policy. Yet, as the discussion above demonstrates, the types of policies subject to analysis have been increasing, and it seems only logical to extend the analysis from domestic to foreign policies.

Foreign Policy and National Attributes

All of the data and analyses reported here are based on a cross-sectional study of 109 nations for the year 1963. Nine independent measures of governmental and societal attributes, all statistically independent of each other, were defined by factor analyzing 64 governmental and societal attributes of nations.[16] The nine factors that emerged from the analysis are shown in Table 1, along with the highest loading variables on each

Table 1. Measures of Governmental and Societal Variables

1. Economic Development
   a. GNP per capita (.93)[a]
   b. Energy consumption per capita (.91)
   c. GDP per capita (.89)
   d. National income per capita (.88)
2. Size
   a. Total population (.97)
   b. Total GNP (.85)
   c. Total GDP (.79)
   d. Total national income (.78)
   e. Total energy consumption (.72)
3. Accountability
   a. Status of legislature (.82)
   b. Electoral system (.82)
   c. Horizontal power distribution (.78)
   d. Freedom of press (.77)
   e. System style (.76)
   f. Representative character of current regime (.73)
4. Governmental Stability
   a. Governmental stability (.75)
   b. Communist party (.64)
   c. Stability of party system (.64)
5. Christian Culture
   a. Per cent Christian (.88)
   b. Per cent Moslem (−.78)
   c. Per cent Roman Catholic (.68)
6. Elite Tenure
   a. Elite tenure (based on last two officials) (.84)
   b. Elite tenure (based on last ten years) (.80)
7. Population Density
   a. Population density (.81)
   b. Total land (.68)
8. Urbanism
   a. Population in cities over 20,000 (.55)
   b. No. of workers in agriculture as per cent of economically active population (−.49)
   c. Per cent GNP from Agric. (−.44)
9. Institutionalization
   a. Interest aggregation by the executive (.69)
   b. Period of national independence (−.61)

[a] Numbers in parentheses refer to each variable's loading on the respective factors. Thus, GNP per capita enjoyed a .93 loading on (or correlation with) the Economic Development factor.

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[16] The data used here were provided by the Comparative Analysis of Policy Environments (CAPE) Project under the direction of Philip M. Burgess, The Ohio State University. A report on the factor analysis is included in my "Governmental and Societal Influences on Foreign Policy in Open and Closed Nations," in *Comparing Foreign Policies: Theories, Findings, and Methods*, ed. James N. Rosenau (Sage, 1974).

Table 2. List of Foreign Policy Measures

1. Cold War 2. Intervention in Africa 3. Supranationalism	The first three variables represent the three voting dimensions of the United Nations General Assembly, 1963. Data were all supplied by Bruce M. Russett.[19]
4. Alignment	A measure of international alignment based on the per cent of votes in the United Nations for the U.S. on issues which found the U.S. and the U.S.S.R. opposed. Correlates .68 with the Cold War issue above.
5. Defense Expenditures as a Percentage of GNP	
6. Conflict	A measure of verbal conflict based on the number of demands, protests, and accusations made by each nation. Data provided by R. J. Rummel.[20]
7. Participation	A measure of involvement in international relations based on the number of ambassadors sent to foreign countries.

factor. Seven foreign policy dimensions were also defined and are listed in Table 2. Except for Alignment and Cold War, the foreign policy measures are only moderately or slightly intercorrelated,[17] thus tapping a variety of foreign policy dimensions.

In a previous study,[18] each foreign policy measure was regressed against all nine independent variables to examine the relative importance of the political representativeness or accountability of a nation on its foreign policy actions (see Hypothesis #1). The Accountability variable proved to be the most important of all nine variables, as indicated by the size of its partial correlation coefficient, in accounting for the measures of Cold War, Supranationalism, Alignment, and Defense Expenditures. For Conflict, the correlation coefficient was statistically significant, but smaller than the Size correlation coefficient; for Participation, the coefficient was significant but only fifth in importance; and for Intervention in Africa, the correlation coefficient was not significant. Overall, Accountability had higher partial correlation coefficients with the foreign policy measures than did any of the other independent variables, including Size and Economic Development. Thus, it is clear that on the "economic" issue of Defense Expenditures as well as the "political" issues of Cold War, Supranationalism, and Alignment, the degree of a country's political accountability did exert significant impact even when other important variables, such as Size and Economic Development, were controlled.

In a related study,[21] the differences in determinants of foreign policy between democratic and undemocratic (open and closed) nations were examined. Both Cnudde and Neubauer[22] in discussing domestic policies, and Rosenau[23] in discussing foreign policies, hypothesized that in the democratic nations, environmental or societal variables are more important than governmental variables in determining policy, while the opposite is true in undemocratic nations (see Hypothesis #2). To examine this notion, all nations were classified either as "open" or "closed," and each foreign policy measure was regressed against the five societal and four governmental variables. The relative weight of each cluster was then assessed by adding partial beta coefficients. The results were less clear than those of the previous analysis, but on five of the seven issues, Intervention in Africa, Supranationalism, Alignment, Defense Expenditures, and Conflict, the proportional importance of governmental determinants turned out as predicted to be greater in closed than open nations. On the Cold War issue no differences emerged; on the Participation measure the differences between open and closed nations were opposite to those predicted, although the magnitude of the differences was smaller than that on any of the five issues that fit the predictions. Thus, overall, some support is given to the importance in policy analysis of the distinction between democratic and undemocratic polities hypothesized by Cnudde and Neubauer and by Rosenau. The determinants of policy do seem to act in different ways in each of the polity types, thus suggesting different models for each of the two types.

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[17] Of the 21 intercorrelations among the seven foreign policy measures, 13 are less than .30; only Cold War and Alignment correlate more than .50.

[18] Moore, "National Attributes and Nation Typologies: A Look at the Rosenau Genotypes," in *Comparing Foreign Policies.*

[19] For a report of these dimensions, see Bruce M. Russett, *International Regions and the International System* (Chicago: Rand McNally, 1967).

[20] See R. J. Rummel, "Some Dimensions in the Foreign Behavior of Nations," *Journal of Peace Research,* No. 3 (1966), 201–23.

[21] Moore, "Governmental and Societal Influences on Foreign Policy in Open and Closed Nations," in *Comparing Foreign Policies.*

[22] Cnudde and Neubauer, *Empirical Democratic Theory,* 511–34.

[23] James N. Rosenau, "Pre-Theories and Theories of Foreign Policy," in *Approaches to Comparative and International Politics,* ed. R. Barry Farrel (Evanston: Northwestern University Press, 1966).

A third analysis of foreign policy data deals with the relationship noted by Cutright between democracy and economic development (see Hypothesis #3). Cutright's study of national social insurance programs, it should be recalled, revealed that only developed nations had the technological and bureaucratic resources to respond to the demands of the population for such programs. Thus, the hypothesis inferred from this analysis is that political representativeness is more important for policy outcomes in developed nations than in undeveloped nations.

To examine the applicability of this hypothesis to foreign policy issues, all nations were classified as either developed or undeveloped,[24] and for both groups each foreign policy measure was regressed against the nine independent variables. If Cutright's observations are applicable to the foreign policy measures, the partial correlations of the Accountability variable with each foreign policy measure should ideally be strong among the developed nations and weak or nonexistent among undeveloped nations. In a less ideal case, the partial correlation coefficients should at least be larger among the developed than the undeveloped nations.

As Table 3 reveals, on every foreign policy measure, Accountability is more important in developed than undeveloped nations. On three of the seven issues, Accountability is statistically significant even in undeveloped nations; and on three other issues, statistical significance is not achieved even in developed nations. But tests of significance are at best only suggestive of substantive importance, and in this particular analysis the focus is on the overall pattern of coefficients in developed and undeveloped nations. The probability of finding the pattern described here, in which the correlation coefficients are larger in one group than the other for all seven cases, is less than one in a hundred.

Table 3. Relative Importance of Political Accountability in Developed and Undeveloped Nations

Foreign Policy Measure	Partial Correlation Between Accountability and Foreign Policy Measure: Developed Nations	Undeveloped Nations
Alignment	.96*	.50*
Cold War	−.89*	−.46*
Supranationalism	.91*	−.12
Defense Expenditures	−.47*	−.30*
Africa Intervention	−.34	−.05
Conflict	−.30	.05
Participation	−.24	−.05

* *Statistically significant at the .05 level using a "t"-test.*

Summary and Conclusion

In this comparison of the studies of American state domestic politics with those of national foreign policies, at least two methodological problems should be mentioned. First, the variables of democracy, governmental representativeness, and accountability, while conceptually similar, are all measured in different ways for different studies. Thus some doubt may exist about the validity of comparing findings from these different studies when one of the central concepts—democracy—is not measured identically in each study. In the studies cited in this paper, however, most of the findings tend to reinforce each other in spite of the different ways of measuring democracy. This convergence of findings in the face of methodological differences should, in fact, be reassuring because it demonstrates that the findings are not dependent upon just one measurement.

A more serious problem is the conceptualization of issue areas, particularly in the foreign policy analysis. In this study, seven issues were analyzed, only two of which are highly intercorrelated. There is no assurance, however, that those seven issues are the most important ones, nor that some very important issues have not been excluded. So far, little agreement has been reached among scholars about the most useful way to typologize issues, and the few quantitative attempts that have been made are too narrowly focused upon one kind of data.[25] Furthermore, these data are much more highly aggregated and less obviously policy-specific than the domestic policy studies cited in this paper.

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[24] For the method used to classify nations as developed or undeveloped, see Philip M. Burgess, "Nation Typing for Foreign Policy Analysis: A Partitioning Procedure for Constructing Typologies," in *Methodological Concerns in International Studies,* ed. Edwin H. Fedder (St. Louis: University of Missouri, Center for International Studies Monograph Series, 1970), pp. 3–66. Of the 109 nations included in this study, 42 are classified developed, 67 undeveloped.

[25] For a review of this literature and a creative attempt at devising a typology of foreign policy outputs, although based solely on WEIS events data, see Charles W. Kegley, Jr. *A General Empirical Typology of Foreign Policy Behavior,* Sage Professional Papers in International Studies (Beverly Hills: Sage, 1973). For a description of WEIS events data, see Charles A. McClelland, "The World Event/Interaction Survey: A Research Project on Theory and Measurement of International Interac-

In spite of these limitations, the comparison of the hypotheses generated by the studies of domestic policy with the results of the foreign policy analyses reveals many similarities. While the distinction between economic and political outcomes suggested by the first hypothesis does not seem to hold for the foreign policy measures, the importance of political accountability on some foreign policy outcomes is emphatically demonstrated. Furthermore, the second hypothesis, suggested by Cnudde and Neubauer from the results of American state studies, also receives support from the foreign policy analysis. Governmental influences on most of the foreign policy measures are proportionately greater in undemocratic than democratic nations. And the third hypothesis, drawn from Cutright's analysis of social insurance programs, was clearly supported by the analysis of foreign policy issues.

While the similarities are encouraging, many deviations from the general trend need to be explained. More important, some refinements of the underlying democratic theory may also be needed. Cutright's hypothesis, for example, was based on the assumption that more technological and bureaucratic resources are available for the democratic nations to respond to the demands of the people. Yet it is not at all clear that for the foreign policy issues analyzed in this study, a nation requires *more* resources in order to respond to the people's demands. In fact, as one example, the more representative a nation, the *lower* its defense expenditures. Furthermore, the underlying democratic theory is based on the notion that the more representative nations allow wider participation by the general public, and the general public in turn pressures its governmental representatives to adopt specific kinds of proposals. As Cnudde and Neubauer point out,[26]

> . . . democracy, however defined, includes the sharing of power or influence by wider sections of society than nondemocratic alternatives. Therefore, by widening the degree of representation, democracy in practice results in a greater tendency for the enactment of preferences of the lower social strata.

Implicit in this formulation is the assumption that members of the lower social strata will act to have government adopt policies favoring themselves, such as programs in health, education, and welfare. But it is not as easy to assume pressure from the lower social strata for specific kinds of foreign policies. In fact, research indicates that the general public is usually ill-informed and unconcerned about such issues and certainly does not act as a very important pressure group for specific policy actions.[27]

In spite of this incongruity between the theory underlying the hypotheses and the conditions that actually exist, the data reported above tend to support the three hypotheses. The reasons for this paradox may lie in the democratic theory which postulates direct linkages between the representatives and the general public, linkages which do not seem to exist, at least for foreign policy. It may be instead that representatives in the more democratic states, because of the democratic norms they have learned throughout their lives, operate within similar philosophic references and consequently favor similar kinds of policies, regardless of whether there is public pressure for these policies and perhaps even regardless of whether the policies are necessarily beneficial to the general public. Thus, the more democratic the nations, the greater the impact of a specific philosophical approach to foreign policy and the more likely that the representatives of such nations will arrive at similar foreign policy outcomes. While this reasoning differs from the democratic theory outlined by Cnudde and Neubauer, the first two hypotheses derived from that theory nevertheless are consistent with this reformulation.

Furthermore, the agreement among the democratic leaders of the developed nations may be greater than that among democratic leaders of the undeveloped nations because of the longer time that democracy has been operative in the developed nations and because of the greater impact that this philosophy has consequently exerted on decision-makers. The third hypothesis is thus also suggested, but again with a different theoretical tenet.

This paper is not intended, however, to argue for a specific modification of empirical democratic theory, but rather to show the similarity of findings between the empirical studies of domestic policies and of foreign policies, and to suggest that the implications of these similarities for democratic theory need to be considered. In a recent article,[28] Harvey Mansfield, Jr. points out that

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tion and Transaction," University of Southern California, Department of Political Science (mimeo), 1967.

[26] Cnudde and Neubauer, p. 524.

[27] For a review of much of the literature in this area as well as an illuminating study of the importance of public opinion to executive officials, see Bernard C. Cohen, *The Public's Impact on Foreign Policy* (Boston: Little, Brown, 1973).

[28] Harvey C. Mansfield, Jr., "Hobbes and the Science of Indirect Government," *The American Political Science Review*, 65 (March, 1971), 97–110.

Hobbes was the first philosopher to argue that the "indirect" question of whether policies are representative of the people's will ought to be asked before the "direct" question of whether the policy is "decent, good, or useful"; in short, one ought to focus first on process and only subsequently on the content of policy. It seems, however, that because of public ignorance and general unconcern with foreign policy issues, the question of whether or not such policy is representative of the people's will may not even be relevant. Thus, contrary to Hobbes's argument, for an understanding of the differences in foreign policies between democratic and undemocratic nations, perhaps attention should first be given to differences in the philosophical underpinnings of their policies rather than to the specific processes by which such policies are adopted.

The ABM Issue in the Senate, 1968–1970: The Importance of Ideology

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The U.S. Senate is beginning to play a major role in deciding national security/war and peace policy for the nation.[1] The modern Senate apparently perceives its role in this area to concern far more than just "real estate" questions.[2] The extensive debate and close votes on the ABM issue and various issues related to the Vietnam war give qualitative evidence of the Senate's policy concern. Furthermore, Russett's analysis of Senate voting behavior and defense spending gives strong quantitative evidence that something more than state economic advantage is motivating Senate behavior.[3]

The ABM issue caused more extensive debate in the Senate than did any other national security issue since 1945. Voting on the issue was about as close as possible, with killing amendments turned back 50–50 (the vice-president voting against) in 1969 and 47–52 in 1970. The issue spanned three years, two Congresses, and two administrations. It was one that the senators apparently considered important, on which they were forced to take a stand, and on which every vote counted. It was also an issue on which the factors influencing senatorial behavior had a chance to build over time. Thus, there is reason to believe that analysis of the ABM issue in the Senate will have general value.

The literature suggests at least three important theoretical reasons that a senator might adopt or change positions on such an issue as the ABM. One obvious hypothesis is that the greater the relative state economic benefits (in terms of production and research and development contracts) the more likely a senator will be to adopt a position favoring the issue.[4] Even where other factors suggest opposition to the bill, a senator might vote for a bill that would aid his constituency and presumably enhance his chances for re-election.[5]

A second hypothesis is that the more committed a senator is to a party (Republican or Democratic) the more likely he will be to reflect that party's position on the issue. At least by 1969, this would mean that the more consistently a Republican senator has voted with the party, the more he would favor the ABM. Hinckley states that in the 91st Congress, the Senate "debate on ABM . . . produced clear party lines, with an average of 62.3 per cent of the Democrats and 30.8 per cent of the Republicans voting in four roll calls against the expanded ABM program."[6] Party commitment then, might be the overriding influence in senatorial behavior.[7]

The third hypothesis is that senators vote in accordance with an internalized set of political beliefs variously termed political doctrines, conscience, or ideology.[8] It is assumed that the ideologies of virtually all U.S. senators may be ordered along a conservative-liberal continuum, and the more conservative the senator, the more likely he is to favor the ABM.[9] Hinckley states that the "ABM controversy in the Ninety-first Congress . . . split the Senate into liberal and conservative lines, with a majority of northern

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[1] For an elaboration of this point, see Barbara Hinckley, *Stability and Change in Congress* (New York: Harper & Row, 1971), chapter 7.

[2] Even "real estate" questions have policy implications and the Senate has followed up on those implications, at least since the fifties; see Raymond Dawson, "Congressional Innovation and Intervention in Defense Policy: Legislative Authorization of Weapons Systems" *American Political Science Review*, 56 (March, 1962), 46–50.

[3] Bruce Russett, *What Price Vigilance? The Burdens of National Defence* (New Haven: Yale University Press, 1970), pp. 72–79.

[4] This is one of the general hypotheses tested by Russett, chapter 3.

[5] In regard to this issue, note the discussions of delegates, locals and opportunists in Roger Davidson, *The Role of the Congressman* (New York: Pegasus, 1969), chapter 4; and J. C. Wahlke, H. Eulau, W. Buchanan, and L. C. Ferguson in *The Legislative System: Explorations in Legislative Behavior* (New York: Wiley, 1962), part 4.

[6] Hinckley, p. 155.

[7] The American Political Science Association Committee on Political Parties in its 1950 report (*American Political Science Review*, 44 [September, 1950] supplement, 1–10) suggested that such party influence would be a desirable goal, rather than a current practice. However, Julius Turner in his *Party and Constituency: Pressures on Congress* (Baltimore: The Johns Hopkins Press, 1952) suggests that party influence is already felt strongly.

[8] Note the discussion of delegates and statesmen in Davidson, chapter 4, and Wahlke et al., part 4.

[9] While the conservative-liberal typology is sufficiently multidimensional (Robert Lane, *Political Thinking and Consciousness* [Chicago: Markham, 1969]) to cause serious conceptual problems in some cases, for this issue the subdimensions of the typology are unlikely to predict contradictory behavior.

Democrats opposing a majority of southern Democrats and a majority of Republicans . . . "[10]

It is our contention that ideology is a much more influential factor than either party commitment or economic incentives in determining a senator's position on national security/war and peace issues. National security issues such as this one, cause a fairly sharp liberal-conservative break because the relevant subdimensions of that typology tend to be reinforcing. That is, the conservative tends to support the ABM not only because of his focus on the "priority of American interests and rights"[11] but also because he believes that communism presents a grave danger to the U.S., that our defense against it should be adequately maintained, that defense is one of the few legitimate concerns of the national government, and that it may be necessary to risk war to prevent the advance of Communism.[12] The liberal tends to reject each of these positions. He emphasizes equality of nations,[13] suggests many other functions of government that should have priority over an increased defense effort and fears nuclear war more than he fears intermediate levels of Communist expansion.[14]

Asserting the importance of ideology does not mean, of course, that is the only influence senators experience in their choice of positions. Senators are subject to considerable party and administration pressure, and we expect party to be a minor factor in determining a senator's position.[15] In the case of the ABM issue, this may be especially true after the change of administrations in 1969.[16] We will try to demonstrate, however, that party commitment is not a major factor, that such evidence as Hinckley presented to illustrate the importance of party is spurious, and that party appears to be a strong factor primarily because it is so highly correlated with ideology; that is, the more committed Democrats tend to be more liberal and the more committed Republicans to be more conservative. If liberals tend to oppose and conservatives to favor the ABM, then Democrats will also tend to oppose and Republicans to favor the issue, even if party is of no importance in determining position on the ABM. We will attempt to demonstrate that if ideology is controlled for, party will become a relatively minor factor in determining a senator's position.

Even though it seems obvious that any representative would support spending beneficial to his constituents, and despite all the discussion of defense decisions being motivated by "real estate" considerations, we expect economic incentives to have virtually no influence in determining a senator's position on the ABM or, indeed, any other major national security/war and peace issue. We believe that the mechanisms regulating selection and tenure for the U.S. Senate were designed[17] and operate to increase self-direction and decrease constituency-direction. Further, it seems that national security/war and peace issues would not tend to be perceived as salient to the constituency.[18] Finally, Russett's empirical analysis of senatorial voting and defense-related expenditures for 1961–62 and 1967–68, yields virtually no evidence that economic incentives more than slightly influence the position that senators take on even general defense spending.[19] On any specific issue of major importance to the nation such as the ABM, we expect even this slight influence to disappear.

These arguments can be carried an important step further. Because ideology merely *tends* to influence position, not all senators would have adopted the position on the ABM that one would have anticipated on the basis of their degree of liberalism or conservatism. These deviants found the spotlight of national publicity turned on themselves and their position on the ABM from 1968 on. Especially as the vote grew close in 1969 and 1970, these senators found themselves under increasing pressure. By 1970, it is hypothesized that most of the deviants would have become painfully aware of a dissonance between their ideological beliefs and their 1968 position on the ABM.[20] In an effort to reduce this dissonance, the deviants were likely to change their positions on the ABM (this being easier than changing their ideological beliefs). Such a change in their positions regarding the ABM would also bring them in line with the positions of their principal reference groups in the Senate; thus, they would then conform to the expected behavior for their ideological group.[21]

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[10] Hinckley, p. 151.

[11] Lane, p. 52.

[12] For elaboration of these themes, see especially Barry Goldwater, *Why Not Victory?* (New York: McGraw-Hill, 1962); James Burnham, *Suicide of the West* (New York: Day, 1964); Charles Malik, "The Challenge to Western Civilization," in *The Conservative Papers* (Garden City, New York: Doubleday, 1964), pp. 1–17.

[13] Lane, p. 52.

[14] For more elaboration, see especially Students for a Democratic Society, "America and the New Era," convention paper, Pine Hill, New York, June 14–17, 1963; H. Stuart Hughes, *An Approach to Peace* (New York: Atheneum, 1962); *The Liberal Papers* (Chicago: Quadrangle, 1962).

[15] For a discussion of administration pressures, including comments by Senator James Pearson regarding such pressures, see *Congressional Quarterly* (Fall, 1969) 99–104. Additionally, see Thomas Halsted's "Lobbying Against the ABM, 1967–1970," a paper delivered at the Sixty-sixth Annual Meeting of the American Political Science Association, California, 1970.

[16] As Hinckley notes, "With a Republican President . . . Democrats in Congress could give clearer voice to their 'dovish' inclinations than they could in the previous Democratic administration," p. 144.

[17] For a detailed discussion of this point, see Paul Eidelberg, *The Philosophy of the American Constitution* (New York: Free Press, 1968) chapters 5, 7 and 8.

[18] See Warren Miller and Donald Stokes, "Constituency Influence in Congress," *American Political Science Review*, 57 (March 1963), 45–56, for a discussion of how constituency influence varies directly with the saliency of the issue.

[19] Russett, pp. 72–76. It should be noted that both this and the Russett study have been restricted to the Senate; it is probable that economic incentives would be a more significant factor in determining the position of members of the House.

It follows that senators who had adopted expected positions did not suffer from this type of cognitive dissonance. Additionally, they received support for the position that they had adopted by comparing their position with that of others like themselves. Hence, it is anticipated that those who adopted expected positions in 1968 would tend not to change them.

Methods

Position. A senator's position on the ABM is defined here as the extent to which his behavior indicates opposition to the establishment of an ABM system. Three scales were constructed, one for each year, by combining roll calls reported in *Congressional Quarterly* into Guttman scales. In each case, the roll calls were taken on amendments designed to kill or weaken the ABM program. Therefore, in the construction of the scales, every senator that had been paired or had announced against the amendment (i.e., in favor of ABM) was coded as a "one," and those who were paired or had announced in favor (i.e., against ABM) were coded as "zeros."

In 1968, there were six Senate votes on the ABM issue.[22] The Goodman test[23] indicated (at the .01 level) that these were indices of a single, unidimensional variable. In order to obtain the most valid and reliable scale, i.e., to minimize errors while still differentiating sizable groups, only three of the six votes (*Congressional Quarterly* Senate Votes #155, 81 and 154, in that order) were used in constructing the final Guttman scale of 1968 positions.[24] Therefore, all of the 1968 senators were classified as representing one of the four scale positions,[25] varying from zero (most anti-ABM) through three (most pro-ABM). The senators who were classified as zero's supported all of the killing amendments including the Young amendment to delete authorization for the ABM. Those classified as one's did not go as far as the Young proposal, but they were willing to reduce the army missile authorization $342.7 million, which would have had somewhat the same effect as the Young amendment. The two's supported neither of these amendments, but were willing to support the Cooper-Hart amendment delaying authorization for a year. The three's opposed all of these amendments. (See Appendix for classification of senators.)

There were five votes on the ABM issue in 1969,[26] all of which indexed a single, unidimensional variable (significant at the .01 level using the Goodman test). Again, to obtain the optimal scale, we combined only three of these votes (*Congressional Quarterly* Senate Votes #53, 211 and 54, in that order) to form the 1969 Guttman scale of positions on the ABM issue.[27] The 1969 senators were therefore also divided into four scale types. (See Appendix.) Senators classified as zero's supported all these amendments, including the first Smith substitute for the Cooper-Hart amendment, that would have prevented funds from being used for any aspect of the Safeguard ABM system. One's went almost as far, supporting deletion of Safeguard funds. Two's favored the second Smith substitute, which banned funds for Safeguard but allowed development of other ABM systems. Three's opposed all amendments.

In 1970, there were only three votes on the ABM issue.[28] While they all met the Goodman test criteria (at the .01 level), only two (*Congressional Quarterly* Senate Votes #237 and 238) could be used for the final Guttman scale.[29] Therefore, the 1970 senators were classified only as 0, 1½ or 3—anti-ABM, neutral, or pro-ABM, respectively. (See Appendix.) Those classed as zero's opposed any funds in any bill being allocated for deployment of the ABM system at any site. The one and one-half's merely opposed specific allocations for deployment of the ABM at Whiteman and Warren Air Force bases. The three's opposed both amendments.

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[20] These may both be treated as "cognitive elements," hence we have an application of the theories developed by Leon Festinger, *A Theory of Cognitive Dissonance* (Stanford: Stanford University Press, 1957).

[21] Note also Festinger's, "A Theory of Social Comparison Processes," *Human Relations*, 7 (1954), 117–140; and George Homans, *Social Behavior: Its Elementary Forms* (New York: Harcourt, 1961).

[22] *Congressional Quarterly*, Senate Votes #81, 82, 154, 155, 222, and 257.

[23] Leo J. Goodman, "Simple Statistical Methods for Scalogram Analysis," *Psychometrika*, 24 (March, 1959), 20–43.

[24] For example, we could have included *Congressional Quarterly* Senate Vote #22 in the scale, but it added one scale error and it created a scale step which included only four senators. For the scale we did compute the Coefficient of Reproducibility (.983) and Menzel's Coefficient of Scalability (.933).

[25] Except Senator Long (Mo.) who took no position on any of the votes and Kennedy (NY) who was assassinated before his position was clearly established.

[26] *Congressional Quarterly*, Senate Votes #53, 54, 55, 56 and 211.

[27] Coefficient of Reproducibility = .990; Menzel's Coefficient of Scalability = .971.

[28] *Congressional Quarterly*, Senate Votes #237, 238, and 242.

[29] *Congressional Quarterly*, Senate Votes #242, 238 had nearly identical cutoff points. The Coefficient of Reproducibility and Menzel's Coefficient of Scalability for the 1970 scale were both 1.00.

Relative Change in Position, 1968–70. Change in position refers to significant shifts, either more for or more against the ABM. This is how senators change vis-à-vis each other, not how the whole body changes vis-à-vis some outside standard.

Measuring this change would have been simpler if positions were indexed by interval data, or more discrete ordinal data. Unfortunately, given the nature of the data, senators could only be assigned to groups. This was done for the beginning (1968) and end (1970) of the time period. Thus, relative change was indexed by a change from one group to another.

To best ensure comparability of groups, equal numbers of groups were formed for each year, with corresponding groups (e.g., anti-ABM) being of roughly equal size. This partitioning could be done by simply collapsing the 1968 zero and one types to form a single anti-ABM group. The three's for each year remained the pro-ABM groups, while the other group was termed neutral, yielding the results shown in Table 1.

Table 1. Size of Groups

Year	Anti	Neutral	Pro
1968	28	14	56
1970	26	11	53

Relative change in position was then indexed by a change from one of these groups in 1968 to another in 1970. If the change was out of the anti-ABM group or from the neutral to the pro-ABM group, it was termed a *pro-ABM move*. Change out of the pro-ABM group or from the neutral to the anti-ABM group was termed an *anti-ABM move*.

Ideology. Ideology refers to how liberal or conservative a given senator's voting behavior has been relative to that of other senators. *Congressional Quarterly* (1968–70) reports Senate Conservative Coalition support and opposition scores and ACA and ADA indices for each senator for the Ninetieth Congress and for 1969, all of which tend to index ideology. Most of these measures of ideology, however, were influenced slightly by each senator's position on the ABM. To avoid a possible overlapping of the independent and dependent variables, the measures of ideology were recalculated, excluding all ABM votes from the roll calls. For as many years as the relevant data were available, the measures were combined to form an index based on the following: (conservative support score plus ACA index) minus (conservative opposition score plus ADA index). Senators were rank-ordered on the resulting index, from 1 for the most liberal (Hart) to 83 for the most conservative (Gurney). Thus, Fulbright, with a neutral index of zero, ranked forty-ninth. This rank order is reported in the Appendix.

Party Commitment. Party commitment refers to how consistently Democratic or Republican a given senator's voting behavior has been relative to that of other senators in his party. *Congressional Quarterly* (1968–1970) reports for each senator the percentage of party unity roll calls for 1969 and the Ninetieth Congress in which that senator voted with and against his party. "Party unity roll calls are those in which a majority of voting Democrats opposed a majority of voting Republicans."[30] Each senator's commitment was indexed by the percentage of his votes supporting his party minus the percentage of his votes opposing his party for the relevant sessions. All of the senators were then rank-ordered on party commitment: first the Democrats, from 1 for the most committed (Hughes) to 47 for the least committed (Allen); then the Republicans, from 48 for the least committed (Case) to 89 for the most committed (Gurney). The rank order is reported in the Appendix.

State Economic Benefit. State economic benefits refers to the relative economic advantage that would accrue to the senator's state if the ABM program were to be adopted. Economic incentive was indexed by dividing the total research and development and production contracts and subcontracts of $1,000,000 or more for each state[31] by that state's total personal income.[32] (See Appendix.)

Deviation From Expected Position. Deviation from expected 1968 position was established by comparing each senator's position with his ideological ranking. Since there were 28 anti-ABM senators in 1968, any one of the 28 most liberal senators was considered to have deviated from his expected

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[30] *Congressional Quarterly Almanac*, 1969, p. 1070.

[31] Reported, *Congressional Quarterly*, May 30, 1969, pp. 848–849.

[32] Reported, *The American Almanac*, 1970, p. 318.

Table 2. Deviants from Expected 1968 Position

A. More Pro-ABM Than Expected		B. More Anti-ABM Than Expected	
Dodd	Magnuson	Williams (Del)	Cooper
Inouye	McGee	Ellender	Scott
Bayh	Muskie	Aiken	Prouty
McIntyre	Javits	Fulbright	Church
Harris	Pell	Hatfield	Smith
Pastore	Moss	Symington	Mansfield
Yarborough	Ribicoff	Burdick	Gore
Jackson	Brewster	McCarthy	Bartlett
		Lausche	Gruening
			Morton

position if he was not in the anti-ABM group. Similarly, the 56 most conservative senators were expected to adopt a pro-ABM position. And any moderate senator (i.e., one more conservative than the 28 and more liberal than the 56) was considered to have deviated from his expected position if he was not in the neutral group[33] (Table 2).

Tests and Results

Position. It was hypothesized that the more conservative a senator, the more likely he was to adopt a position favoring the ABM. The Gamma rank-order correlations[34] reported in Table 3 lend substantial support to this hypothesis. For each year there is a close association between ideology and position, Gamma being statistically significant and higher than between any other explanatory variable and position.

It might be expected that a general measure of liberalism-conservatism, such as we have used here would not have as close an association with position as a more specific measure of ideology based solely on defense issues. But in actual fact, the general ideology index more closely associated with position on the ABM each year than did Russett's general defense index.

Our contention that economic incentives would have little effect on the position each senator adopted is strongly supported. As Table 3 shows, none of the correlations between economic incentive and position are statistically significant, and while they are positive, none exceeds .10.

It was argued that party commitment would be only a minor factor in determining position. Simply examining the degree of association between party and position will not settle that argument. Because of the anticipated closeness of association between ideology and both party commitment (Gamma = .63) and position (mean Gamma = .76), it was expected that there would be a close association between party and position even if the independent influence of party was very limited. In order to ascertain the comparative independent influence of each of the factors, and to check for significant interactions among them, the position scores for each cross-classification for each year were averaged (summarized in Table 4). The means were then subjected to an analysis of variance for each year. The analysis of variance (summarized in Table 5) clearly supports the theoretical propositions. Ideology accounts for a significant amount of the variation in position every year. It is the only variable which accounts for a significant proportion of the variance, and furthermore, in each case it accounts for more than 50 times the variance accounted for by all of the other variables together.

Party commitment and state economic benefits, alone or in interaction with other variables, never account for anything approaching a significant proportion of the variance in position. The change in administrations in 1969 apparently makes little difference as none of the other variables becomes appreciably stronger over time. Ideology is the only influential variable in 1968, and its influence grows stronger each year.

Change in Position. It was further hypothesized that if ideology proves to be very influential, then a senator whose position in 1968 was not consonant with his ideology would experience sufficient cognitive dissonance to make it likely that he would change positions in order to reduce that dissonance. On the other hand, a senator who adopted a position that was in accord with his ideology would not be expected to change positions.

Table 3. Gamma Correlations between Explanatory Variables and Position 1968, 1969, 1970

	ABM Position		
Variable	1968	1969	1970
Ideology	.65*	.77*	.86*
Party Commitment	.45*	.59*	.66*
Economic Incentive	.04	.10	.05
General Defense	−.64*	−.75*	−.78*

* Significant at the .05 level.

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[33] The procedure adopted here follows closely that used by Robert Bernstein and Ralph Meyers ("An Approach to the Study of Change Over Time," presented at the Forty-Third Annual Meeting of the Southern Political Science Association, Gatlinburg, Tennessee; November 13, 1971) in their residual model of short term change.

[34] Gamma correlations were used here and later because Gamma is the ordinal measure of association best interpretable in terms of proportional reduction in error. (See Herbert Costner, "Criteria for Measures of Association," *American Sociological Review*, 30 [June, 1965], 341–353.)

Table 4. Mean Position Scores and (Number) of Senators for all Crossclassifications[a]

		Party Influence			
		Republican		Democratic	
Year	Ideology	Economic Incentive	No Economic Incentive	Economic Incentive	No Economic Incentive
1968	Conservative	2.91	2.70	3.00	2.87
		(8)	(20)	(7)	(15)
	Liberal	1.60	1.33	1.45	1.43
		(5)	(3)	(11)	(28)
1969	Conservative	3.00	2.67	3.00	2.92
		(10)	(21)	(5)	(13)
	Liberal	1.33	1.33	1.18	1.15
		(6)	(6)	(11)	(27)
1970	Conservative	3.00	2.81	3.00	2.88
		(10)	(21)	(5)	(13)
	Liberal	0.50	1.00	0.95	0.56
		(6)	(6)	(11)	(27)

[a] 3 = strongest pro-ABM position; 0 = strongest anti-ABM position. Conservative is defined as having a positive score on the ideology index; liberal as having a negative score.

Table 6 indicates extraordinary support for the hypothesis. Of the 16 senators who changed position, 15 had deviated from their expected positions in 1968 and then moved to reduce their dissonance by 1970. Of the 56 senators who had adopted positions in accordance with their ideology in 1968, only Montoya changed his position by 1970. The Gamma correlation for Table 6 is .99 (significant at the .01 level.)

Table 5. Analysis of Variance in Position: 1968–1970

	F Ratio		
Source of Variance	1968	1969	1970
Main Effects			
Ideology (I)	33.47*	85.54*	125.66*
Party (P)	0.00	0.00	0.00
Economic Incentive (E)	0.40	0.46	0.00
Interaction Effects			
I×P	0.08	0.62	0.00
I×E	0.00	0.00	0.26
P×E	0.08	0.15	1.32
I×P×E	0.00	0.15	1.45

* Significant at the .01 level. All ratios were computed using a variation on the harmonic mean procedure for analyzing unbalanced designs suggested by Charles Gates of the Statistics Department, Texas A&M University.

In conclusion, then, the position a senator adopted on the ABM issue has been shown to reflect his ideology, not his party commitment or the economic benefits his state would derive from the adoption of the ABM. The influence of ideology seems to have grown during each successive year. Virtually all the senators who changed positions between 1968 and 1970 had initial positions which were not in accord with their ideology, and they moved so as to bring them more in accord. Virtually all those senators whose initial position was in accord with their ideology maintained that position.

Table 6. Deviation from Expected Position, Based on Ideology and Relative Change in Position, 1968–1970

	Deviation from Position, 1968		
Relative Change in Position, 1968–1970	More Anti-ABM than Expected	As Expected	More Pro-ABM than Expected
Pro-ABM move	5	0	0
None	10	55	5
Anti-ABM move	0	1	10

APPENDIX

State Senator	ABM Position 68	ABM Position 69	ABM Position 70	Ideological Ranking	General Defense	Party Commitment Ranking	State Economic Benefits
ALABAMA							
Hill	3			69	0	34	1.19
Sparkman	3	3	3	68	9	39	1.19
Allen		3	3	86		47	1.19
ALASKA							
Bartlett	1			43	23	27	0.00
Gruening	0			36	46	26	0.00
Gravel		1	0	21		19	0.00
Stevens		3	3	56		65	0.00
ARIZONA							
Hayden	3			57	9	23	2.14
Fannin	3	3	3	89	0	87	2.14
Goldwater		3	3	78		70	2.14
ARKANSAS							
Fulbright	0	0	0	49	47	30	0.00
McClellan	3	3	3	85	0	46	0.00
CALIFORNIA							
Kuchel	3			46	6	55	1.40
Murphy	3	3	3	78	0	77	1.40
Cranston		1	0	11		5	1.40
COLORADO							
Allott	3	3	3	86	5	84	0.00
Dominick	3	3	3	75		78	0.00
CONNECTICUT							
Dodd	3	3	3	39	11	25	0.36
Ribicoff	2	0	1½	15		17	0.36
DELAWARE							
Boggs	3	3	3	62	10	72	4.22
Williams	2	3	3	72	2	75	4.22
FLORIDA							
Holland	3	3	3	79	9	41	4.11
Smathers	3			67	10	38	4.11
Gurney		3	3	93		89	4.11
GEORGIA							
Russell	3	3	3	77	1	42	0.00
Talmadge	3	3	3	82	4	43	0.00
HAWAII							
Inouye	3	1	0	17		15	0.00
Fong	3	3	3	53	7	58	0.00
IDAHO							
Church	0	1	0	39	37	22	0.00
Jordan	3	3	3	77	1	80	0.00
ILLINOIS							
Dirksen	3	3		76	2	73	0.00
Percy	2	2	1½	35		58	0.00
Smith			3	60		63	0.00
INDIANA							
Bayh	3	1	0	28		14	0.00
Hartke	0	1	0	26	17	11	0.00
IOWA							
Hickenlooper	3			91	12	83	0.00
Hughes		1	0	6		1	0.00
Miller	3	3	3	76	1	74	0.00
KANSAS							
Carlson	3			73	9	69	0.00
Dole		3	3	81		81	0.00
Pearson	3	2	3	67	9	70	0.00

APPENDIX—(*Continued*)

State Senator	ABM Position 68	69	70	Ideological Ranking	General Defense	Party Commitment Ranking	State Economic Benefits
Kentucky							
Morton	1			63	11	66	0.00
Cook		1	3	51		56	0.00
Cooper	2	1	1½	47	18	59	0.00
Louisiana							
Ellender	2	3	1½	71	7	37	0.00
Long	3	3	3	74	46	36	0.00
Maine							
Muskie	2	0	0	9	19	7	0.00
Smith	1	0	0	63	6	62	0.00
Maryland							
Brewster	3			32		20	0.00
Tydings	1	1	0	10		12	0.00
Mathias		1	0	24		49	0.00
Massachusetts							
Kennedy	0	1	0	8		16	5.68
Brooke	1	1	0	25		53	5.68
Michigan							
Hart	0	1	0	1	38	6	0.10
Griffin	3	3	3	54		67	0.10
Minnesota							
McCarthy	0	0	0	31	18	28	0.62
Mondale	1	1	0	2		4	0.62
Mississippi							
Eastland	3	3	3	83	0	43	0.00
Stennis	3	3	3	88	0	44	0.00
Missouri							
Eagleton		1	0	7		3	0.00
Symington	1	1	0	37	17	26	0.00
Montana							
Mansfield	1	2	1½	45	18	10	0.00
Metcalf	0	1	0	16	18	15	0.00
Nebraska							
Curtis	3	3	3	91	0	86	0.36
Hruska	3	3	3	90	0	85	0.36
Nevada							
Bible	3	3	3	64	10	33	0.00
Cannon	3	2	3	57	9	31	0.00
New Hampshire							
McIntyre	3	2	3	29		17	0.00
Cotton	3	3	3	79	1	79	0.00
New Jersey							
Williams	1	1	0	3	18	2	18.66
Case	0	1	0	10	18	48	18.66
New Mexico							
Anderson	3	3	3	48	17	29	0.00
Montoya	2	1	0	34		20	0.00
New York							
Goodel		1	0	12		49	0.79
Javits	2	1	0	14	31	51	0.79
North Carolina							
Ervin	3	3	3	87	7	44	9.45
Jordan	3	3	3	82	7	40	9.45
North Dakota							
Burdick	1	2	1½	30	46	10	0.00
Young	3	3	3	84	2	65	0.00

APPENDIX—(*Continued*)

State Senator	ABM Position			Ideological Ranking	General Defense	Party Commitment Ranking	State Economic Benefits
	68	69	70				
Ohio							
Lausche	2			66	4	27	0.00
Young	0	0	0	13	26	10	0.00
Saxbe		0	0	41		57	0.00
Oklahoma							
Monroney	3			52	18	28	0.00
Harris	3	1	0	20		13	0.00
Bellmon		3	3	86		74	0.00
Oregon							
Morse	0			23	47	27	0.00
Hatfield	0	1	0	40		52	0.00
Packwood		3	3	44		64	0.00
Pennsylvania							
Clark	0			4	47	18	1.02
Schweiker		1	0	27		50	1.02
Scott	2	3	3	46	5	60	1.02
Rhode Island							
Pastore	3	3	1½	19	18	21	0.00
Pell	2	1	0	9	23	14	0.00
South Carolina							
Hollings	3	3	3	65		35	0.00
Thurmond	3	3	3	92	0	82	0.00
South Dakota							
McGovern	0	0	0	19		8	0.00
Mundt	3	3	3	80	3	68	0.00
Tennessee							
Gore	1	0	0	42	47	24	0.00
Baker	3	3	3	70		77	0.00
Texas							
Yarborough	3	1	1½	22	46	9	0.54
Tower	3	3	3	85	0	76	0.54
Utah							
Moss	2	1	0	15	46	12	0.00
Bennett	3	3	3	86	2	82	0.00
Vermont							
Aiken	2	2	1½	50	18	54	0.00
Prouty	2	3	3	55	9	61	0.00
Virginia							
Byrd	3	3	3	84	0	46	0.00
Spong	3	3	3	58		32	0.00
Washington							
Jackson	3	3	3	18	23	8	0.11
Magnuson	3	1	1½	30	17	12	0.11
West Virginia							
Byrd	3	3	3	59	17	30	0.00
Randolf	2	1	1½	38	17	12	0.00
Wisconsin							
Nelson	0	0	0	5		3	0.00
Proxmire	0	1	0	21	13	14	0.00
Wyoming							
McGee	3	3	3	33	18	20	0.00
Hansen	3	3	3	88		88	0.00

Need for Achievement and Competitiveness as Determinants of Political Party Success in Elections and Coalitions*

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The personalities of individual leaders and the strategies of groups, such as political parties, are important in determining the outcome of political contests. While analyses of both the individual and the group have been used extensively in studying political parties, relatively little empirical research has analyzed parties in terms of the linkages between individual and group behavior. The research that has been done in this area generally focusses on the effects that group pressures have on individual behavior.[1] That is, individual behavior has been the dependent variable, and group structure and behavior the independent variables. To my knowledge, no rigorous nonexperimental research has treated political party behavior as dependent on the collective personality attributes of the individuals who compose the group. Yet the goals and the strategies of political parties are in part determined by the predispositions of individual leaders.

This article identifies some of the linkages between the individual and the group level of analysis. The principal concern is with the impact of each leader's need for achievement[2] on his party's strategies in elections and in coalitions. In particular I will be concerned with the linkage between the need for achievement of individuals and the preferences of political parties for high, moderate, or low risks. In developing the hypotheses for this study, some reliance is placed on the experimental literature concerned with games and with need for achievement. Tests of the hypotheses, however, are restricted to the performance of political parties in actual political contests.

Two sets of dependent variables are used in this paper. The first set is concerned with the *electoral success* of parties. The second set concerns the *success in coalitions* of the same parties. Electoral success is defined by a party's ability to secure votes and win legislative representation. Success in coalitions is defined by the ability to gain control over the important resources of a coalition government, such as key cabinet portfolios. In short, a party's performance in both elections and coalitions may be defined as the ability to secure control over scarce, valuable resources.

Under conditions of scarcity, keen competition for control over valuable resources is inevitable. Where competition exists, success usually accrues to that group which combines the best mix of *resources* and the *skills* necessary to mobilize and utilize those resources. Size, or representation, is probably one of the most important resources of political parties, but size alone usually is not sufficient to insure success. History is replete with examples of major parties that, through a succession of defeats, lost most of their influence.[3] Sheer size, or representation, then, is important, but the ability to use resources wisely is probably more important. Thus, one may defeat a stronger adversary provided one can mobilize the *right* resources and make the *right* strategic choices. Indeed, there is evidence that when a party surpasses

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* The field research for this project was made possible by a generous grant from the American Institute of Indian Studies. I wish to express my gratitude to Richard Park, A. F. K. Organski, and Donald Stokes who gave me many helpful comments on an earlier version of this article. Rajni Kothari and Marcus Franda were of great assistance to me while I was in India. Finally I want to thank the many Indian politicians whose cooperation made this research possible.

[1] Angela S. Burger, *Opposition in a Dominant-Party System: A Study of the Jan Sangh, the Praja Socialist Party, and the Socialist Party in Uttar Pradesh, India* (Berkeley: University of California Press, 1969); Paul R. Brass, *Factional Politics in an Indian State: The Congress Party in Uttar Pradesh* (Berkeley: University of California Press, 1965); Michael Brecher, *Political Leadership in India: An Analysis of Elite Attitudes* (New York: Praeger, 1969); Thomas A. Flynn and Frederick M. Wirt, "Local Party Leaders: Groups of Like-Minded Men," *Midwest Journal of Political Science*, 9 (February, 1965), 77–98; Samuel C. Patterson, "Characteristics of Party Leaders," *Western Political Quarterly*, 16 (June, 1963), 332–352; M. Margaret Conway and Frank B. Feigart, "Motivation, Incentive Systems, and the Political Party Organization," *American Political Science Review*, 62 (December, 1968), 1159–1173; Bruce Bueno de Mesquita, *Personality, Risk and Coalitions: The Case of India* (New York: Cambridge University Press, 1975).

[2] John W. Atkinson, *An Introduction to Motivation* (Princeton: Van Nostrand, 1964); John W. Atkinson, ed., *Motives in Fantasy, Action, and Society: A Method of Assessment and Study* (Princeton: Van Nostrand, 1958); David C. McClelland, *The Achieving Society* (Princeton: Van Nostrand, 1961).

[3] A few examples include the British Liberal Party, the American Federalist Party, and the Muslim League in India.

some threshhold in legislative representation its size militates against its ability to implement its best strategy for electoral and coalitional success.[4] From this discussion it follows that within certain parameters—including a minimal and an optimal size—the probability of achieving success under conditions of competition increases when a party is willing and able to make the appropriate strategic choices.

If the selection of contending strategies is a major determinant of party performance, then it is important to identify the determinants of strategic preferences among political parties. It is a hypothesis of this study that the attitude or *predisposition* that party leaders have toward risk taking determines their strategic preferences. These predispositions, in turn, are hypothesized to be an important predictor of party success. The covariation between risk-taking preferences on the one hand, and party performance on the other, then, is one of the central themes of this study.

The Model

In the next few pages I will develop a model intended to account for (a) the precoalition electoral success of political parties; (b) the success of the same parties in coalition governments; and (c) the postcoalition electoral success of those parties. The principal independent variables are the *strategic and motivational predispositions* of political party leaders.

The derivation of the model follows from these assumptions:

(1) Political parties compete in elections because they want to increase their organizational strength and legislative representation.
(2) When political parties form coalitions, some of them are more concerned with the potential electoral gains they will derive from a coalition than with fulfilling the immediate policy objectives of the coalition.
(3) (a) A political party will attempt to remain in a coalition as long as its leaders perceive that their party is not suffering a net loss from the alliance.
    (b) If the leaders perceive that their party is losing by remaining in the coalition, they will withdraw the party from the coalition.
(4) One large loss is more readily perceived than a comparable set of small losses.
(5) The more recent a loss, the more salient it is to a party's leaders and the greater its impact on their perception of the costs and benefits of the coalition.

Assumption 1 merely suggests that most parties are pragmatic, mass-oriented electoral parties; only a few are primarily ideological.[5] Assumption 2 contends that some pragmatic parties behave differently when they achieve power than they do in elections. That is, some parties use coalition governments primarily to fulfill their programmatic, or policy, goals, while other parties attempt to exploit the coalition's resources primarily to enhance their future electoral prospects. The third assumption posits that party leaders behave rationally in the context of their perceptions and their goals. Finally, the fourth and fifth assumptions maintain that political leaders, like all people, do not have perfect memories. Thus, they evaluate the costs or benefits of a particular decision from the perspective of the most recent experiences of the party.[6] Leaders therefore are more likely to tolerate a nibbling away of their resources than the loss of large portions of their resources in a single bite. That is, given imperfect memories, leaders compute the seriousness of some specific loss in terms of their base of strength at the time of the loss. They are less careful to compute the seriousness of the cumulative impact of such losses over an extended period of time.

Among the implications that may be derived from these assumptions are the following: (1) Different parties give different degrees of priority to fulfilling their policy goals as compared to their electoral goals. (2) Some parties will be satisfied (or at least not seriously dissatisfied) with less than their fair share of the coalition's payoffs.[7]

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[4] For a theoretical discussion of this point see William H. Riker, *The Theory of Political Coalitions* (New Haven: Yale University Press, 1962); William A. Gamson, "Experimental Studies of Coalition Formation," in *Advances in Experimental Social Psychology,* ed. Leonard Berkowitz (New York: Academic Press, 1964); Theodore Caplow, "Further Developments of a Theory of Coalitions in the Triad," *American Journal of Sociology,* 64 (March, 1959), 488–493; Bueno de Mesquita, *op. cit.;* and Jerome M. Chertkoff, "Sociopsychological Theories and Research on Coalition Formation," in *The Study of Coalition Behavior: Theoretical Perspectives and Cases from Four Continents,* ed. Sven Groennings, E. W. Kelley, and Michael Leiserson (New York: Holt, Rinehart and Winston, 1970). Nonexperimental evidence for this point may be found in Peter H. Merkl, "Coalition Politics in West Germany," in Groennings et al.

[5] Joseph G. LaPalombara and Myron Weiner, eds., *Political Parties and Political Development* (Princeton: Princeton University Press, 1966); and William E. Wright, "Ideological-Pragmatic Orientations of West Berlin Local Party Officials," *Midwest Journal of Political Science,* 11 (August, 1967), 381–402.

[6] Melvin Guyer, "A Review of the Literature on Zero-Sum and Non-Zero-Sum Games in the Social Sciences," Mental Health Research Institute, University of Michigan (mimeo), n.d.

[7] Gamson, "Experimental Studies," in Berkowitz; Mancur Olson, Jr., *The Logic of Collective Action: Public Goods and the Theory of Groups* (Cambridge: Harvard University Press, 1965).

(3) Parties have different tolerance limits for dissatisfaction or relative gains and losses.

One of the most important decisions all coalition governments must make is the assignment of cabinet portfolios. On this decision depend both the performance of government functions, and the control over patronage, publicity and the establishment of grass-roots contacts and organizations which are so important to party growth. Given these stakes, why do some parties accept a disproportionately smaller share of important portfolios than other parties do? The preceding assumptions will be used to derive hypotheses concerning the distribution of important coalition payoffs.

It is an old adage that the squeaking wheel gets the oil. Perhaps in much the same way, a party that asks for more has a higher probability of getting more than a party that does not make any special demands. But how much more can it demand before it surpasses the limits of tolerance proposed by assumptions 3 and 4? I hypothesize that the distribution of benefits favors parties predisposed to pursue a mixed competitive/cooperative strategy at the expense of those parties preferring either pure cooperative or pure competitive strategies. Why?

In order to answer this question, I must first define some key terms. A party's strategic orientation is defined as cooperative if its leaders are predisposed to accept less than their fair share of the coalition's private benefits in the interest of achieving the coalition's collective goals.[8] Conversely, a competitively oriented party is one whose leaders are predisposed to sacrifice the collective goals of the coalition in the interest of their own party's pursuit of private benefits.

Cooperative parties prefer to distribute coalition payoffs (or private benefits) so as to minimize disruptive bargaining over individual benefits and maximize attention given to the policy goals of the alliance.[9] Because of their attentiveness to policy goals, cooperative parties attempt to distribute private benefits in accordance with an equity norm.[10] Such a decision rule would give each party a proportion of the private benefits that is commensurate with its contribution to the strength, or resource base, of the coalition. If a member of the coalition demands more than its "fair share" of the payoff, the cooperators meet that demand if they believe it will help facilitate the attainment of the coalition's policy goals. Of course, if the demand is so great that it violates assumption 3, the cooperators withdraw from the coalition rather than sacrifice any additional resources. In this way, they avoid any further erosion of their position, thereby preserving enough of their resources to help insure their influence in future coalitions. Insofar as such a demand does not arise, the dependence on an equity rule for distributing payoffs does, incidentally, help maintain the relative representational and organizational status quo between the parties within the coalition. Thus, if each party receives its fair share of patronage, publicity, and grass-roots contacts, then each party may expand its organizational and electoral base relative to those outside the coalition, but *ceteris paribus*, no party within the coalition will achieve disproportionately more than any other party within the coalition. Consequently, a completely cooperative coalition is in the best interest of the largest party, but not in the best interest of ambitious, smaller parties.

Competitive parties prefer to distribute the private benefits of the coalition in accordance with a non-equity rule. That is, by behaving competitively they attempt to convince their more cooperative partners that they are willing to disrupt the coalition's quest for the collective goals unless the cooperators give part of their share of the private benefits to the competitors. Of course, by sacrificing some of their share, the cooperators are surrendering a portion of the long-term growth of their party. Because the sacrifice of such resources is costly to the cooperators, it turns out that competitors who always pursue their private benefits at the expense of their partners can acquire a disproportionately large share of the payoff only for a limited time. Once they demand too large a portion, or demand one resource too many, the cooperators will lose their incentive to remain in the coalition. At this point, the coalition is terminated, with the competitor losing access to the private benefits it values, and the cooperator losing access to the collective benefits it values. The precise time when this occurs will depend, according to assumptions 4 and 5, on whether competition takes the form of constant demands or only of occasional, small demands for sacrifices. A completely competitive party, by definition, makes constant demands, while a party following a mixed strategy makes occasional demands.

If a party pursues a mixed strategy, sometimes facilitating the attainment of policy goals, and sometimes demanding extra incentives for its support of these goals, it should be effective in using the coalition to increase its organizational and representational potential. In fact, by nibbling away at the resources of its coalition partners, the mixed-strategy party should be able, in the long run, to gain more private benefits from a coalition than a completely competitive party would. The mixed-strategy party depends on two factors to insure its long-run success in coalitions. It depends on the faulty memory of its allies, and on its ability to convince its cooperative partners that a small sacrifice increases *everyone's* opportunity to enjoy access to all the benefits of the coalition for the longest possible time.

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[8] Olson, *Logic of Collective Action;* Mancur Olson, Jr. and Richard Zeckhauser, "An Economic Theory of Alliances," *Review of Economics and Statistics,* 48 (August, 1966), 266–279.

[9] Gamson, in Berkowitz.

[10] The equity norm is defined as follows: If a party contributes X% to the legislative strength of a coalition, it is entitled to X% of the coalition's payoffs.

From this discussion, two hypotheses follow:

H1: There is a positive association between the predisposition to pursue a mixed competitive/cooperative strategy, and the acquisition of a relatively large share of the important payoffs in a coalition.

and

H2: There is a positive association between the pursuit of a mixed strategy and the subsequent electoral success of political parties.

Need for Achievement

Deciding to pursue a competitive, cooperative, or mixed strategy depends, at least in part, on the attitude party leaders have toward taking risks.[11] If, for example, the leaders want to minimize the probability that their party will be ousted from a coalition government, then they behave as cooperatively as possible toward their partners. Regardless of the strategy chosen by the other members of the coalition, a cooperator is always a welcome partner, and as such, may expect to remain in a coalition government until it chooses to withdraw. Of course, in cooperating, a party implicitly accepts a high probability that its private benefits from the coalition will be relatively small in return for continued membership and the opportunity to pursue the collective goals of the alliance.

If a party's leaders choose to risk continued membership in a coalition government, in the hope of securing substantial benefits quickly, they behave very competitively toward their partners. By doing so they have the opportunity to secure large gains before being ousted from the government. Of course, they also run the risk that the other coalition members will balk at their demands before they can convert their new resources into real strength. Thus, offering patronage which you lose control of, or receiving unfavorable publicity may reduce a party's future electoral appeal, rather than enhance it. Such unfavorable consequences are more likely to be experienced by very competitive parties than by either very cooperative or mixed-strategy parties.

If the leaders of a party are predisposed to take moderate risks, their best choice is a mixed strategy, which allows them to make several incremental gains—in legislative seats, in popular votes, and in important cabinet portfolios. Because the party is not always competitive, it can capitalize on at least some of the extra patronage, publicity and grass-roots organizing that it has enjoyed. Nevertheless, it does run a higher risk than a cooperative party of being ousted from the coalition. Put somewhat differently, cooperative parties have a low probability of securing a disproportionate share of the coalition's private benefits at any point in time and a low probability of being ousted from the coalition. Mixed-strategy parties have a higher probability of securing extra payoffs at any point in time, and a higher probability of being ousted from the coalition and thereby being cut off from those benefits in the long run. Finally, competitive parties have the highest probability of securing a disproportionate share of the coalition's private benefits at any point in time, and the highest probability of losing access to those benefits in the long run by being ousted from the coalition. Thus, cooperation is a low-risk strategy, competition is a high-risk strategy, and the mixed strategy involves moderate risks.

How do parties decide among a high-risk competitive strategy, a moderate-risk mixed strategy, or a low-risk cooperative strategy? One attribute which influences this decision is the degree of need for achievement manifested by the party's leaders. Atkinson and others have repeatedly found that risk-taking preferences are significantly associated with a person's need for achievement. People above the mean in the need to achieve (N-Ach) tend to prefer activities involving *moderate* risks, while people below the mean prefer either high or low-risk activities.[12] This variable is a particularly reliable indicator of risk-taking preferences in competitive situations that the participants evaluate as being important to them. Elections and coalitions clearly are both competitive situations in which the outcome is important to the participants. (Assumptions 1, 2, and 3 deal with this point.) We may conclude, therefore, that parties whose leadership is high on the need to achieve are more likely than the others to implement a mixed strategy and, thereby, gain disproportionately both within the coalition and in postcoalition elections. But how does need for achievement help to account for the precoalition success of political parties?

Parties must make important strategic decisions when they decide which constituencies to contest and which to ignore in political systems characterized by single-member districts. Of course, no party would willingly forego the opportunity to contest a constituency it is almost certain to win. Thus, virtually no variance is expected in the probability of contesting sure constituencies because of differences in need for achievement. Parties must frequently choose, however, between committing the bulk of their resources to contests they expect to be close or channeling some of those resources into constituencies where they have a low probability of winning. In a close constituency, the probability of success approaches the 50:50 level. That is, a party could reasonably expect to win half the time and to lose half the time. Contesting such a constituency involves, by definition, a moderate risk. Contesting a remote constituency, on the other hand, involves a high probability of defeat.

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[11] Guyer, "Review of the Literature."

[12] Atkinson, *Motives*.

Why would any party commit resources to remote constituencies? First of all, success in such contests is an indication that the base of the party is expanding. A good showing in a remote constituency helps to increase the overall visibility of a party. A poor performance in a remote constituency, on the other hand, is not unexpected and so probably does not detract significantly from the party's image or base of support. Thus, little support is lost by failure, and some support may be gained by success in remote constituencies. The decision, however, to commit resources to remote contests does incur costs. Even if the party contests all of its potentially close constituencies, its resources are diverted if it is also contesting remote constituencies. The failure to concentrate its efforts in close constituencies may have serious repercussions for the future growth of the party which outweigh the gains derived from scattered successes in remote constituencies.

What kind of leaders are likely to prefer that their party pursue close contests, and what kind are likely to lead the party into remote constituencies? If the experimental findings are applicable here, we would expect leaders high in need for achievement to prefer the 50:50 chance of victory offered by close constituencies, while leaders low in need for achievement are expected to prefer high-risk remote constituencies. Because this difference in risk-taking preferences affects the selection of constituencies, parties with leaders who score high in the need to achieve should perform better in both pre- and postcoalition elections than parties low in need for achievement.

The discussion above can be restated in the form of the following hypotheses:

H3: Parties whose leaders are *above* the mean in need for achievement within their political system are likely to contest close constituencies, while parties *below* the mean are more likely to contest remote constituencies.

H4: Need for achievement is positively associated with the acquisition of important coalition benefits.

and

H5: Need for achievement is positively associated with the pre- and postcoalition electoral performance of political parties.

Research Design

A test of these hypotheses requires (a) indicators of party performance; (b) indicators of strategic preferences; and (c) indicators of need for achievement. The appropriate political system in which to gather data for these variable sets is one that has experienced several coalition governments composed of a broad spectrum of political parties. That system must also have experienced at least one pre- and one postcoalition election. Eight Indian states meet these criteria within a fairly brief, and comparable, time span. These states are Uttar Pradesh, the Punjab, Bihar, Madhya Pradesh, West Bengal, Kerala, Orissa, and Haryana.

India is an excellent political system for testing the hypotheses presented in this article for several other reasons as well. There probably is no other political system with as great a potential for developing diverse forms of government. Certainly, no other nation has experienced the diversity of governments that India has since 1967.

The widely differing experiences with coalition behavior that the Indian states have undergone provide an abundant opportunity for the formation and examination of a broad spectrum of generalizations. Since 1967 there have been more than twenty different coalition governments in the Indian states. In West Bengal, and to a lesser extent in Kerala, the governments have been based on common programs agreed to by ideologically homogeneous political parties of the left, while in Orissa there has been a coalition between ideologically like-minded parties of the right. In Uttar Pradesh, Bihar, Madhya Pradesh, Haryana, and the Punjab, the coalitions have consisted of parties with extremely disparate ideologies. Thus, in India we are able to examine the strategies used by parties in rightist, leftist, and nonideological alliances.

The Indian electoral and coalitional experiences are also representative of the diverse possibilities for governments in terms of the *number* of parties involved in the various state elections and coalitions. In Orissa, for example, there was a two party government between 1967 and 1971, while in West Bengal a coalition of no less than fourteen parties was formed. The other state governments have fallen at various intervals between these two extremes.

Table 1. Parties Studied, By State

Party	State
Jana Sangh	Uttar Pradesh
Communist Party of India (CPI)	Uttar Pradesh
Samyukta Socialist Party (SSP)	Uttar Pradesh
Praja Socialist Party (PSP)	Uttar Pradesh
Swatantra Party	Uttar Pradesh
Jana Sangh	Bihar
CPI	Bihar
SSP	Bihar
PSP	Bihar
Jana Sangh	Punjab
CPI	Punjab
Swatantra	Orissa
Communist Party of India (Marxist) (CPIM)	West Bengal
CPI	West Bengal
SSP	West Bengal
PSP	West Bengal
CPIM	Kerala
CPI	Kerala
SSP	Kerala
Jana Sangh	Madhya Pradesh
SSP	Madhya Pradesh
PSP	Madhya Pradesh

India is, in short, an excellent source of data for testing the model presented above because it is more representative than any other single political system of the entire spectrum of political ideologies, with a broad range of numbers of parties competing against each other in single-member districts, and a large assortment of coalition structures and behaviors. Thus, by being tested in India the model attains greater external validity than if it were tested in any other single parliamentary system.[13]

The principal data of this study come from extensive interviews with prominent political leaders in all the above states but Haryana. These interviews serve as the source for indicators of strategic preferences and elite need for achievement. The indicators of electoral performance are derived from several published sources of Indian voting statistics.[14]

Of the more than twenty coalition governments investigated in this article almost all included both national and regional parties. For the purposes of this study only the state branches of national parties that were represented in at least one coalition cabinet within their state are included in the analysis. Using this criterion, we consider the 21 state party branches of national parties listed in Table 1. National parties were selected because their participation in coalitions in several states facilitates cross-state comparisons of performance, and because their national-level aspirations play an important part in defining the significance of risk taking behavior for them. Regional parties, unlike the national parties, often have such a secure hold over their constituencies that they need not worry about the electoral repercussions of their risk-taking behavior in coalitions. They simply are not very likely to lose their base of support. National parties, on the other hand, are considerably more vulnerable to the consequences of risk-taking behavior in coalition governments. That is, regional parties can afford to be primarily ideological, while national parties with greater aspirations for power are generally compelled to be more pragmatic.[15] Selecting national parties also has the advantage of permitting an evaluation of the model's predictive power in national parliamentary elections (i.e., Lok Sabha) as well as in state legislative assembly elections (i.e., Vidhan Sabha).

We assume that the predisposition of a party to pursue a competitive, cooperative, or mixed strategy is equivalent to the individual predispositions of its principal leaders. A party's need for achievement is also defined as being equivalent to the individual achievement motivation of its leaders.[16] The principal leaders of a state party branch of a national party between 1967 and mid-1970 included any individual who (a) held the office of president, vice president, or secretary (or their equivalent) in one of the parties listed in Table 1;[17] (b) was a full cabinet minister in a coalition government (but not a deputy minister, or state minister); or (c) was named as a principal leader in the course of my interview with a majority of the people of the same party who met either of the above criteria. Through the use of these overlapping criteria, 119 individuals were identified as the relevant population. Of these, 88, or nearly seventy-five per cent, were interviewed by the author.[18]

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[13] Donald T. Campbell and Julian C. Stanley, *Experimental and Quasi-Experimental Designs for Research* (Chicago: Rand McNally, 1963).

[14] Craig Baxter, *District Voting Trends in India: A Research Tool* (New York: Columbia University Press, 1969); R. Chandidas, Leon Clark, Richard Fontera, and Ward Morehouse, eds., *India Votes, A Source Book on Indian Elections* (Bombay: Popular Prakashan, 1968); India Office of the Registrar General, *Fourth General Election: An Analysis* (New Delhi: Ministry of Information and Broadcasting, 1967).

[15] Bueno de Mesquita, chapter 4.

[16] Alvin Zander and John Forward, "Position in Group, Achievement Motivation, and Group Aspirations," *Journal of Personality and Social Psychology,* 8 (March, 1968), 282–288.

[17] Further discussion of the criteria for selecting subjects, all of whom were interviewed between October, 1969 and June, 1970, may be found in Bueno de Mesquita.

[18] The 31 potential subjects who were not interviewed were missed generally because of logistic or

Each respondent's level of need for achievement was assessed with a set of open-ended, projective questions concerning the individual's and the party's future prospects and hoped-for accomplishments. These responses were examined for the presence of achievement imagery according to the revised coding scheme developed by Joel Raynor.[19] The questions used in the measurement of need for achievement are found in the appendix to this study.

From the individual need for achievement scores, the arithmetic mean was calculated to determine each party's achievement level. Although this computation is probably an oversimplification of the true relationship between a party's need for achievement and the achievement motive of each of its leaders, the experimental literature does lend some credence to the procedure.[20] In addition, other nonexperimental studies suggest that averaging does not commit as grave an injustice to reality as would several available alternative procedures.[21]

After the need for achievement level was computed for each state party unit, an index of the relative need for achievement of the parties was computed using the following procedure:

$$\text{N-Ach} = \frac{X_{ij}}{\sum_{i=1}^{n} X_{ij}} - 1/N$$

where X_{ij} is the need for achievement level of party i in state j and N is the number of national parties that participated in at least one coalition with i in state j. (A party is counted only once per state no matter how many of the state's coalitions it participated in.)

The achievement index reflects the extent to which a party's leaders' need for achievement was above or below the mean *in its state.* A positive sign indicates a party whose leaders were high in the need to achieve, while a negative sign indicates a party whose leaders were low in the need to achieve. This indicator is more meaningful than the raw need for achievement score of the party's leaders because it reflects the strength of a party's achievement motive relative only to those parties that it must actually compete against. The raw scores, on the other hand, reflect the distribution of need for achievement across parties that are not necessarily involved in competition with each other.

Before concluding this section, I should mention that there are a host of potential sources of error in the need for achievement variable. Some result from linguistic problems, some from the situational context, and still others from our virtually complete ignorance of the reliability of interview data for coding achievement imagery. The reader who is interested in these and related measurement problems is referred to an extensive discussion presented elsewhere.[22] Fortunately, it is highly unlikely that the achievement scores are contaminated by the actual degree of success experienced by the individuals interviewed for this study. Need for achievement is an extremely stable personality attribute which, in several long-term panel studies, has not been found to fluctuate with the waxing and waning of personal success. Instead, each individual's score remains essentially constant across time and across life's experiences.[23] Therefore, it is safe to assume that whatever measurement error there is in the achievement index is probably random and unbiased. Such error tends to reduce associations rather than inflate them.

The strategic *predispositions* of the parties were measured from the leaders' responses to a series of questions that could have elicited competitive or cooperative or other replies. The questions presented hypothetical situations that might arise in a variety of coalition settings. The leaders were asked to specify the ways in which they would respond to such situations. All the questions were open-ended so that the respondent could elaborate on the problem-solving techniques he would employ in the situations described.

Responses that indicated a preference for actions that would aid the respondent's party *at the expense of the other members of the coalition* were judged competitive. Responses that indicated that the respondent was willing to sacrifice some benefit that his party might have acquired in *order to*

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scheduling difficulties. Many of them were defeated in the 1969 midterm election and so returned to their homes which were often situated in remote villages or inaccessible regions of the country. One potential respondent was in jail, and three were engaged in prolonged hunger strikes at the time it would have been feasible for me to interview them. Among the 88 who were interviewed, 83 were interviewed in English, I interviewed 1 in Hindi, 3 others were interviewed in Hindi, with the aid of an interpreter, and 1 was interviewed with an interpreter in Malayalam. Other details concerning the interviews may be found in Bueno de Mesquita, Appendix II.

[19] Joel Raynor, "Preliminary Notes on the Coding of the Professor's *Interview on Job and Job Pressures* for *n* Achievement, *n* Affiliation, and *n* Power," (Mimeo, Department of Psychology, University of Buffalo, March 28, 1967). Raynor's coding scheme is a slightly modified version of the standard coding procedures for assessing *n* Achievement. These procedures are described in John Atkinson, ed., *Motives.* The major modifications involve the use of controls for the length of time a respondent takes to answer interview questions.

[20] Zander and Forward.

[21] Bueno de Mesquita, chap. 6.

[22] *Ibid.,* chap. 4 and Appendix II.

[23] Atkinson, *Motives.*

protect the interests of the coalition were judged cooperative. For instance, one of the questions each respondent was asked was "Would you rather win a legislative victory that would lead to the break up of a coalition your party was in, or would you rather have the coalition remain intact and not win the victory?" If the respondent preferred to win a legislative victory at the cost of the coalition's survival, he received a competitive score on this question. If he preferred to give up the legislative victory and, thereby, preserve the coalition, he received a cooperative score on this question. To be included in the subsequent analyses of strategic predisposition, the respondent had to score as cooperative or competitive on at least one of the eight questions in this series. Only two respondents did not meet this requirement. From the raw scores each party's strategic predisposition, or *strategy index*, was computed using the following formula:

$$\mathrm{SI} = 1 - \left|\frac{f_1 - f_2}{f_1 + f_2}\right|$$

f_1 is equal to the frequency of competitive responses for the leaders of a given party, and f_2 is equal to the frequency of cooperative responses for the leaders of the same party. The strategy index assesses the extent to which a party deviates from a purely cooperative or purely competitive strategy, without regard for the direction of its deviation. Thus, a score of 0.00 indicates that a party is predisposed to pursue either a purely cooperative or a purely competitive strategy. A score of 1.00 indicates that the number of competitive and cooperative responses given by the leaders of that party are balanced.

The strategy index (SI) is a measure of a party's *predisposition* to compete or cooperate and *not* a measure of actual behavior. Indeed, the very questions from which the index was derived dealt with situations that logically could have occurred but never actually did. All other things being equal, one can reasonably assume that strategic behavior is highly correlated with strategic predispositions. This inference is helpful, though not essential to the utility of the model being tested here.

Data Analysis

Hypothesis 1 says that parties predisposed to follow a mixed strategy are more likely than others to secure a disproportionate share of the important payoffs in a coalition. Earlier I defined important payoffs as the percentage of key portfolios controlled by a party.[24] A key portfolio was one that involved control over a substantial amount of patronage, publicity, and grass-roots contacts. The relative importance, or desirability, of each cabinet portfolio was determined from a question asking the party leaders to specify the portfolios they would most like to have under their own control. The portfolios were then ranked according to the frequency with which they were named. Eight portfolios were named most often: Home, Finance, Agriculture, Education, Labour, Land and Land Revenue, Power and Irrigation, and Industries.[25] Each party's success at acquiring important coalition payoffs was computed from the percentage of these important portfolios controlled by each party. This percentage indicated their absolute control over these ministries. Hypothesis 1, however, is concerned primarily with whether a party received a disproportionate share of these portfolios, rather than with the actual amount it received. To measure disproportionality, it was assumed, in accordance with the equity norm, that a fair distribution would result if each party received a percentage of important portfolios equal to the percentage of seats it contributed to the legislative strength of the coalition. Thus, if a party received a larger percentage of portfolios than the percentage of seats it contributed to the alliance, its share was disproportionate. In fact, if the number that results from dividing the first of these percentages by the second is greater than one, a party is receiving a disproportionately large share, and if it is less than one, it is receiving a disproportionately small share. This measure is termed the ministerial index (MI).

The correlation between the strategy index and the ministerial index is .21 ($N = 20$). Although this correlation is fairly weak, it is in the predicted direction, and does lend some support to the hypothesis.

Hypothesis 2 suggests that the greater a party's predisposition to follow a mixed strategy, the better it does in postcoalition elections. This hypothesis was tested by computing the correlations between the strategy index and (a) the number of seats each party won in the 1969 midterm elections (which occurred following the termination of the first round of coalitions); (b) the percentage of votes won in 1969; (c) the number of seats won by the parties in the 1971 national parliamentary (Lok Sabha) elections; (d) the percentage of votes won in the 1971 Lok Sabha elections; (e) the change in state legislative assembly (Vidhan

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[24] Michael Leiserson, "Factions and Coalitions in One-Party Japan: An Interpretation Based on the Theory of Games," *American Political Science Review*, 62 (September, 1968), 770–787.

[25] The respondents were explicitly asked not to include the Chief Ministership among their choices. This was done because it was assumed this choice would add no additional variance since all the respondents almost certainly would have wanted the highest office in the government.

Sabha) seats won between 1967 and 1969; (f) the change in votes won between 1967 and 1969; (g) the change in the number of Lok Sabha seats won between 1967 and 1971; (h) the change in Lok Sabha votes won between 1967 and 1971; (i) the percentage change in Vidhan Sabha seats; and (j) the percentage change in Vidhan Sabha votes. The results are presented in Table 2.

An examination of Table 2 reveals several interesting patterns. First of all, each of the coefficients is in the predicted direction. While one of them is only negligibly positive, four are sufficiently large, given the number of observations, to be considered statistically significant if a random sample had been used. Although we cannot, strictly speaking, generalize about their significance, probability tests are useful here as a means of comparing the relative magnitude of correlation coefficients based on different numbers of observations. It is for this purpose that significance levels are reported in this paper.

The four significant coefficients are especially interesting because they occur with the measures of *change* in party performance over time. In each instance the initial year used to measure the party's performance is a precoalition year. That is, it is a time period before the parties could implement their strategic predispositions within the framework of a ruling government. The strategy appears to have paid off handsomely. Thus the correlations suggest that the greater a party's predisposition for a mixed strategy, the greater the improvement in its ability to win seats in *both* the state assembly and national parliamentary contests. Furthermore, the greater its orientation toward a mixed strategy, the greater the improvement in its vote-getting ability in Lok Sabha contests. Correspondingly, the ability to win votes in the state assembly elections increases with orientation toward a mixed strategy, but it is not as strong. In general, we may conclude that the evidence lends considerable support to the second hypothesis.

Hypothesis 3 is concerned with the extent to which need for achievement affects a party's propensity to take moderate risks. Risk taking here is operationalized by assessing the propensity each party had in 1967 to contest legislative assembly constituencies in which it had a reasonable or a remote chance of winning. The Indian Election Committee defines a constituency as being reasonable, or *close*, for a party if that party wins or loses the constituency by fewer than 5,000 votes. Constituencies which are won or lost by more than 15,000 votes are all defined as *remote*. Winning by more than 15,000 votes is taken as an indicator that a party took a low risk, while losing by that much suggests a high risk. Close constituencies approximate a fifty-fifty chance of winning, and so involve a moderate risk. As noted earlier, all parties are expected to pursue all constituencies that they are very likely to win, regardless of their need for achievement level. In fact, this is borne out by the evidence. The parties whose leaders were high in need for achievement contested 19 constituencies that were safe for them (out of a total of 678 constituencies contested), while the parties whose leaders were low in need for achievement contested 20 such constituencies (out of a total of 731 contested).

Table 2. Strategy and Postcoalition Performance

	Strategy Index (SI)	*N*
Vidhan Sabha Seats, 1969	.38	18
Vidhan Sabha Votes, 1969	.15	15
Lok Sabha Seats, 1971	.37	21
Lok Sabha Votes, 1971	.07	12
Change in Vidhan Sabha Seats, 1969–1967	.56*	18
Change in Vidhan Sabha Votes, 1969–1967	.38	15
Change in Lok Sabha Seats	.42*	21
Change in Lok Sabha Votes	.58*	12
Percentage Change in Vidhan Sabha Seats	.66**	19
Percentage Change in Vidhan Sabha Votes	.35	15

* significant at the .05 level or better
** significant at the .01 level or better

The risk taking indicator being used does not, strictly speaking, reflect the *subjective probability* of success perceived by each party in each constituency. Rather, it reflects the *post hoc* probability of success. One might argue that this indicator should not be used to express each party's awareness of its likelihood of success or failure in these constituencies. This argument, however, is probably incorrect. I have excluded from the analysis those constituencies that were won or lost by more than 5,000 votes but fewer than 15,000 votes. These constituencies, rather than the extreme cases I have selected, are likely to have been the most difficult for the parties to formulate reasonable expectations about. It is unlikely that any party would systematically assess its chances as good across the several constituencies where it was severely defeated. Similarly, it is unlikely that many parties systematically evaluate very close constituencies as offering a remote probability of success or failure. In any event, even if these perceptual errors did occur, there is no reason to believe that they would be systematically related to a party's aggregate level of need for achievement. Thus, insofar as the indicator used reflects

Table 3. The Association Between the Number of Political Parties Preferring Close or Remote Constituencies and the Parties' Levels of Need for Achievement[a]

		Constituency Risk	
		Close	Remote
Need for Achievement	High	5	2
	Low	3	5

$Q = .61$
$\phi = .34$

[a] Based on 15 political parties

such perceptual errors, these errors are probably randomly distributed across the parties under investigation.

In the first analysis, the percentage of close constituencies contested by each party is treated as the indicator of its propensity to take moderate risks. If a majority of the constituencies contested by any given party was close, then the party is assumed to prefer moderate risks. If, on the other hand, the majority of a party's contests were in remote constituencies, then the party is assumed to prefer high risks. If the hypothesis is to be supported, then most parties with leaders who have relatively high need for achievement should prefer close constituencies, while most parties with leaders who have low need for achievement should prefer remote constituencies. Table 3 presents the relationship between need for achievement in parties and their risk-taking orientation in selecting constituencies to contest. As we can see, high achievement parties do prefer close constituencies and low achievement parties prefer remote constituencies, as predicted. The strength of this difference can be determined by computing two simple measures of strength of association. The first, Yule's Q, reflects the extent to which some condition, such as high need for achievement tends to accompany some other condition, such as contesting close constituencies. For Table 3, Yule's Q is a fairly substantial .61.

The second measure of association (ϕ) assesses the extent to which high need for achievement is accompanied by a disproportionately great tendency to avoid remote constituencies, while low need for achievement is accompanied by a disproportionately great tendency to contest remote constituencies. For Table 3 the ϕ coefficient is .34. Although the ϕ coefficient is only moderately large, ϕ, like Q, is in the predicted direction and so does support the hypothesis. From these two coefficients we may conclude that high need for achievement very frequently occurs with a preference for close constituencies, while low need for achievement disproportionately occurs simultaneously with a preference for remote constituency contests.

The hypothesis can also be examined another way. Whereas in Table 3 the cell entries were the number of political parties that shared the particular row and column trait, in Table 4 the cells are the number of constituencies in which a party shared each of the row and column characteristics. This test is considerably more demanding than was the first test of the hypothesis. Here we are asking not simply whether any particular party contested a majority of close or remote constituencies, but rather how many constituencies in all were contested by parties whose leaders were high in need for achievement or low in need for achievement. Yule's Q for Table 4 is .33, while ϕ is .17. Given the large number of cases here, these coefficients may be considered as fairly substantial deviations from the null hypothesis that there is no association between need for achievement and the type of constituency preferred.

We may tentatively conclude from the evidence presented in Tables 3 and 4, that high need for achievement is substantially associated with a disproportionately large preference for moderate risk, close constituencies.

Hypothesis 4 posits that need for achievement is positively associated with the acquisition of important coalition benefits. To test the hypothesis, both the ministerial index and the absolute percentage of important portfolios controlled by a party were used as dependent variables. Because the hypothesis simply predicts that parties with leaders who have high need for achievement do better than parties with leaders who have low need for achievement, rather than predicting a linear relationship between need for achievement and coalition performance, a simple one-way analysis of variance was used to test the hypothesis. Here we measure the extent to which the presence of a

Table 4. The Association Between the Number of Candidacies in Close or Remote Constituencies and Each Candidate's Party's Level of Need for Achievement[a]

		Constituencies	
		Close	Remote
Need for Achievement	High	376	283
	Low	287	424

$Q = .33$
$\phi = .17$

[a] Based on 1370 candidates

particular characteristic, such as high or low need for achievement, systematically differentiates a set of subjects on some other dimension, such as coalition performance. If, for example, the parties with high achievement leaders and the parties with low achievement leaders each secured the same average percentage of important portfolios, the analysis of variance would indicate that need for achievement does not adequately differentiate parties on the basis of performance. If, on the other hand, the difference in the coalition success of parties is much larger across the two need for achievement categories than within each of the categories, the analysis of variance would indicate that we can differentiate between successful and unsuccessful parties based on their need for achievement. Tables 5 and 6 present the analysis of variance for the ministerial index and for the absolute percentage of important portfolios secured in the coalitions.

As can be seen from the two analyses of variance, the parties whose leaders were high in need for achievement did better both on the ministerial index, where the difference is slight, and on the percentage of important portfolios secured during the tenure of the coalitions, where the association is far more substantial. The high need for achievement parties secured, on the average, more than their fair share of important portfolios (103 per cent of their fair share), while the low need for achievement parties received, on the average, less than their fair share (87 per cent of their fair share). The high need for achievement parties also received a much larger absolute percentage of important portfolios than did the parties with leaders who have low need for achievement (nearly 18 per cent compared with 4 per cent for the low achievement parties). In other words, the parties with high need for achievement leaders controlled a disproportionately large number of important cabinet portfolios. Control over these cabinet positions, in turn, gave the parties with high need for achievement leaders greater control over such important coalition benefits as the distribution of patronage, publicity, and greater contacts with the masses. We can conclude, therefore, that hypothesis 4 is substantially supported by the evidence drawn from these Indian political parties.

Table 5. Disproportionality in the Distribution of Important Cabinet Ministries as Associated with Need for Achievement

	Ministerial Index		
	Mean	Variance	*N*
High *N*-Ach Parties	1.03	.075	7
Low *N*-Ach Parties	0.87	.070	8
All Parties	0.95		

Significance <.30

Table 6. Percentage Distribution of Important Cabinet Portfolios as a Function of Need for Achievement

	Percentage of Important Portfolios		
	Mean	Variance	*N*
High *N*-Ach	.178	.008	7
Low *N*-Ach	.040	.001	8
Total	.105		

Significance <.002

Need for achievement has proved to be a good indicator both of risk-taking preferences in elections and of performance in coalitions. Now it remains to be seen if parties with leaders who have high need for achievement are able to capitalize on these advantages. That is, do these parties actually perform better than low need for achievement parties in elections? The response to this question is more complex than were the responses to the previous four hypotheses. Electoral performance is an intricate phenomenon that requires a broader set of variables and controls than we have utilized thus far. First, electoral performance is operationalized using the following fourteen variables: the number of state legislative assembly seats won (1) in 1967 and (2) in 1969; the number of Lok Sabha seats won (3) in 1967 and (4) in 1971; the percentage of votes secured for the legislative assembly (5) in 1967 and (6) in 1969; the percentage of votes secured for the Lok Sabha (7) in 1967 and (8) in 1971; (9) the change in the number of seats won in the state assembly contests between 1967 and 1969; (10) the percentage change in the number of seats won in the state assembly contests between 1967 and 1969; (11) the change in the number of Lok Sabha seats won between 1967 and 1971; (12) the change in the percentage of votes won in the state assembly elections between 1967 and 1969; (13) the percentage change in the percentage of votes won in the state assembly elections between 1967 and 1969; and (14) the change in the percentage of votes won for the Lok Sabha between 1967 and 1971.

Variables 9 and 10 differ in that 9 is a measure of the absolute shift in number of seats for each party, while variable 10 controls for the initial base of support a party had in 1967. Thus, a party losing twenty seats out of forty would be considered equal to a party losing twenty seats out of twenty seats on variable 9. Variable 10, however,

Table 7. Party Performance on 14 Indicators of Electoral Success as Associated with Need for Achievement

Var.	Need for Achievement	Mean	Variance	N	Significance
1	High	53.29	858.90	7	<.002
	Low	12.75	33.07	8	
2	High	46.33	353.87	6	<.001
	Low	9.71	73.24	7	
3	High	6.86	13.14	7	<.01
	Low	2.00	3.71	8	
4	High	6.00	50.33	7	<.10
	Low	1.38	3.98	8	
5	High	16.09	58.05	7	<.001
	Low	4.40	3.85	8	
6	High	13.68	28.21	6	<.001
	Low	3.57	10.74	7	
7	High	16.41	55.63	7	<.001
	Low	4.24	9.32	8	
8	High	26.25	103.07	3	<.03
	Low	1.25	0.13	3	
9	High	−2.83	871.77	6	<.96
	Low	−3.43	22.95	7	
10	High	.18	.36	6	<.12
	Low	−.29	.18	7	
11	High	−.86	63.14	7	<.94
	Low	−.63	0.84	8	
12	High	−.37	13.13	6	<.84
	Low	−.70	4.49	7	
13	High	.01	.08	6	<.27
	Low	−.21	.16	7	
14	High	8.19	88.24	3	<.17
	Low	−1.05	0.89	3	

would record the first party as having suffered a 50 per cent loss, while the second party would have suffered a 100 per cent loss. The difference between variables 12 and 13 is comparable to the difference between variables 9 and 10.

Table 7 is a summary of fourteen analyses of variance that measure the relative performance of parties whose leaders score high or low on need for achievement. Only in the analysis of the change in the number of Lok Sabha seats won between 1967 and 1971 did the low need for achievement parties have a higher average level of performance than did the high need for achievement parties, and in that one case the difference between the two groups is negligible. Of the remaining thirteen analyses of variance in which the parties with high need for achievement leaders *did* out-perform the parties with low need for achievement leaders, seven of the differences between the average level of performance of the high and low need for achievement parties is significant at the .05 level or better. Six of these seven are significant at the .01 level or better, and four of these six are significant at the .001 level or better. In other words, hypothesis 5 is overwhelmingly supported by the evidence. The parties with leaders who have high need for achievement clearly outperformed the parties with leaders who have low need for achievement in pre- and postcoalition electoral situations. What is more, they apparently used the advantages they gained during the tenure of the coalitions to expand their legislative representation disproportionately and likewise disproportionately to increase their vote-getting ability between the pre- and postcoalition periods.

The bivariate analyses are now complete. All five hypotheses have been supported by the evidence. In other words, parties whose leaders are predisposed to follow a mixed strategy and who are high on the need to achieve seem to do better, at least in India, in elections and in coalitions than parties whose leaders prefer cooperative or competitive strategies and who are low in need for achievement. It is now appropriate to explore the combined effects of the predictor variables on party performance. In doing so, only two of the most interesting dependent variables will be examined further in this particular study. Others have been explored elsewhere.[26]

Using multiple regression, I attempt to account for the absolute change and percentage change in the number of seats won in the state legislative assembly contests between 1967 and 1969 as a function of the model's predictor variables. These dependent variables have been chosen because they reflect the dynamic, longitudinal impact of the model on party representation. These two variables are reliable indicators of the expansion or contraction of a party's legislative base. Thus, although measures of change in voting percentages, or measures of absolute strength at a single point in time, are interesting, they are not as informative for party leaders, nor are they as indicative of the future potential of a party as are the two variables I have selected. If the model does accurately predict changes in representation, then it provides a useful supplement to the intensive investigation of the idiosyncratic aspects of Indian politics which account for the success or failure of political parties.

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[26] Bueno de Mesquita, chaps. 5 and 6.

Table 8. Percentage Change in Vidhan Sabha Seats Won Between 1967 and 1968 as a Function of the Strategy Index and Need for Achievement

Variable	Coefficient	Std. Error	*T*-Statistic	Significance
Intercept	−1.311	.301	−4.361	<.001
SI	1.64	.405	4.043	<.001
N-Ach	1.71	.873	1.959	<.07

$R^2=.63$ Std. Error = .34 $F=10.93$ Sig. = .002 Number of Parties = 16

Positive findings in the multivariate analyses would suggest that exploring the psychological dynamics of risk taking in political parties has great utility. The success of the multivariate analyses would also suggest that the overall performance of political parties is very much determined by factors controlled by the party, as well as by the electorate. Such a finding would be theoretically interesting to social scientists and policy-relevant to party leaders. Along with several other studies,[27] it would suggest that when elites are recruited on the basis of political style and other psychological attributes, handsome electoral gains may result. That is, the regression models will help to identify the parameters within which parties may manipulate their electoral performance by recruiting leaders with particular strategic and achievement orientations.

First, the percentage change in Vidhan Sabha seats won between 1967 and 1969 is estimated as a function of the strategy index (SI) and need for achievement (*N*-Ach). The results are presented in Table 8. The regression coefficients indicate the sizable benefits that result from being high in need for achievement and from preferring a mixed strategy. Thus, a hypothetical party predisposed to follow a completely mixed strategy and having in its ranks all of the leaders in its state with high need for achievement could expect, all other things being equal, to treble its representation.

Parties, of course, are interested in the actual number of seats they can expect to win or lose more than in percentage changes in their strength. The second dependent variable assesses actual changes in numbers of seats. To estimate these changes we must add an additional variable to the regression analysis to account for the initial base of strength from which each party contests subsequent elections. The number of seats won by each party in 1967 is used to assess that base of strength. The other independent variables are, again, the strategy index and need for achievement. These three variables account for 72 per cent of the variance in the change in the number of seats won between 1967 and 1969. Table 9 presents the regression analysis, while Figure 1 is the scatterplot of the observed change in seats compared with the estimated change in seats. The coefficients in this analysis indicate the number of seats a party could expect to gain or lose as a function of its performance on any one of the three predictor variables if we control the effects of the other predictor variables. For instance, all else being equal, the average party could have expected to lose half its representation between 1967 and 1969. On the other hand, if it followed a completely mixed strategy, it could have expected a gain of 39 seats, all other things being equal. If a party had in its ranks all the leaders in its state who had high need for achievement it would have gained 75 seats, all other things being equal.

Table 9. Changes in Vidhan Sabha Seats Won Between 1967 and 1969 as a Function of the Number of Seats Won in 1967, the Strategy Index and Need for Achievement

Variable	Coefficient	Std. Error	*T*-Statistic	Significance
Intercept	−18.91	11.43	−1.654	<0.12
Number of Seats Won in 1967	−0.47	0.13	−3.690	<0.003
SI	39.31	13.40	2.934	<0.01
N-Ach	75.12	31.82	2.360	<0.04

$R^2=.72$ Std. Error = 10.91 $F=10.07$ Sig. = .002 Number of Parties = 16

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[27] Gordon S. Black, "A Theory of Political Ambition: Career Choices and the Role of Structural Incentives," *American Political Science Review*, 66 (March, 1972), 144–159; Joseph A. Schlesinger, *Ambition and Politics: Political Careers in the United States* (Chicago: Rand McNally, 1966); Dwaine Marvick, ed., *Political Decision-Makers: Recruitment and Performance* (New York: Free Press of Glencoe, 1961); Robert D. Putnam, "Studying Elite Political Culture: The Case of 'Ideology,'" *American Political Science Review*, 65 (September, 1971), 651–681.

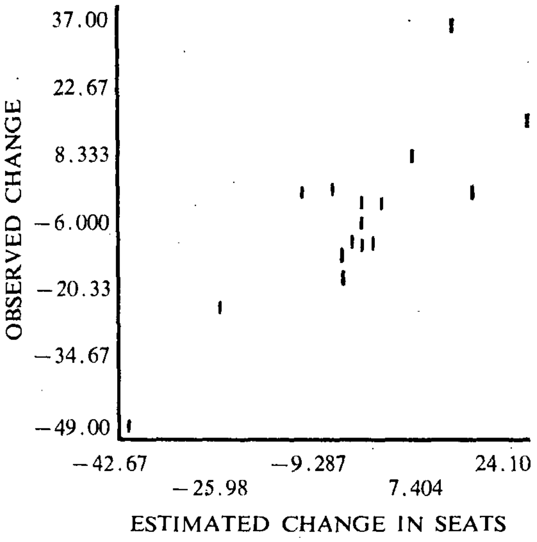

Figure 1. Scatterplot of the Observed Change in Vidhan Sabha Seats Won by 16 Parties Between 1967 and 1969 Compared to the Estimated Change in Their Representation As a Function of Their Legislative Strength in 1967, Their Strategic Predispositions, and Their Need for Achievement.

Conclusion

This study has developed and tested a model of strategic and motivational predispositions which successfully accounted for differences in the pre- and postcoalition electoral success of Indian political parties. It has also proved to be a good indicator of at least one aspect of party success within coalition governments. The psychological attitudes of party leaders toward risk taking accounted for much of the variance in the political success of their parties.

The model also explains differences in party performance without examining such questions as voter attitudes and behavior, party platform and ideology, or historical and situational context. Although those factors are important, and probably account for much of the unexplained variance, it is interesting to know that we are able to make significant generalizations about political parties based on elite scores on a very small set of variables.

The model underscores the significance of elite recruitment processes in determining the future prospects of political parties. It also highlights the ability of present party leaders to manipulate their party's strategic predisposition and level of need for achievement so as to improve future success in winning elections and in securing important positions in government.

APPENDIX

Questions Pertaining to Need for Achievement

The questions from which need for achievement was measured are as follows:

Would you please tell me what are the most important objectives that your party has for 1970? Any others?

How do you think coalitions affect the public image and political influence of your party?

Do you think there is any advantage to initiating negotiations [with other parties]? What are the advantages?

Would you rather win a legislative victory that would lead to the breakup of a coalition your party was in, or would you rather have the coalition remain intact and not win the victory? Why?

Of course being in the government gives you the opportunity to distribute patronage. What effect has this had on the relations between the party's leaders and its rank and file membership?

Do you feel that the distribution of patronage can be an effective tool for improving the party's electoral appeal? How?

Can patronage improve the party's legislative effectiveness? How?

In what ways do coalitions affect the organization and cohesion of your party? Its discipline?

What has your party gained from joining coalitions?

What did you originally expect to gain from the coalitions?

Do you think you have gained more or less than the parties you coalesced with? In what ways?

Does your party have a coalition strategy? What is it?

Compared to coalitions, what are the advantages and disadvantages of pre-election alliances? Any others?

This is a very hypothetical question. I realize that this situation shall never arise in India. Nevertheless, I am interested in your general attitude toward the problem. Let's say there were a legislature with one dominant party, several parties of approximately equal strength, and one or two weaker parties. If in that legislature your party had 18 per cent and the other parties had 30 per cent, 20 per cent, 15 per cent, 11 per cent and 6 per cent respectively, which of the following do you think would be the best coalition for your party?

(a) 30, 20, 18 = 68%
(b) 30, 18, 15 = 63%
(c) 30, 18, 11 = 59%
(d) 30, 18, 6 = 54%
(e) 20, 18, 15 = 53%
(f) 20, 18, 11, 6 = 55%

Why would you prefer that one?

Bases of Budgetary Incrementalism*

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The lure of incrementalism as an explanatory tool in the social sciences is strong because the chaos in human behavior which scholars seek to rationalize makes sense according to incrementalist explanations. Lindblom's "Science of 'Muddling Through' "[1] gave intellectual respectability to apparently mindless and minor decisions by pointing out that these decisions follow a pattern in being successive, limited, and comparative. Lindblom explains such decisions by noting that because of resource and rationality constraints they cause only a slight departure from the status quo. This means that the apparently mindless change is reasonable and even predictable.

Unfortunately, incrementalism, though intuitively and experientially appealing, infrequently meets the canons of academic and scientific explanation. Incrementalism, I suggest, can be used in a descriptive sense or an explanatory sense. A phenomenon is descriptively incremental if changes in the system are only marginally different from the status quo. A phenomenon is explained by incrementalism if reasons for the marginality of the changes can be given. In most situations incrementalism fails because it is difficult to define the status quo adequately and to measure the small change from that state. Even where that is possible, it is very difficult to show that the change is a result of limited rationality and resources or, indeed, of any other cause.

In the budgetary arena, however, scholars have been able to define the status quo as the previous appropriation and to define changes from that state both as the increase requested for the next fiscal year and also as the increase appropriated by the legislature. Fenno, Sharkansky, Crecine, and Davis, Dempster, and Wildavsky[2] all have found empirical evidence for incrementalism so operationalized. They have established that the agency requests are a small increment over the previous appropriation and that the current appropriation is only a small amount less than the amount requested. Incrementalism, then, at least descriptively obtains in the budgetary process.

But to be an explanatory tool, incrementalism must specify not only that the change is a slight modification of the existing state, but also must specify why that modification is slight. Davis, Dempster, and Wildavsky's prominent work on federal budgeting attempts to show that descriptive incrementalism occurs because of strategic considerations on the part of the participants.[3]

The present essay proposes to show that the techniques used by Davis, Dempster and Wildavsky do not allow valid inferences to be made about *why* budgetary incrementalism occurs. Second, evidence from recent Labor Department budget experience will be presented to explain why budgetary change at the federal level can properly be called incremental, not only in a descriptive sense, but also in an explanatory sense.

The Davis, Dempster, and Wildavsky Model of Budgetary Incrementalism

Davis, Dempster, and Wildavsky explain federal budget behavior through linear regression models—three for the agency decision about what to request and three for the congressional decision about what to appropriate. These six models are all variants of the following basic models:

$$\text{APPROPRIATION}_t = \alpha\ \text{REQUEST}_t + \phi \qquad (1)$$

$$\text{REQUEST}_t = \beta\ \text{REQUEST}_{t-1} + \theta \qquad (2)$$

$$\text{REQUEST}_t = \gamma\ \text{APPROPRIATION}_{t-1} + \rho \qquad (3)$$

The congressional decision rule says basically that the appropriation is a relatively fixed percentage of the request plus some random variations, ϕ. The agency decision rules say that the request is a percentage change from either the last request or the last appropriation plus suitable random variations, θ or ρ. Variants of these models allow for other more sophisticated interpretations of budgetary interaction. These models were fitted to actual appropriations data and the variant that yielded the best fit was adjudged to represent budget decision behavior. Davis, Dempster, and Wildavsky say:

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* I would like to express my gratitude to Lawrence V. Grant, Virginia Gray, and an anonymous reviewer for their help. I also thank The Brookings Institution and the University of Kentucky for support received during the gestation and writing of this essay. An earlier version of this paper was presented at the Annual Meeting of the Public Choice Society, March 22, 1973, at College Park, Maryland.

[1] Charles E. Lindblom, "The Science of 'Muddling Through,' " *Public Administration Review*, 19 (Spring, 1959), 79–88.

[2] Richard Fenno, *The Power of the Purse* (Boston: Little, Brown, 1966), pp. 267, 354, 356; Ira Sharkansky, "Agency Requests, Gubernatorial Support, and Budget Success in State Legislatures," *American Political Science Review*, 62 (December, 1968), p. 1223; John P. Crecine, *Governmental Problem-Solving* (Chicago: Rand McNally, 1969), pp. 206–208; O. A. Davis, M. A. H. Dempster, and Aaron Wildavsky, "On the Process of Budgeting: An Empirical Study of Congressional Appropriation," in Gordon Tullock, ed., *Papers on Non-market Decision-Making* (Charlottesville, Va.: Thomas Jefferson Center for Political Economy, 1966), pp. 102–123.

[3] O. A. Davis, M. A. H. Dempster, and Aaron Wildavsky, "A Theory of the Budgetary Process," *American Political Science Review*, 60 (September, 1966), 530–531.

> Our models are not predictive but explanatory. The alternative decision equations can be tried and the most appropriate one used when data on requests and appropriations are available. The appropriate equation explains the data in that, given a good fit, the process behaves "as if" the data were generated according to the equation.[4]

Thus, if the fit to the data is best for the equation $\text{REQUEST}_t = \gamma\ \text{APPROPRIATION}_{t-1} + \rho$, the data is explained "as if" the agency, "convinced of the worth of its programs, tends to be aware that extraordinarily large or small requests are likely to be viewed with suspicion by Congress"[5] and consequently "requests a percentage of its previous year's appropriation."[6]

Similarly, if the fit to the data is best for the equation $\text{APPROPRIATION}_t = \alpha\ \text{REQUEST}_t + \phi$, the data is explained "as if" "Congress believes that an agency's request, after passing through the hands of the Budget Bureau, is a relatively stable index of the funds needed by the agency to carry out its programs," and therefore "responds by appropriating a relatively fixed percentage of the agency's request."[7]

The principal criterion used to choose one of the equations to represent budget behavior is correlation between the variables.[8] Davis, Dempster, and Wildavsky accept the equations and the basic models underlying the equations because

> estimated correlation coefficients for the best specifications of each agency are generally high. Although the calculated values of the multiple correlation coefficients (R's) tend to run higher in time series than in cross-sectional analysis, the results are good. We leave little of the variance statistically unexplained.[9]

Estimation of the equations (1), (2), (3) and their variants yielded high r's and regression coefficients such that typically

$$.90 < \alpha < 1.00$$

$$1.00 < \beta < 1.20$$

$$1.00 < \gamma < 1.20$$

These findings corroborated Fenno's conclusions that at the federal level the budget process is at least descriptively incremental since the requests and the appropriations are only slightly different. These findings are also congruent with those of Sharkansky at the state level and Crecine at the municipal level. The correlation coefficients typically are well above .90 for the best fitted agency decision equation, with the congressional decision equation usually having a correlation exceeding .96. Thus it would seem that the equations are good fits to the data and it is valid to infer that the "as if" models explain budgetary incrementalism.

A cautionary note must be sounded, however. Comparison of the r's and other statistics associated with equations (1), (2), (3) and their variants allows choosing those two equations of the six options that "fit" the data best; but the choice is limited to only those six options. Even though the correlations are very high, the analyst does not know whether the correlations are "significantly" high. That is, can these high correlations be generated by random data? To answer that question, data were generated that fitted the constraints of descriptive incrementalism but which varied randomly within those constraints. The size of the correlations from the randomly generated data relative to the correlations from the Davis, Dempster, and Wildavsky equations allows judging whether their "as if" explanations are justifiably inferred.[10]

To investigate this question the descriptive model of budgetary behavior at the federal level was defined by the inequalities:

$$\text{REQUEST}_t \geq \text{APPROPRIATION}_{t-1} \qquad (4)$$

$$\text{APPROPRIATION}_{t-1} \leq \text{APPROPRIATION}_t \qquad (5)$$

$$\text{APPROPRIATION}_t \leq \text{REQUEST}_t \qquad (6)$$

and by the added constraint that generally the terms of the inequalities are to be only marginally unequal. The data presented by Crecine, by Davis, Dempster, and Wildavsky, by Fenno, and by Sharkansky with few exceptions bear out the accuracy of this characterization of the budget relations. Because their work in different arenas of budgeting says that relations (4), (5), and (6) apply, I am confident that the budgetary system behaves as the model says it does. Therefore, any data that meet constraints (4), (5), and (6) can be interpreted as simulating budgetary data, and these data can be used in a sensitivity analysis of

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[4] *Ibid.*, p. 542.

[5] *Ibid.*, p. 532.

[6] *Ibid.*

[7] *Ibid.*, p. 534.

[8] *Ibid.*, p. 536.

[9] *Ibid.*, p. 537.

[10] For a good discussion of the use of sensitivity analysis, see L. V. Grant, "Aspects of Theory Evaluation," *British Journal of Political Science*, 3 (January, 1973), 101–110.

Table 1. Frequency Distribution of Correlations on Various Data Bases

	Random Variation Data			Davis-Dempster-Wildavsky Data*	
R	$APP_t=\alpha REQ_t+\phi$	$REQ_t=\beta REQ_{t-1}+\theta$	$REQ_t=\gamma APP_{t-1}+\rho$	Best Congressional Equation	Best Agency Equation
.995–1	9.4%	0.4%	1.2%	41.8%	13.1%
.990–.995	16.0	0.8	4.0	4.5	3.3
.980–.989	35.6	8.6	21.7	22.4	3.3
.970–.979	17.6	14.3	24.2	7.5	14.8
.960–.969	11.9	10.6	19.7	1.5	6.6
.950–.959	4.9	12.3	10.6	4.5	4.9
.940–.949	2.0	13.9	7.8	6.0	6.6
.930–.939	0.4	8.2	3.7	1.5	4.9
.900–.929	1.2	16.0	5.7	4.5	8.2
.850–.899	0.8	10.2	1.2	4.5	18.0
0.0 –.849	0.0	4.5	0.0	1.5	16.4
$N=$	244	244	244	67	61

* Source: O. A. Davis, M. A. H. Dempster, and Aaron Wildavsky, "A Theory of the Budgetary Process," *American Political Science Review*, 60 (September, 1966), p. 537.

the Davis, Dempster, and Wildavsky decision models.[11]

When only these constraints were used, sets of number pairs representing requests and appropriations were generated in the following fashion:

(1) A number between 0 and 1000 was randomly chosen to represent the appropriation in year zero, APP_0

(2) With a ceiling of a 10 per cent increase over APP_0, the request, REQ_1, was generated by using a random number, V_1, such that $0 \leq V_1 \leq 1$. Then REQ_1 was defined as equalling $APP_0+(.1)(V_1)(APP_0)$. REQ_1 is therefore equal to or a random increase over APP_0, but less than or equal to a 10 per cent increase.

(3) The appropriation for year one, APP_1, was generated by using a second random number, W_1, such that $0 \leq W_1 \leq 1$. This number W_1 represents a random cut from the increment requested by the agency. Remember that the base, APP_0, is sacrosanct. Therefore, APP_1 is defined as

$$APP_1 = APP_0 + (.1)(V_1)(APP_0)(W_1)$$

(4) By using APP_1 as APP_0 had been used in step (2), the next request and appropriation were iteratively generated in accord with the following equations.

$$REQ_t = APP_{t-1} + (.1)(V_i)(APP_{t-1})$$

$$APP_t = APP_{t-1} + (.1)(V_i)(APP_{t-1})(W_i)$$

(5) The process was repeated until a sequence of 15 requests and 15 appropriations were generated. The number 15 was used to approximate the period from FY 1947 to FY 1963 used in Davis, Dempster, and Wildavsky's study.

(6) This entire process was then repeated using another randomly generated base number between 0 and 1000 for a total of 50 times.

Steps 1 through 6 were then repeated using ceilings of 20 per cent, 40 per cent, 80 per cent, and 160 per cent. This yielded sequences of size 50, 50, 48, 48, and 48 for a total of 244 randomly generated pairs of sequences representing appropriations and requests.

By lagging the variables APP and REQ, the variables APP_t, APP_{t-1}, REQ_t, and REQ_{t-1} were made available. These allowed calculating the correlation coefficients for the basic equations (1), (2), and (3) on the random variation data. By comparing correlations for the random variation data on the three basic equations with the best fit correlations for the longest period from Davis, Dempster, and Wildavsky's study, the validity of their "as if" explanation could be adjudged. Since the distributions of the correlations for ceilings of 10 per cent, 20 per cent, 40 per cent, 80 per cent, and 160 per cent were substantially the same, all the runs were combined into the one distribution displayed in Table 1.

Analysis of Table 1 shows that 76 per cent of the best congressional specifications from Davis, Dempster, and Wildavsky have $r \geq .97$, while 80 per cent of the random variation cases have $r \geq .97$. For agency decision equations, 34 per cent of the Davis, Dempster, and Wildavsky cases have

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[11] Consistency of simulation output and actual data is one of two criteria Crecine suggests for evaluating a simulation model. See his *Governmental Problem-Solving*, pp. 111–114, 143–145.

Table 2. Correlations Between Sets of Two Random Variables, One of Which Dominates the Other

R	Frequency
.95–1	1
.90–.94	3
.85–.89	6
.80–.84	9
.75–.79	11
.70–.74	14
.65–.69	12
.60–.64	9
.55–.59	9
.50–.54	9
.45–.49	5
.40–.44	4
.35–.39	2
.30–.34	3
.25–.29	0
.20–.24	2
.15–.19	1

For $N=15$, if $r \geq .412$, $p < .05$
if $r \geq .558$, $p < .01$
if $r \geq .725$, $p < .001$

$r \geq .97$, while 24 per cent of the $\text{REQ}_t = \beta\ \text{REQ}_{t-1}$, and 51 per cent of the $\text{REQ}_t = \gamma\ \text{APP}_{t-1}$ correlations exceed or equal .97. While the Davis, Dempster, and Wildavsky results are clustered a little higher than the random variation data correlations, we are dealing with differences of only .01 or .02 in the absolute value of Pearson correlation coefficients. Also we are comparing the r's of the basic equations on the random data with the r's of the best fits of all the variants on the actual data. We conclude that while the maximum correlation criterion is adequate for distinguishing among the models presented by Davis, Dempster, and Wildavsky, it does not justify accepting that whole class of models because the correlations are of the same magnitude as those generated by random data under budgetary constraints. This is not to say that the Davis, Dempster, and Wildavsky characterization of budgetary decision rules is inaccurate. It may very well be that those decision models are true representations of budgetary decision phenomena, but such a judgment cannot be made solely on the basis of high correlation coefficients.

Though slightly tangential to the analysis, it is important to know what constraints generated the high correlations obtained by Davis, Dempster, and Wildavsky. Logically it has to be either the marginality of the differences or the constant dominance of one variable by the other. If the strong correlation is caused by marginality, correlation techniques might still be useful if increments requested and cut were the units of analysis. However, the fact that the distributions of correlation coefficients in the random data analysis were substantially the same no matter what the request "ceiling" suggests that the domination of one variable by the other is the determining relation. To investigate that conjecture, 100 pairs of variables were generated in the following fashion:

(1) 15 random numbers between 1 and 1000 were generated, X_i, $i=1, 15$. This is the first variable.
(2) 15 random numbers, V_i, were generated between 0 and 1.
(3) The second variable, Y_i, is created according to the equation,

$$Y_i = (X_i)(V_i)$$

The correlations between X (corresponding to the requested increment) and Y (corresponding to the amount appropriated) are displayed in Table 2. The median r is in the range .65 to .69. For $N=15$, an $r > .41$ is significant at the .05 level, which means that about 90 per cent of the correlations are "significant" at the .05 level. Since these sets of sequences were created only with the constraint that, pairwise, one variable dominate the other, and since the r's are very high, we must conclude that for an N of 15, the high correlations are a function of one variable's always dominating the second and not of marginality. This implies that dealing with budget increments will not normally allow the use of correlation as a criterion by which one can clearly discriminate among linear models.

Another Basis of Budgetary Incrementalism

What then can be done to explain the incremental increases on budget requests sent to the legislature and the restriction of cuts to those requested increments? In other words, how does one explain the basic and descriptively incremental relations:

$$\text{REQUEST}_t \geq \text{APPROPRIATION}_{t-1}$$
$$\text{APPROPRIATION}_t \geq \text{APPROPRIATION}_{t-1}$$
$$\text{APPROPRIATION}_t \leq \text{REQUEST}_t$$

where generally the terms of the inequalities are only marginally different. I will first present evidence that the inequalities obtain, then reasons why the terms of these inequalities are only marginally different from each other.

Since it is desirable that the terms of explanation be isomorphic to elements in the real phenomenon to be explained,[12] it is reasonable to examine agency and congressional behavior for one's formulations of the crucial variables.

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[12] Similarity of decision mechanisms between those in the real world and those of the model is another criterion suggested to evaluate a simulation model. See Crecine, *Governmental Problem-Solving*, pp. 111–114, 143–145. See also Richard M. Cyert and James G. March, *A Behavioral Theory of the Firm* (Englewood Cliffs, N.J.: Prentice-Hall, 1963), p. 317.

To that end, all general fund line-items in the regular Labor Department budget bills for FY 1968 through FY 1972 were examined. All House appropriations hearings and all the House and Senate reports on the bills were studied. This analysis corroborated the conclusions of other budget research that the base, or the previous year's appropriation for all practical purposes, is not considered to be fair game.[13] For instance, the Budget Bureau requires that the justification materials given Congress by the agencies spell out the increases requested over the previous year's base and the purposes to which the increases are to be put.[14] Questioning in the hearings focused on the requested increases. The text portion of Congressional appropriations reports almost always dealt entirely with the reasons for allowing or disallowing the increment requested and seldom paid any attention to the total amount requested. All of this points to the fact that Congressional and executive actors specifically focus on the increments and not on the total amount requested.

The composition of these increments, therefore, was next examined. In the justification material the agencies presented to Congress, a primary distinction was always made between mandatory and programmatic requests.[15] This means that the agency conceived of the request as composed of three parts: the base, the agency's mandatory needs, and the agency's programmatic desires. Mandatory requests are all those new costs needed to keep the agency operating at the same level as in the previous year. Usually these are the amounts necessary to cover increased pay costs, within-grade pay increases, and increased telephone, mail, utility, and other service costs. These costs occur year after year, though not always in the same amounts, because the costs of "staying in business" rise irrespective of changes in programs. These costs generally represent only a small percentage of the total request. Programmatic requests are those made for initiating new programs or for expanding services or products.

Since the Labor department agencies and the Budget Bureau thought in terms of base, mandatory requirements, and program desires, the question arises whether Congress did also. Evidence to be presented indicates that the Congress did, in fact, use the distinction between programmatic and mandatory requests as a basis for cutting the requests. Representative selections from the House and Senate Reports on the Labor Department Appropriations bills illustrate Congressional awareness of this distinction.

To begin with, most of the House reports typically contained an introductory section titled "Controllable and Uncontrollable Appropriations," referring to programs like Medicaid, Social Security, retirement pay for the Public Health Service veterans, grants to states for unemployment services, etc., in which existing law specifies that the appropriation be tied to some measure out of the agencies' control, such as number of eligible applicants. These items occasioned comments like the following:

> The total "uncontrollable" appropriations in the bill account for an increase of almost $2.2 billion over the appropriations for fiscal year 1971. The Committee has in each case approved the budget estimates for these programs.[16]

Even where Congress was not compelled by law to appropriate an "uncontrollable" amount of money, it appeared to treat mandatory requests without question. For instance, reporting on requests from the Bureau of Labor Statistics, the House Committee once said, "the amount recommended provides for all mandatory cost increases and one-half of the $2,855,000 requested for program increases."[17] Similarly, it reported, "The action of the Committee provides for mandatory cost increases, but allows for no program increase" for the Wage and Labor Standards Administration for FY 1970.[18] In like manner, the Bureau of International Labor Affairs for FY 1969 was cut $24,000, while being allowed a $38,000 increase over the FY 1968 appropriation, the House report noting that "The amount recommended is the exact amount estimated to be necessary to continue activities at the current year's level."[19] These quotations are typical of those made in the reports on the Labor-HEW appropriations bills in the period under consideration.

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[13] It is very curious that with all the qualitative evidence presented by Wildavsky, by Fenno, and by Sharkansky about the concept of the base, Davis, Dempster, and Wildavsky's operationalization of the budget relations did not utilize that concept. Consider the basic agency equation, $\text{REQUEST}_t = \gamma\ \text{APPROPRIATION}_{t-1} + \rho$. This means that the request, which we know consists of the accepted base plus the desired increase, is "equated" to a percentage of the base appropriation. This makes the decisionable increase a function of the already accepted base and thereby muddles concepts the qualitative literature holds to be distinct. The same kind of criticism can be made of the congressional decision equations.

[14] "The justification of each appropriation or fund account will begin with a statement relating the estimate to past and current year requirements. This statement will follow the form of exhibit 24 . . ." Exhibit 24 is titled "Analysis of Changes in Requirements." Executive Office of the President, Bureau of the Budget, "Preparation and Submission of Annual Budget Estimates," Circular No. A-11, June 1970, p. 27.

[15] Actually a fourfold distinction can theoretically be made. The Budget Bureau in Circular No. A-11 of June 1970, Section 24.2, says that increases requested over the budget authority of last year should be categorized as program increases, automatic increases (equivalent to our mandatory category), financing increases ("Activities financed by another program or agency in the preceding year."), and administration commitments ("Program increases to which the president has publicly and specifically committed the Administration."). In the data used in this study no administration commitments were spelled out, and only rarely was a financing increase found. For all practical purposes, therefore, the mandatory/programmatic distinction is all that applied.

[16] U.S., Congress, House, Appropriations Committee, "House Report 92–374," p. 4.

[17] U.S., Congress, House, Appropriations Committee, "House Report 91–1310," p. 7.

The Senate Committee's reports evince a similar criterion for cutting. For example, the Bureau of Apprenticeship and Training's FY 1969 request merited the following comments, "The amount allowed includes $496,100 to cover mandatory and financing increases and $299,600, with 24 of the requested 40 new positions. . . ."[20] The Women's Bureau's request for FY 1968 was granted because, "The small increase over 1967 is for mandatory changes in pay for employees of the Bureau."[21] And the Wage and Hour Division's FY 1970 request was granted with the observation that, "The increase is for mandatory costs and for increased costs in the enforcement activity."[22]

As the reports indicate, Congress gave at least the Labor agencies the money they requested for "mandatory" purposes. Since the mandatory requests usually recur every year, we now have a reason *why* $\text{REQ}_t \geq \text{APP}_{t-1}$. And since Congress seemed to always grant the request for the mandatory items but not always for programmatic items, we have reason to believe that $\text{REQ}_t \geq \text{APP}_t \geq \text{APP}_{t-1}$.

It is not necessary to rely solely on representative quotations from the reports to see what Congress did with the requests. Additional verification can be found by collecting data on the requests and the appropriations themselves, and subjecting those quantitative data to regression analysis. Regression is appropriate here because our argument centers about the estimated coefficients and not the associated correlations. To that end, data were collected on the amounts requested and appropriated for all 68 general fund line items in the five regular Labor Department appropriations bills from FY 1968 through FY 1972. The components of the request were found in the hearings.[23] The appropriations were regressed on the components of the request: the base, the mandatory requests, and the programmatic requests. One estimated equation follows:

$$\begin{aligned}\text{APPROPRIATION} &= .9901\ \text{BASE} \\ &\quad \mathit{SE} = .0018 \\ &\quad + .9446\ \text{INCREMENT REQUESTED} \\ &\quad \mathit{SE} = .0078 \\ &\quad R^2 = .9998\end{aligned}$$

This regression shows that the base was practically untouched by Congress, while the increment was cut by more than 5 per cent, as previous qualitative literature led us to expect. Since the base is evidently sacrosanct, the increment requested, ΔREQ, and the increment appropriated, ΔAPP were analyzed. By breaking the requested increment, ΔREQ, into the mandatory request, ΔMANDREQ, and the programmatic request, ΔPROGREQ, the following regression equation was estimated:

$$\begin{aligned}\Delta\text{APP} &= 1.0038\ (\Delta\text{MANDREQ}) \\ &\quad \mathit{SE} = .0374 \\ &\quad + .9398\ (\Delta\text{PROGREQ}) \\ &\quad \mathit{SE} = .0091 \\ &\quad R^2 = .9938\end{aligned}$$

This equation very clearly shows that Congress gave the mandatory requests from the Labor Department clear passage, while cutting requests for new or expanded programs significantly more.

All this means that since agencies have mandatory needs to present to Congress,

$$\text{REQUEST}_t \geq \text{APPROPRIATION}_{t-1}.$$

And since the mandatory elements of the request are honored, we now know that

$$\text{APPROPRIATION}_t \geq \text{APPROPRIATION}_{t-1}.$$

But because the requests for programmatic purposes are not always fully honored,

$$\text{APPROPRIATION}_t \leq \text{REQUEST}_t.$$

Thus we have been able to explain the budgetary relations (4), (5), and (6) for the Labor Department. There is little reason to believe that Congress would act in a substantially different manner with regard to agencies in other departments, and so I feel that the mandatory/programmatic distinction would in general explain budgetary relations at the federal level.

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[18] U.S., Congress, House, Appropriations Committee, "House Report 91–391," p. 7.

[19] U.S., Congress, House, Appropriations Committee, "House Report 90–1575," p. 7.

[20] U.S., Congress, Senate, Appropriations Committee, "Senate Report 90–1484," p. 3.

[21] U.S., Congress, Senate, Appropriations Committee, "Senate Report 90–469," p. 6.

[22] U.S., Congress, Senate, Appropriations Committee, "Senate Report 91–610," p. 13.

[23] Financing increases were combined with mandatory increases. I felt that this class of increases did not exhibit clear programmatic content but was "imposed" on the agencies and thus was closer to mandatory increases. Furthermore, since there were few such increases, where they were classified would have little impact on analysis.

There still remains the task of accounting for the marginality of the budget relations. The median percentage increases requested by the Labor agencies for mandatory items for fiscal years 1968 through 1972 were respectively 0.85 per cent, 4.05 per cent, 2.18 per cent, 1.63 per cent, and 8.61 per cent. For those same years the comparable figures for programmatic items were 2.51 per cent, 5.26 per cent, −0.25 per cent, 6.74 per cent, and 1.52 per cent. All of these are small percentages. Since the reasons for the marginality of the requests and the subsequent marginal appropriations cannot be explained by "gaming" considerations on the basis of high correlations, how can we account for the marginality?

While I have no definitive explanation for the marginality of budgetary changes based on the Labor Department experience, the source of mandatory increases offers a clue. Inflation, increases in the client population, within-grade pay increases, and other components of the mandatory increases are consequences of forces exogenous to the agency/Congress nexus. For the most part these external forces are tied to the state of the economy, which does not usually exhibit massive and radical changes in the short run. If the exogenous economic forces giving rise to the need for mandatory increases are slow moving, the demands for mandatory requests will also be small.

Why then is the programmatic component of the request usually small? It is not usually tied to economic forces. Furthermore, Fenno and Wildavsky[24] have detailed the programmatic drive of agency officials, a drive which unalloyed could hardly be called incrementalistic. Yet the requests presented to Congress generally are small. The explanation offered here is based on a theme presented by Crecine, who argues that external constraints restrain requests from being grossly expansionary.[25]

The "great equation" binding budget makers demands that

$$\text{EXPENDITURES} = \text{REVENUES} + \text{BUDGET DEFICIT}.$$

Since the size of a tolerable deficit is pretty well circumscribed, and since revenues will not increase appreciably from one year to the next, the amount that the President and Budget Bureau allow for *both* mandatory and programmatic increases is marginal compared to the total budget. These constraints at the macro level are transmitted down to each agency and are then manifested in the marginal increases requested by the individual agencies. Just how a given agency receives its particular allotment is not predicted by this line of argument but this approach explains the overall marginality of the budget relations.

Summary and Conclusions

In the social sciences incrementalism has been used in both descriptive and explanatory senses. For a phenomenon to be descriptively incremental, changes in the system have only to be marginally different from the status quo. In this regard budgeting has been well established as incremental. For a phenomenon to be explained by incrementalism, on the other hand, reasons must be given for the marginality of changes. In the literature, budgeting has not been successfully explained as incremental in this sense.

This paper analyzed the explanatory power of the theory of budgetary incrementalism presented in the prominent work of Davis, Dempster, and Wildavsky. By means of sensitivity analysis it was demonstrated that inferences to "gaming" explanations of budgetary incrementalism are not warranted on the bases of correlation analysis.

To explain budgetary incrementalism, recourse was made to concepts and variables explicit in the vocabulary of the budget process participants. The distinction between mandatory and incremental requests and the differential treatment of those two kinds of requests by Congress allowed good explanation of the incremental changes in requests and appropriations.

In particular, the inexorable but small mandatory request that was automatically granted was alone adequate to explain why requests always increased and why one year's appropriation slightly exceeded the previous year's appropriation. Fiscal constraints binding the government as a whole were called upon to explain the overall marginality of budget changes. Because of the isomorphism between concepts and variables used by budgetary actors and the theory presented in the second part of the paper, this theory of budgetary incrementalism appears to be superior to Davis, Dempster, and Wildavsky's.

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[24] Fenno, pp. 266–274 and Aaron Wildavsky, *The Politics of the Budgetary Process* (Boston: Little, Brown, 1964), pp. 18–31.

[25] "A Simulation of Municipal Budgeting: The Impact of Problem Environment," in William Coplin, *Simulation in the Study of Politics* (Chicago: Markham, 1968), pp. 119–122, 136–139, 144 and "Defense Budgeting: Organizational Adaptation to Environmental Constraints," in R. F. Byrne, A. Charnes, W. W. Cooper, O. A. Davis, and Dorothy Gilford, *Studies in Budgeting* (Amsterdam and London: North-Holland Publishing Company, 1971), pp. 218–222, 227–235.

Because the mandatory request and congressional acquiesence to it can alone account for the descriptive incrementalism of the budget process, in further research attention must be paid to the programmatic portion of the budget. In a sense, this is the only part of the budget that is "variable." It is only in this component of the budget that the "politics" of the budgetary process can be found.

Environment and Party: The Impact of Political and Demographic County Characteristics on Party Behavior*

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Introduction

Political parties have been the focus of perhaps as much empirical research as any other actors in the American political process. Yet for all this attention, no empirical theory attempts to explain the activities of the party organizations themselves. Such activities have been utilized frequently as independent variables in the analysis of voter behavior,[1] but only infrequently have they been treated as variables to be explained in their own right—that is, as dependent variables. At the same time it is commonly acknowledged, as a result of research on political machines, that parties are adaptive organisms—that their behavior should be viewed as a response to, rather than a cause of, the conditions or environmental circumstances which they face.[2]

This view of the activities of local party organizations as dependent variables will guide the research reported here. In the following pages, I shall develop a theoretical frame for analyzing the impact of environmental conditions on party activities and then test a number of hypotheses, many of which derive from the findings of previous studies, concerning the relationship of environment to party during the 1964 campaign. The principal goal of this research is to chart the path, both conceptually and empirically, toward an empirical theory of environment and party behavior.

A number of studies have analyzed the impact of the environment on the performance of party activities. Some of them have attempted to explain variations in party behavior by reference to the political features of the environment as reflected in inter-party competition. Crotty and Key found that parties in the South performed their activities better the more competitive they were with one another.[3] The relationships between competition and party behavior reported by Eldersveld for Wayne County, Michigan, however, diverge sharply from those in the South. When Eldersveld's two highest categories of performance are combined, each party performs better where it holds the competitive advantage.[4] The studies of Cutright and Putnam present yet a third set of empirical findings: no significant relationships between the political environment and party performance.[5] Certainly no clear picture of the impact of the political environment emerges from previous research.[6]

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[*] I am grateful to Ray Corrado, Morris Ogul, Michael Margolis, Bert Rockman, James Scott, Donald Zauderer, and several anonymous reviewers for their constructive comments on earlier versions of this article. The data used in the article were provided by the Inter-University Consortium for Political Research.

[1] See Phillips Cutright, "Activities of Precinct Committeemen in Partisan and Non-partisan Communities," *Western Political Quarterly*, 17 (March, 1964), 93–108; Phillips Cutright, "Measuring the Impact of Local Party Activity on the General Election Vote," *Public Opinion Quarterly*, 27 (Fall, 1963), 372–386; Phillips Cutright and Peter H. Rossi, "Grass Roots Politicians and the Vote," *American Sociological Review*, 23 (April, 1958), 171–179; Daniel Katz and Samuel J. Eldersveld, "The Impact of Local Party Activities upon the Electorate," *Public Opinion Quarterly*, 25 (Spring, 1961), 1–24; Gerald H. Kramer, "The Effects of Precinct-Level Canvassing on Voter Behavior," *Public Opinion Quarterly*, 34 (Winter, 1970–1971), 560–572; Robert Putnam, "Political Attitudes and the Local Community," *American Political Science Review*, 60 (September, 1966), 640–654; and Peter H. Rossi and Phillips Cutright, "The Impact of Party Organization in an Industrial Setting," in *Community Political Systems*, ed. Morris Janowitz (Glencoe, Illinois: Free Press, 1961), pp. 81–116.

[2] This view of the party as a dependent variable underlies Robert K. Merton's classic discussion of the functions of the party machine. See *Social Theory and Social Structure* (New York: Free Press, 1968), pp. 126–127.

[3] William J. Crotty, "The Party Organization and Its Activities," in *Approaches to the Study of Party Organization*, ed. William J. Crotty (Boston: Allyn and Bacon, 1968), pp. 247–306; and V. O. Key, *Southern Politics in State and Nation* (New York: Alfred A. Knopf, 1949), pp. 386–405.

[4] Samuel J. Eldersveld, *Political Parties: A Behavioral Analysis* (Chicago: Rand McNally, 1964), p. 424.

[5] Phillips Cutright, "Measuring the Impact of Local Party Activity . . . ," pp. 372–386; and Robert Putnam, "Political Attitudes and the Local Community," p. 643.

[6] There are several methodological reasons why these research findings might be so contradictory: (1) both the independent and dependent variables were measured differently in each; (2) different types of party behavior were examined; (3) different levels of analysis were employed (some studies worked at the precinct level while others worked with counties); and (4) variations in the range of the political environment captured by the independent variable, party competition, were quite small in most cases. Whatever the reason, the resultant problem in cumulating these diverse findings into a theory of environment and party behavior underscores the need for truly comparative studies of party performance across a wide range of political environments.

Other studies of environment and party behavior, especially those of that form of party organization known as the "machine," have utilized demographic characteristics of the environment as the independent variables. Party machines have emerged in those areas which contain large concentrations of particularistic voters.[7] They differ from other party organizations in their overarching emphasis on the continuous provision of material benefits and services directly to constituents through local party leaders. When voters are near the subsistence level, their vote is readily exchanged for such rewards as the machine can offer, and more general appeals, in which the anticipated returns are less direct, are likely to be ineffective. As explained graphically by Scott:

> Poverty shortens a man's time horizon and maximizes the effectiveness of short-term material inducements. Quite rationally he is willing to accept a job, cash, or simply the promise of assistance when he needs it, in return for his vote and that of his family. Attachments to policy goals or to an ideology imply something of a future orientation as well as wide loyalties, while poverty discounts future gains and focuses unavoidably on the here and now.[8]

The fuel of the machine, in the past as well as in the present, is the wide range of services it provides for constituents—such material rewards as patronage jobs, legal assistance and protection, and cash to tide the family over during hard times. Performance of service activities alone, though, is rarely sufficient to ensure satisfactory levels of support on election day for even the strongest machine. In order to collect the debts which have been accumulated by continuous servicing of constituents, machine leaders place a heavy emphasis on voter mobilization activities. Performance of these activities is particularly important among the poor because they often lack the motivational force which propels the more affluent or educated voter to the polls. Both adequate servicing of constituents and mobilization, furthermore, depend upon a high level of organizational efficiency, especially at the precinct level "where the voters are."

The relationship between the nature of the constituency and party behavior implied in this description has become increasingly clear with the decline of the party machines. This decline has been traced to a fundamental alteration in the nature of the party's constituency:

> The specific inducements which the machine was organized to supply worked their "magic" on a smaller and smaller proportion of party workers and supporters. Instead, as businessmen and laborers each came to appreciate their broader, longer run interest as a sector of society, they increasingly required general legislation that met their interests in return for political support. Here and there a social context tailored to the machine style remained, but the machine either reconciled itself to the new loyalties—becoming less and less a machine in the process—or was the electoral victim of social change. Parties still continued to offer palpable inducements to voters but the new inducements were more typically embodied in general legislation, where previously they had been particularistic and often outside the law.[9]

That is, as the needs of voters have changed, the party organizations have turned away from service activities toward other kinds of vote-getting methods. Among these new methods has been an emphasis on persuading voters to back the party's candidates through the use of issue and personality appeals. At the same time, as the general level of education among voters grew, the organization could diminish its concentration on mobilizing its supporters. Many of them had become self-mobilized through their increased awareness of and interest in politics and through their internalization of the norms of democratic citizenship. While the activities of the party organization changed with the changing nature of the constituency, the need for a strong organization remained, but the vote-directed activities of that organization were transformed.

The state of our knowledge concerning the relationships between environment and party behavior may be summarized as follows: A number of empirical studies have examined the impact of the political environment (operationalized in terms of interparty competition) and their findings are sufficiently at variance with one another to preclude cumulation into a theory of political environment and party. On the other hand, a theory of the demographic environment and party behavior emerges quite clearly from the literature on machine politics, but this theory lacks the empirical grounding required of scientific theories.[10]

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[7] See Talcott Parsons, *The Social System* (New York: Free Press, 1951), pp. 61–63, for the meaning of particularistic. My use of the term in the context of political machines is derived from James C. Scott, "Corruption, Machine Politics, and Political Change," *American Political Science Review*, 63 (December, 1969), 1145–1149.

[8] Scott, "Corruption, Machine Politics, and Political Change," p. 1150.

[9] *Ibid.*, pp. 1148–1149.

[10] A clear distinction should be made between the "theory" concerning the relationship of the demographic environment to party behavior and the "findings" concerning the relationship of the political environment to party behavior. The expectations embodied in the theory were derived from studies of party organizations, particularly the political machine, which, although highly insightful, were not systematically empirical. With the exception of the findings

Several reasons may be suggested for the lack of theory about political environment and the lack of empirical tests of the theory bearing on the demographic environment. Probably the most important reason is that serious problems of measurement equivalency arise in comparing party activities across environments.[11] If party behavior does not vary along a single dimension under different environmental conditions but varies instead in its emphasis on one dimension or another depending upon the environment, then a prerequisite to the comparative analysis of party behavior is the specification of the universe of party activities and the grouping of these activites along certain common behavioral dimensions. Such an approach has not been common in previous studies. A second reason involves the problems in observing party behavior *per se* even if certain common dimensions of performance can be specified. Party activity is a property which cannot be easily measured directly. Instead it must be aggregated from the behavior of numerous party activists. Such an aggregation is intrinsically more difficult than that for other "analytic" properties, such as median income or Democratic vote, because it requires extraordinarily detailed information of the kind difficult to obtain with even the most intensive survey. Finally, few studies of party behavior have viewed parties as dependent variables. As a consequence, little research has focused explicitly on the relationship between the kinds of activities performed by a local party organization and the political and demographic characteristics of the locale.

The principal objective of the research reported in the following pages is to provide empirical grounding for a theory of environment and party by examining the impact of environment on party behavior across the range of environmental conditions extant in the United States. The dependent variable in the analysis is the behavior of a national sample of county party organizations during the 1964 campaign. While many possible determinants of this behavior warrant attention, the analytical focus is restricted to the environmental factors which have been identified as important in previous research: the political and demographic environments. A second objective of this study is the specification of a conceptual frame for the comparative study of party behavior. Construction of such a frame requires both the theoretical identification of separate dimensions of party behavior and the measurement of these dimensions in such a manner as to make them equivalent across environments. It is to these two tasks that I now turn.

Measuring Party Behavior

To develop measures of party behavior which are equivalent across different environments, it is necessary to discard the notion that party activity is unidimensional and that a summary measure of party performance may be constructed by adding performance scores on diverse activities. It is much more reasonable to assume that the types of activities which the party organization performs will vary greatly depending upon the political and demographic conditions of the environment. If this assumption is valid, then party performance must be measured along several different dimensions.

A few studies of party activity have adopted this approach. In his research on party organization in North Carolina counties, for example, Crotty specified four dimensions of activity. Three of the four—intercampaign, campaign, and election-day activities—were differentiated from one another according to their timing. The fourth —party organization—was singled out because it had an important impact on performance of voter-oriented activities.[12] Eldersveld identified somewhat similar types—organization, intercampaign, and campaign—in addition to a fourth type which he called "adaptive."[13] For the purposes of my research, I have differentiated party activities into four general types or dimensions—organizational, service, mobilization, and persuasion. This typology, in contrast to those of either Crotty or Eldersveld, organizes activities according to a single criterion—their function or purpose. Use of this criterion derives from the theoretical focus of my study: it is assumed that the major impact of the environment will be exerted on the purposes of activities rather than their timing. A more detailed discussion of each dimension follows.

The political party is an organization and may be expected, like all organizations, to engage in activities designed to maintain its organizational vitality. It must select leaders, provide for a staff, hold meetings, keep records, etc., if it is to be successful in pursuing its goals. The organizational dimension of party behavior refers to those activities which are directly concerned with organizational maintenance.[14]

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reported in the Key study, on the other hand, the discussion of the political environment's impact relies on systematically empirical studies.

[11] See Adam Przeworski and Henry Teune, *The Logic of Comparative Social Inquiry* (New York: Wiley, 1970), pp. 106–110, for an excellent discussion of the problems of establishing equivalency in comparative research.

[12] Crotty, "The Party Organization and Its Activities," pp. 254–257 and 269–273.

[13] Eldersveld, *Political Parties,* p. 347.

[14] For a good example of the operationalization of this dimension, see Eldersveld, pp. 410–433.

Political scientists have been interested in political parties not because they are organizations, but because parties have an explicitly political *raison d'être*—to win elections and, thereby, control the reins of government. Most of the effort of the party organization is directed toward fulfillment of this goal, and the three remaining dimensions of party behavior reflect the three different approaches to winning voter support.

Service activities are performed in order to build allegiances among voters which can be converted into votes at election time. In this sense, then, they are future oriented activities. The emphasis on the service dimension above all others and the allegedly superior performance of service activities are the hallmarks of American party machines. It would be a mistake, however, to assume that an emphasis on the performance of service activities is a good strategy in all types of political and, especially, demographic environments.[15]

Mobilization and persuasion activities are even more directly concerned with winning elections since, unlike either organizational or service activities, they are practiced heavily during campaigns but very little during the intercampaign period. The division of campaign activities into those concerned with the mobilization of latent support and those concerned with persuading voters to support the party and its candidates has been adopted in several studies of party behavior.[16] Mobilization has been found to be the more successful of the two activities in terms of winning votes, but this does not imply that persuasion is unimportant.[17] The specific activities which fall into the domain of one or the other of these two dimensions are many and seem to be bounded only by the imagination of party leaders.[18]

Whether the various party activities can be represented by these four dimensions is an empirical question. The assumption which underlies my typology is that there is a correspondence between the conceptualization of the analyst and the behavior, even if not the theory, of local party leaders. The extent to which this assumption is valid may be assessed by examining the interrelationships of performance scores for the various activities. Ideally, if this *a priori* differentiation of party behavior into four different dimensions has any validity, party activities should fall into four clusters reflecting organization, service, mobilization, and persuasion. If the data are found to support this assumption, then a measure of each dimension may be fashioned directly from the performance scores of those activities which cluster on it.

The performance scores for specific activities were derived from reports collected by the Survey Research Center at the University of Michigan on the performance of county party organizations during the 1964 campaign. Democratic and Republican chairmen from a national sample of counties were the sources of these reports.[19]

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[15] Nonetheless, this assumption underlies some research on party behavior, particularly that which implicitly views the political machine as the "ideal" party organization. For example, see Cutright, "Activities of Precinct Committeemen." In this comparative study of two cities, Cutright uses indicators which tap only the service dimension of party activity—(1) contacts with voters, (2) requests for aid (3) responsiveness to requests, and (4) whether the precinct leader held a patronage job. While a measure of party activity based exclusively on such indicators may reflect party behavior in a "machine" city such as Gary (Cutright's partisan city), it is surely not generalizable to a non-machine and non-partisan city. In other words, the political machine is only one form of party organization and can not serve as the model against which the activities of all party organizations should be measured.

[16] Raymond Wolfinger, "The Influence of Precinct Work on Voting Behavior," *Public Opinion Quarterly,* 27 (Fall, 1963), 387–398; Lewis Froman, "A Realistic Approach to Campaign Strategies and Tactics," *The Electoral Process,* ed. M. Kent Jennings and Harmon Zeigler (Englewood Cliffs, New Jersey: Prentice-Hall, 1966), pp. 1–19; and Kramer, "The Effects of Precinct-Level Canvassing on Voter Behavior."

[17] See Froman, "A Realistic Approach to Campaign Strategies and Tactics"; Bernard Berelson, Paul Lazarsfeld, and William McPhee, *Voting* (Chicago: University of Chicago Press, 1966), p. 171; and Kramer, "The Effects of Precinct-Level Canvassing on Voter Behavior," p. 572.

[18] Richard T. Frost lists a number of different campaign activities which were performed by party organizations in eight New Jersey counties. See his "Stability and Change in Local Politics," *Public Opinion Quarterly,* 25 (Summer, 1961), 221–235.

[19] The county chairmen data were collected as a supplement to the Survey Research Center's 1964 election study and were drawn from both parties in all counties where voters were interviewed. Questionnaires were mailed to the chairman of each party in 129 counties and the District of Columbia. The return rate from these mailings was unusually high (more than 90 per cent) and was not noticeably biased in favor of any particular type of county environment. For most of the analyses which follow, the data were weighted by county population to ensure representativeness of the findings. The District of Columbia was deleted from all analysis.

The use of the county as the level of analysis and county party chairmen as the source for perceptions of party activity seems optimal for my present purposes. Because of the decentralization of American party organizations, several levels of analysis are appropriate to the study of environment and party. Most previous studies have utilized either the precinct or the county. Of the two, it seems to me that the county chairmen may be freer to respond to environmental conditions and more knowl-

Among other things, these chairmen were asked to estimate what percentage of the precincts under their jurisdiction had chairmen and to assess their party's performance of six standard campaign activities:

(1) distributing literature (Lit)
(2) transporting voters to the polls on election day (Polls)
(3) holding candidate affairs (Affairs)
(4) raising money (Money)
(5) publicizing candidates through the mass media (Media)
(6) conducting registration drives (Drives)

They were also asked to rank order these six activities in terms of importance.

The constraints of a secondary source unfortunately preclude complete empirical testing of my theory concerning the multidimensionality of party activity. The original questionnaire contained no items which could be identified with the service dimension *a priori* and only one item—the percentage of precincts with chairmen—which seems to reflect performance along the organizational dimension. As a consequence, no attempt has been made to test for the existence of a separate service dimension, and the organizational dimension had to be measured by the single item which bore some relation to it. In order to denote the restricted nature of this measure of organizational performance, the variable will be referred to as "precinct organization" in subsequent analysis.

The theory-testing injunction was followed, on the other hand, where the mobilization and persuasion dimensions were concerned. On the surface, the six standard campaign activities seem to fall into two distinct groups. Two of the activities, Polls and Drives, are clearly identifiable as mobilization activities in terms of their function. The four others—Lit, Affairs, Media, and Money—seem to be activities which are performed with the purpose of persuading voters. Money merits inclusion as a persuasion activity, in theory at least, because it is a prerequisite to certain other persuasion activities, particularly the use of the mass media.

The assumption that these activities form two distinct clusters empirically was tested using the technique of factor analysis. The results of this factor analysis are presented in Table 1 with factor loadings above .50 underlined for emphasis.[20]

Table 1. Dimensions of Campaign Performance[a]

Activity	Factor I (Mobilization)	Factor II (Persuasion)	Variance Explained by Both Factors
Lit	−.43	.01	.19
Polls	.78	.01	.61
Affairs	.06	.59	.35
Money	.11	.68	.48
Media	−.21	.68	.51
Drives	.74	.01	.55
Variance Explained by Factor	.24	.21	.45

(Number of cases = 234)

[a] Each activity was measured as a trichotomous variable, coded jointly from the chairman's evaluation of performance and, to correct for overestimation, the priority ceded to the activity.

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edgeable of party performance at and below their level than any other party leaders. The use of judges' perceptions to measure performance of party activities can not be justified as easily. The inherent difficulties in measuring party performance directly have forced all previous studies to rely upon indirect measurements. Two types of these indirect measures have been utilized. Most studies have depended upon estimates by party leaders, usually precinct captains or county chairmen depending upon the level of analysis. An alternative method has been to compile estimates of party performance by aggregating voter perceptions of party contact from mass samples. (See Putnam, "Political Attitudes and the Local Community," p. 643; and see Kramer, "The Effects of Precinct-Level Canvassing on Voter Behavior," pp. 564–565.) The estimates of party leaders, while still indirect, are clearly preferable to those aggregated from a sample of voters. Not only do they capture a broader range of party behavior, but they are less contaminated by errors caused by selective perception and weak memories or by large sampling variances. Nonetheless, the use of an indirect measure is not wholly satisfactory and more work needs to focus on the development of measures of party performance.

[20] The Varimax solution is presented in Table 1. An eigenvalue of 1.00 was used as the cutoff for the admission of additional factors. This criterion is suggested by R. J. Rummel, *Applied Factor Analysis* (Evanston, Illinois: Northwestern University Press, 1970), pp. 362–364. The matrix which was factored to produce this solution contained coefficients based on equally weighted counties in both the South and the North. This weighting procedure was followed to produce a solution which reflected the behavior of county chairmen as nearly as possible. The results remain substantially the same when the counties are weighted by population size.

Precinct Organization was not included in the factor analysis presented in Table 1 because I expected it to form a separate, yet correlated, dimension of activity. The positive correlations between it and all campaign activities but literature distribution support this expectation. Furthermore, a two-factor Varimax solution for all seven measures of party activity virtually replicates Table 1 and adds Precinct Organization as an important item to each factor. While a three-factor oblique solution seems warranted given my expectations, the

That the solution produced two factors and that the items identified as *a priori* similar were found to load on these factors substantially as expected supports my theory that there are two dimensions of campaign activity. Factor I may be labeled "mobilization" and Factor II "persuasion." Taken together, these factors explain 45 per cent of the total item variance—not a large amount but certainly satisfactory, given the measurement unreliability expected with indirect measures of party performance. Had there been no underlying structure in these activities, the first two factors would have explained only 33 per cent of the item variance, and each would have contained only one item with a high loading.

Only the loading of the literature distribution item (Lit) fails to conform to expectations in that it is not associated with the practice of the other persuasion activities. As expected, however, it is negatively associated with mobilization activities. There is no obvious explanation for this anomalous result, and it clouds the empirical separation of mobilization and persuasion activities. Since the data show that this activity is performed well in almost all counties, though, one could surmise that it is perhaps so universal that it does not constitute an integral part of any campaign strategy.

The results of this factor analysis are important because they provide some empirical support for the theory that there are independent persuasion and mobilization dimensions of campaign performance. These results serve also as the empirical foundations for measures of the two dimensions. If the squared loadings of the important (underlined) items along each dimension are utilized as weights, party performance along each dimension may be represented as the weighted sum of the performance scores on those activities central to the dimension.[21] This factor scoring procedure yields two indices of party performance—Mobilization and Persuasion. The third index of party performance constructed for use in subsequent analysis, Precinct Organization, reflects simply the percentage of precincts in the county staffed with a chairman. Party performance in each county, of course, is measured separately for the two parties.

Measuring the County Environment

While there are many possible determinants of party behavior, my analysis is restricted to the two most frequently cited in studies of party activities—the political and demographic environments. The task of this section of the paper is to provide operational meaning for these two environmental conditions.

The political environment. At the conceptual level, it is easy to imagine most of the citizens in an area divided into two partisan camps, with a smaller number of individuals generally unwilling to commit themselves to the partisan fray. The political environment refers to this division of partisan strength. Were it not for the interstate differences in requirements for party declarations at registration time and the unwillingness of many partisans to state a preference for the public record, a direct count of registration rolls might provide an excellent estimate of the political environment. One possible alternative, to derive estimates of this partisan division from a voter survey, is highly impractical because of its cost. Another possible alternative is to measure the political environment from the voting record for several important offices over a relatively long period of time. This is the alternative adopted in this study. Not only are data on elections the only information on the distribution of partisanship readily available at the county level of analysis, but also these election results are perceived as meaningful by party strategists.

The measure of the political environment developed for this study is constructed from the popular vote for three offices—President, United States Representative, and Governor—from 1946 to 1962.[22] Unlike the standard measures of interparty competition, this measure has been constructed so as to estimate the partisan division beyond the sixteen-year interval (see Appendix I). The measure is titled Democratic Concentration because it ranges from a low of no Democrats to a high of all Democrats.

In order to test some of the hypotheses about environment and party, however, another measure of the political environment is required to reflect deviations in either direction from an equal

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results of this operation are meaningless because too little variance is left to be explained by the additional factors. More items, and particularly more items which reflect organizational activity, are required before a meaningful three-factor oblique solution can be extracted.

[21] This procedure for calculating factor scores, called the composite method, uses only those items with the highest loadings on the factor in scoring that factor; the factor loadings used here have been underlined. See Rummel, *Applied Factor Analysis,* pp. 441–442, for a discussion of the composite method.

[22] This time base is long enough to smooth out the irregularities which may result from the operation of short-term forces, yet not so long that it overlaps two electoral eras. (It would have had to extend back to 1930 to cross into another electoral era.) The offices chosen represent three of the four offices for which county-level data are readily available and are diverse enough so that local idiosyncrasies in voting for a particular office will not distort the measure of the general pattern of partisan division.

partisan division. Such a measure, called Competitiveness to denote absolute rather than pro-Democratic or pro-Republican competition, is constructed by simply folding the Democratic concentration measure at its mid-point so that the ends of the continuum are collapsed upon one another. The measure is scored so that its maximum value will be attained when there is an equal division of partisans.

The demographic environment. Data from the 1960 Census are utilized to measure the demographic characteristics of the population in each county. Following the direction of Scott, the concentration of voters with particularistic orientations toward politics is deemed to be most relevant to the behavior of local party organizations.[23] Such orientations are found among poor and dependent voters and those to whom parochial group loyalties are important in voting behavior. Thus, those variables in the Census which reflect the level of poverty in the county and the concentration of parochially oriented groups are utilized to measure the demographic environment. Three variables are indicators of poverty levels—median income, median education, and median home value. Two other variables seem to reflect, again *a priori*, the relative concentrations of groups which have exhibited loyalty to candidates from their groups in their voting behavior—the percentages of nonwhites and foreign born. Both groups have been counted among the clients of urban political machines. Two other county demographic characteristics—percentage urban and percentage rural—are included because they too are known to be strongly related to the concentrations of particularistic voters. (These two variables are not opposites. That is, while they are negatively correlated with one another, that correlation is far from perfect.)

That these seven demographic variables reflect some underlying traits is clear from their high intercorrelations. In order to identify clearly the underlying traits or dimensions, the matrix of intercorrelations was factor analyzed for southern and nonsouthern counties separately.[24] The Varimax solution is presented in Table 2 with factor loadings above .70 underlined for greater emphasis.[25] Both solutions contain two similar factors or underlying dimensions, and the variance in the items explained by these two factors exceeds 70 per cent in both regions. Only 29 per cent of the variance would have been explained had the items been independent.

Table 2. Dimensions of the Demographic Environment

County Characteristic	Factor I	Factor II	Variance Explained by Both Factors
	Northern Counties (Parochialism)	(Poverty)	
Med Income	.34	−.87	.80
Med Home Value	.54	−.31	.38
Med Education	−.15	−.92	.87
% Foreign Born	.78	−.14	.63
% Nonwhite	.83	.10	.70
% Urban	.70	−.59	.84
% Rural	−.57	.64	.74
Variance Explained by Factor	.37	.35	.72
	(Number of cases = 98)		
	Southern Counties (Nonwhite Concentration)	(Poverty)	
Med Income	−.28	−.85	.80
Med Home Value	.28	−.78	.67
Med Education	−.45	−.84	.90
% Foreign Born	−.05	−.69	.48
% Nonwhite	.95	.04	.90
% Urban	−.06	−.90	.81
% Rural	.06	.89	.79
Variance Explained by Factor	.18	.58	.76
	(Number of cases = 31)		

Labeling these factors is difficult because they reflect the clustering of county, not individual, characteristics. Nonetheless, the two-factor solution in each region fits remarkably well the notion that there are two distinct dimensions of particularism—poverty and parochialism—if regional differences are taken into account. In the northern counties, Factor I may be labeled Parochialism because of the high loading of minority group concentrations upon it, and Factor II may be labeled Poverty because of the high loadings of the indicators of poverty/affluence and education on it. In the southern counties, Factor II clearly warrants the title of Poverty. Factor I, on the other hand, is dominated only by the percentage of nonwhites and reflects one of the unique regional characteristics of the South—the heavy concentration of blacks in the old plantation areas called the "black belt." The style of politics in black-belt counties has been oriented towards maintaining white supremacy for almost one hundred years and has been accompanied by a highly restricted "whites only" electorate. While there is a large concentration of parochially oriented blacks (or, at least, potentially so in 1964) in these areas, local party organizations pay no attention to them because they are disenfranchised. Political circumstances in the South, then, lead to different expectations concerning the relationship between the concentration of parochially oriented blacks and party behavior. To reflect the political facts of southern politics, Factor I in this region will be labeled Nonwhite Concentration instead of Parochialism. Two measures of the demographic environment for each region are constructed from the results of the factor analysis by weighting the performance scores on the important (underlined) factors by the square of their factor loadings following the same factor scoring technique discussed above.[26]

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[23] Scott, "Corruption, Machine Politics, and Political Change," pp. 1145–1149

[24] This factor analysis was performed in exactly the same manner as that presented in Table 1, except that it was done for the two regions separately. The population compositions of the southern and nonsouthern counties were too different to allow for strictly equivalent measures and a meaningful general factor solution. The counties were weighted according to size in calculating correlation coefficients so that the results would be representative of the universe of county voter environments.

[25] The selection of .70 as a cutoff point for this table in contrast to the .50 used in Table 1 is somewhat arbitrary, but it was felt that this level captured a more meaningful factor structure than the lower level would have.

Summary. The independent variables in this study—the political and the demographic environments—are operationalized as described in this section. The political environment is measured so as to reflect the division of partisan strength in each county prior to the 1964 election. The demographic environment is measured along two dimensions in the North and South separately. One dimension, Poverty, had the same meaning in both regions. The second dimension, Parochialism in the North and Nonwhite Concentration in the South, is based on similar items in the two regions but requires nominal differentiation because of the unique features of southern politics.

The Research Hypotheses

A number of propositions concerning the impact of the environment on party behavior were derived from the research reviewed in the introduction. The principal task of this section is to reformulate these propositions into hypotheses which can be tested using the measures developed in previous pages.

The research on the relationships of political environment to party behavior, most of it empirical, provides some evidence for each of two contrasting hypotheses:

(1) The more equal the division of partisans in the constituency (i.e., the more competitive the constituency), the better each party will perform its activities.[27]

(2) The more the division of partisans in the constituency favors a party, the better that party will perform its activities.[28]

Each of these hypotheses implies the null hypothesis that no significant relationship exists between the political characteristics of the environment and party performance. Empirical support for this hypothesis too can be found in previous research.[29] Many of these studies fail to recognize the multidimensional nature of party behavior, implicitly assuming that their results can be generalized to include any type of party activity. In my testing of hypotheses derived from the studies, however, each dimension of party behavior has been examined separately so that empirical evidence can be brought directly to bear on this assumption.

Previous discussions of the relationships of the demographic environment to party behavior yield clear expectations concerning the impact of the demographic environment on each dimension of party behavior. These expectations may be formulated into the following hypotheses:

(3) There is no relationship between the demographic characteristics of the environment and performance of organizational maintenance activities (i.e., organizational maintenance is important in all environments).

(4) The greater the concentration of particularistic voters in a constituency, the better each party will perform service activities.

(5) The greater the concentration of particularistic voters in a constituency, the better each party will perform mobilization activities.

(6) The lesser the concentration of particularistic voters in a constituency, the better each party will perform persuasion activities.

These expectations are derived from what may be termed a "theory" of machine politics and have not been tested empirically in a systematic fashion previously. In contrast, hypotheses one and two are formulated from the research findings of previous studies—most of which articulate no general theory of environment and party.

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[26] These measures of the characteristics of county constituencies are undeniably crude. In an attempt to estimate the concentration of voters whose orientations to politics are what has been described as particularistic, I have been forced to use proxy measures. Instead of information on the distribution of voter orientations, I have used information on the distribution of voters who are likely, according to "machine theory," to have particularistic orientations—the concentration of minority group members and the median levels of "poverty." A good deal of measurement error must be anticipated in such measures.

[27] Crotty, "The Party Organization and Its Activities," and Key, *Southern Politics*, pp. 386–405.

[28] Eldersveld, *Political Parties*, p. 424.

[29] Cutright, "Measuring the Impact of Local Party Activity," and Putnam, "Political Attitudes and the Local Community."

Six major hypotheses have been specified for empirical testing. If the first two are expanded into hypotheses relating to each dimension of party behavior, then the total increases to twelve. Three of these hypotheses, unfortunately, cannot be tested with the data from my study because no information on service activities was solicited from county chairmen during the 1964 campaign. The tests of the three hypotheses involving the organizational dimension, furthermore, are to be regarded as only partial because of the restricted measure of this activity dimension. Even though not all hypotheses specified above will be subjected to empirical testing, it is hoped that specification of all of them will guide future research on environment and party. The remainder of the paper is devoted to the empirical examination of the nine hypotheses, including the ones involving precinct organization as the dependent variable, for which both the independent and dependent variables are measured.

Environment and Party Performance: The Empirical Patterns

The empirical analysis reported in this section of the paper is focused on the interrelationships between the three dependent variables—precinct organization, mobilization, and persuasion—and the four independent variables—Democratic Concentration, Competitiveness, Poverty, and Parochialism (or Nonwhite Concentration in the South).

This analysis is conducted for northern and southern counties separately to allow comparisons between what are generally recognized as two distinct political regions.[30] It is also conducted separately for the Democratic and Republican party organizations because the different coalitional bases of the two parties lead one to expect quite different responses to the same environment, particularly where demographic characteristics are concerned.[31]

Northern Counties

Table 3 contains the product-moment correlations between the four environmental characteristics and each of the three dimensions of party performance for northern counties. Three measures are presented for each dimension of party behavior—one for each party and a "relative" measure which reflects the performance differences between the parties. (Relative performance is measured by subtracting the Republican performance score from that of the Democrats and ranges from a high of maximum Democratic advantage to a low of maximum Republican advantage.) All coefficients of .20 and above are significant at or beyond the .05 level and are underlined for emphasis.[32]

Precinct organization. Of all the possible relationships between the different environmental variables and precinct organizational activities for each party, only two attain significance, and both of these involve Republican levels of performance. In support of hypothesis 3, there is no evidence that the demographic characteristics of the environment affect precinct organization for either party. On the other hand, both Competitiveness and Democratic Concentration have clear and strong impact on Republican performance. The more competitive the county, the more precincts have been staffed by the Republicans. The higher the relative concentration of Democratic supporters in the county, the fewer precincts have been staffed by the Republicans. By contrast, the parallel relationships for the Democrats approach zero.

Even though Competitiveness and Democratic Concentration are correlated at −.47 in the northern counties, the hypotheses involving them are sufficiently contradictory to one another that only one of them can be valid. Support for the other is based on a spurious relationship. This spuriousness can be identified through the use of partial correlations involving both independent variables and the dependent variable at the same

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[30] Parallel analyses of the two regions are required for two methodological reasons as well as the substantive one. First, important regional differences in the clustering of demographic features have precluded a factorial structure which is common to both regions. Second, the nationwide distribution of the political environment variable is bimodal and, as a result, seriously violates the normality assumption underlying correlation analysis. The regionally-specific distributions of this variable, however, are close enough to normality to satisfy the assumption. The South includes all the states of the Old Confederacy: Alabama, Arkansas, Florida, Georgia, Louisiana, Mississippi, North Carolina, South Carolina, Tennessee, Texas, and Virginia.

[31] Three additional points require elaboration prior to this analysis. First, party performance was measured for 1964 and the environment was measured prior to 1964 so that party performance could be used as a dependent variable. Second, the hypothesized relationships between the environment and the "dependent" activity variables are linear and have been tested with a measure of linear association. Examination of the bivariate scatterplots uncovers no theoretically meaningful departures from linearity. Third, since this is an analysis which is based on data from the 1964 election, generalizations to other years should be made with caution.

[32] Some criterion is needed to differentiate between those correlations which are meaningful and those which are not. Since a high probability that the relationship has not occurred by chance seems a prerequisite for meaningfulness, significance at or beyond the .05 level will be the criterion used in the analysis.

Table 3. Environment and Party Performance, North[a]

	Political Environment		Demographic Environment	
	Competitiveness	Democratic Concentration	Parochialism	Poverty
Precinct Organization				
Democrats	.00	.04	−.06	.16
Republicans	.43	−.56	−.05	−.11
Relative	−.31	.47	−.01	.22
Mobilization				
Democrats	−.06	.22	.35	−.18
Republicans	.25	−.07	.26	−.23
Relative	−.25	.27	.06	.05
Persuasion				
Democrats	.20	−.27	−.24	.01
Republicans	.15	−.25	−.30	.26
Relative	−.01	−.06	−.01	−.16

(Number of cases = 83 for Democrats, 96 for Republicans, and 81 for Relative)

[a] See Appendix II for partial correlations cited in text.

time.[33] The partial correlation between Competitiveness and Republican Precinct Organization controlling for Democratic Concentration is insignificant. On the other hand, the partial correlation between Democratic Concentration and Republican Precinct Organization controlling for Competitiveness is a robust −.41. The inference from these results is clear: *Precinct Organization, at least for the Republicans, is performed better the greater the electoral strength of that party in the county.* That is, for the GOP, strength begets strength.

To the extent to which precinct organization is critical to a party's electoral performance, however, it is the relative edge that one party enjoys over the other which is important. This edge, measured as Relative Precinct Organization, is related to the Poverty dimension of the demographic environment as well as to the two measures of political environment. Additional analysis shows that both the Relative Organization-Competitiveness and Relative Organization-Poverty relationships are spurious. Thus, more empirical support is found for the hypothesis that a party's organization effort is predicated upon its electoral strength. This finding implies that the minority party must overcome two hurdles if it is to become a majority. It lags behind in both electoral support and organizational activity.

Mobilization. Unlike the case for precinct organization, both the political and demographic environments appear to influence performance of mobilization activities. The impact of the demographic environment is basically the same for both parties. The greater the concentration of parochial voters in the county, the higher are Democratic and Republican mobilization performance levels. Where this aspect of demographic environment is concerned, then, hypothesis 5 receives empirical support. The relationships of the Poverty dimension with Mobilization, on the other hand, are opposite to those predicted by the hypothesis—a pattern that will be found rather consistently throughout the analysis and may possibly be explained by the fact that urban counties, even though they contain large numbers of poor and uneducated voters who seem to be "machine" clients, rank relatively low on the Poverty dimension (rural counties rank higher). One conclusion appears amply justified: *Mobilization activities are performed better for both parties*

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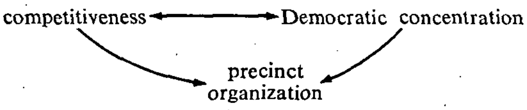
[33] This test for spuriousness assumes the following causal ordering among the variables:



Since competitiveness and Democratic concentration are highly intercorrelated, as are many other pairs of independent variables in this study, it is clearly possible that the relationships of either of them to the dependent variable may really reflect the influence of the other on both. Thus, the partial correlation coefficients must be used to sort out the influences of the independent variables. When the original zero-order correlation is substantially reduced by partialing, one may conclude that it is the control variable which is responsible for the bulk of the relationship.

the higher the concentration of parochially oriented voters.

This interparty similarity disappears when measures of the political environment are used, and a pattern appears which clarifies even more the responses of the two parties to environmental conditions. The Democrats are found to perform mobilization activities better as their electoral strength increases, while the Republicans perform them better as the competitiveness of the county increases. Democratic Concentration and Parochialism are correlated at .26, though, and partial correlation coefficients must be utilized to disentangle environmental effects for the Democrats. When this is completed, the relationship between Democratic Concentration and Mobilization performance by the Democrats approaches zero, while the partial correlation between Parochialism and Democratic Mobilization remains a high .31. Democratic Mobilization efforts seem to be affected by the concentration of parochial voters only. The relationships between Republican levels of Mobilization and both Competitiveness and Parochialism, on the other hand, remain undisturbed after partials are employed. Republicans, it would seem, make mobilization of parochials conditional upon the existence of close competition.

Persuasion. The environment has had somewhat different impact on each party's performance of precinct organizational and mobilization activities. This is not the case where persuasion is concerned. Not only are the relationships between each environmental variable and the performance of each party in similar directions in all cases, but they are of almost the same magnitudes in three of the four comparisons. Both parties seem to perform persuasion activities less as the electorate becomes more Democratic, as the county becomes more one-party dominated, and as the concentration of parochial voters increases. The Republicans also perform significantly better in the poor counties (which, it should be remembered, are largely nonurban).

Taken together, these findings should be read so as to support the aspects of hypothesis 6 which relate to parochialism—*performance of persuasion activities is low where there are large concentrations of group-oriented voters.* This interpretation is reinforced by examination of the first-order partials. After controls have been imposed, both parties still eschew persuasion activities in those counties where the concentrations of parochial voters and Democrats are high, emphasizing these kinds of activities in rural, Republican-dominated counties. These are the counties where, traditionally, the anti-partisan political culture has been the strongest. It seems reasonable that both parties should emphasize persuasion activities there. Great care must be taken lest this argument be overstated. After all, the coefficients are not high, and in most cases they barely pass the cutoff point for significance. What is most striking about these findings is that they run in the same direction for both parties. Where the political climate seems to encourage persuasive appeals, both parties will respond by relying upon them.

Southern Counties

Table 4 contains the correlations between the four environmental characteristics and each of the three dimensions of party performance for the southern counties. As before three measures are to be presented for each dimension of party behavior. All coefficients of .35 and above are significant at or beyond the .05 level and are underlined for emphasis.

Precinct organization. Unlike the case in the North, the Democrats and the Republicans respond similarly to the environment in their performance of precinct organizational activities. Each comparable pair of coefficients goes in the same direction with differences only in magnitude. Both parties are better organized in the competitive counties of the South and show their lowest levels of organization in those counties with large nonwhite concentrations, where Democratic hegemony has traditionally gone unchallenged. None of the relationships between environment and party performance attains significance for the Democrats, however, although the one involving Competitiveness is near enough to the threshold to warrant attention. By contrast, both features of the political environment and Nonwhite Concentration are significantly related to Republican performance levels.

The three environmental variables which are strongly related to Republican activity are themselves highly intercorrelated,[34] and their contributions must be disentangled through the use of partial correlations if one is to get a clear picture of the impact of the environment on Republican precinct organization. When the partials for each triad of variables are computed, both Competitiveness and Nonwhite Concentration are found to contribute to organizational performance.[35] Re-

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[34] Competitiveness and Democratic concentration are almost perfectly correlated at −.94, and this confounds substantive interpretations as will be seen later. The relationship between nonwhite concentration and competitiveness is −.64, while that between nonwhite concentration and Democratic concentration is .63.

[35] The scarcity of counties with pro-Republican political environments is clearly reflected in the near-perfect correlation between Democratic concentration

Table 4. Environment and Party Performance, South[a]

	Political Environment		Demographic Environment	
	Competitiveness	Democratic Concentration	Nonwhite Concentration	Poverty
Precinct Organization				
Democrats	.32	−.24	−.24	−.26
Republicans	.72	−.69	−.71	−.34
Relative	−.27	.30	.32	.01
Mobilization				
Democrats	−.25	.25	.04	.20
Republicans	.31	−.19	−.41	.01
Relative	−.42	.33	.32	.14
Persuasion				
Democrats	.15	.06	−.20	−.16
Republicans	.26	−.23	−.13	−.11
Relative	−.11	.20	.00	.03

(Number of cases = 26 for Democrats, 29 for Republicans, and 24 for Relative)

[a] See Appendix II for partial correlations cited in text.

publicans organize better as competition increases and the concentration of nonwhites decreases. Both findings seem consistent with the conventional wisdom concerning southern politics. There is little incentive for Republicans to be well organized in the precincts in counties where they never receive more than minimal support and where such organization might threaten the "white monopolies" on politics in the black-belt counties.

The pattern of environmental effects becomes more complex, as well as more substantively interesting, when relative performance levels are examined. Democratic performance advantages are found where Democratic electoral strength is high and where nonwhites comprise a substantial portion of the population. Thus, *even though Democratic performance levels are lowest in the "black-belt" counties of the South, they are high enough to surpass the meager performance of the Republicans.* Democratic electoral monopolies in these counties are reinforced by a virtual monopoly on organizational activity. This Democratic performance advantage is overcome by increases in Republican activity as the counties become more competitive. Even so, the Democrats sustain a higher average level of performance in the region with 71 per cent of the precincts organized to 63 per cent for the Republicans.

Mobilization. The patterns of mobilization performance in the South differ greatly from those in the North. That only one of the coefficients attains significance, the one relating Nonwhite Concentration to Republican performance, underlines the uniqueness of the South as a political region. One would expect mobilization activities to be performed most extensively, *ceteris paribus*, in those black-belt counties with high concentrations of parochially oriented black voters given the findings for the northern counties. Yet other things are clearly not equal. These counties, in response to the threat to white supremacy posed by a large black population, have had the most restricted electorates of any counties in the nation since the Civil War. The last thing that party leaders there want to do is mobilize voters, because in many cases this might mean the mobilization of black voters. Thus, the strong differences in the political cultures of the two regions can be cited to explain the relationship between demographic environment and party performance of mobilization activities in the South.

If attention is focused on relative performance levels, the regional differences appear much less sharp. *The more competitive the county, the greater the Republican edge in mobilization performance.* Again the implication of these findings is not completely clear because of the skewed nature of the political environment continuum in the South. The other coefficients are also consistent with those in the North even though they do not attain the magnitude necessary for significance. Democrats perform mobilization activities better

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and competitiveness. Because of this, scores on either variable reflect the other as well. Any inferences made regarding the relative importance of the two political variables rest on those few counties which are pro-Republican. These inferences must be regarded as highly tentative.

where their electoral edge is greater and where the concentration of nonwhites is higher.

Persuasion. There is some similarity in the response of each party to the environment in the South and between the two regions where performance along the Persuasion dimension is concerned. This similarity hardly warrants discussion, however, because *none of the relationships between the environmental variables and Persuasion approach significance in the South.* The explanation for variations in the performance of persuasion activities is not to be found in the kinds of environmental variables which have been operationalized in this study.

Summary and Interpretation of Empirical Patterns

The empirical patterns reported in the preceding section are most meaningful when used to test the validity of the hypotheses advanced earlier. In this way additional empirical grounding for a theory of environment and party can be laid. Table 5 portrays the results of the empirical testing of each hypothesis. Several different results are possible, and each is represented by a specific symbol in the table.[36]

The most obvious conclusion which can be drawn from Table 5 is that there is support in the northern counties for the theory which links the demographic environment to party behavior. As predicted by this theory, party performance of campaign activities is related to the concentration of parochial voters. Furthermore, as expected considering the pivotal role of organization for all campaign activities, Precinct Organization does not vary with the demographic environment.

Table 5. The Results of Hypotheses Testing

	HYPOTHESIS 1 (competitiveness)	HYPOTHESIS 2 (Demo. concentration)	HYPOTHESES 3–6 (machine theory)
NORTH			
Organization			
Demo.	R_n	S*	S
Rep.	R_s	S	S
Service			
Demo.	U	U	U
Rep.	U	U	U
Mobilization			
Demo.	R_n	R_s	S
Rep.	S	S*	S
Persuasion			
Demo.	R_s	R_o	S
Rep.	R_n	S	S
SOUTH			
Organization			
Demo.	R_n	R_n	S
Rep.	S	R_s	S
Service			
Demo.	U	U	U
Rep.	U	U	U
Mobilization			
Demo.	R_n	R_n	R_n
Rep.	S*	R_n	R_o
Persuasion			
Demo.	R_n	R_n	R_n
Rep.	R_n	R_n	R_n

KEY: U = untested
S = supported
S* = supported using relative performance measure
R_n = rejected because no relationship was found
R_s = rejected because only a spurious relationship was found
R_o = rejected because relationship did not go in the hypothesized direction

The failure of the Poverty dimension to show any relationships to party performance, however, was not expected and requires that the theory be revised. The poorest counties in the North are rural, as can be seen from Table 2. Political machines have been rare in these counties in the past, and the types of activities which characterize the political machine are not practiced there in the present. One can only conclude from this finding that relative poverty alone is not sufficient to stimulate the development of machine-style party organizations, with their twin emphases on service and mobilization. Perhaps this poverty has to fall closer to the subsistence level than it does in these counties for machines to emerge. One may also speculate that political machines will emerge only where there is a demand for large-scale governmental activity. Where government performs few services, as is the case in sparsely settled rural areas, the kind of "profits" necessary to sustain a machine may be absent. Whatever explanation for the unimportance of the poverty dimension proves valid, one thing is apparent: in modern

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[36] The hypotheses involving the service dimension went untested as is indicated by the letter "U." Some hypotheses were supported in the analysis using the party-specific performance measures as is indicated by the letter "S." The few hypotheses which did not receive empirical support in the party-specific analysis but did receive it where the relative performance measures were utilized are indicated by "S*." Rejection of the hypothesis is symbolized by the letter "R," and each R is followed by a subscript to indicate the reason for rejection. Where rejection was warranted because no *significant* relationship was found, the subscript is "n." Where a significant relationship was found which vanished when partials were employed, the hypothesis was rejected due to a spurious relationship as is indicated by the letter "s." Where the hypothesis was rejected because the relationship was significant but not in the direction predicted by the hypothesis, the subscript "o" was used.

America, the vestiges of the machine-style political organization rest on large concentrations of parochially oriented voters.

The theory of demographic environment and party receives much less empirical support in the South, where unique features of southern politics probably render it inapplicable. Only the *absence* of a relationship between the demographic environment and Precinct Organization conforms to the expectations of the theory, and it is dangerous to base even tentative support for a theory on the confirmation of null hypotheses.

When one turns to an examination of the hypotheses involving the political environment and party behavior, it is immediately apparent why previous empirical studies of these relationships have produced seemingly contradictory results. The kind of relationship between political environment and party performance which emerges in any particular case depends not only on the performance dimension analyzed but also on both the party and the region of the country. Thus, in the data analyzed here, there is support for the conclusions Key and Crotty drew concerning the South, those Eldersveld proposed concerning Wayne County, and also those Putnam and Cutright outlined in their studies. Yet, just as is true when one attempts to generalize from these preceding studies, no clear pattern emerges overall in this one. Thus, both parties in the North appear to organize better as their electoral edge increases, but this is not true in the South. The Republicans perform persuasion activities better as their electoral strength grows in the North, but again this is not the case in the Old Confederacy. Only for Republican mobilization activities is the impact of the environment the same in both regions: as the Competitiveness of the county increases, so too does Republican performance along the Mobilization dimension.

One of the most theoretically interesting findings in this study is obscured by the hypothesis-by-hypothesis mode of presentation employed in Table 5. It involves the interaction of the political and demographic environments. In the North, the Democrats perform mobilization activities better as the concentration of Democratic voters increases, while the Republicans perform these activities better as the county becomes more competitive. Also in this region, Democrats perform persuasion activities better as the constituency becomes more competitive, and Republicans perform them better as the concentration of Republican voters increases. These contrasting performance patterns may be explained by reference to the demographic characteristics of each party's basic constituency. Because voters who require mobilization come generally from groups with pro-Democratic orientations, mobilization is more naturally a Democratic activity. On the other hand, because those groups most affected by persuasive appeals are more likely to be pro-Republican, persuasion is more naturally a Republican activity. Each party performs its "natural" activity better as the concentration of voters becomes more favorable to it. Conversely, each party performs the other party's "natural" activity better as the constituency becomes more competitive. One may infer from these findings that, as the challenge posed by the opposition grows, the incentive increases for a party to practice activities which promise to yield relatively small returns. This tendency also appears for Mobilization in the South.

A focus on only those relationships which are significant, moreover, conceals substantial similarity between the North and South. Of the sixteen pairs of variables which were significantly related in the North, only three have the opposite sign in the South, and two of these are near zero in magnitude. In fact, most of the significant relationships for northern counties are paralleled by high, though not significant, relationships in the southern counties. In other words, the clearest differences between the two regions involve the magnitudes of the relationships rather than their directions. It seems likely that the historic lack of interparty competition in the South has attenuated the impact of environment on party there. No matter what the county environment, and it almost always favors the Democrats, it is uniformly embedded in a heavily Democratic state political system. The impact of race on southern politics may also attenuate the environment-party correlations. Preservation of white supremacy has dominated the politics of that region for decades and has left an indelible mark on both southerners and their political institutions.

Since the Second World War, interparty competition has increased greatly in the South, particularly in presidential elections. While most counties in this region were still not competitive in all elections by 1964, it should be only a matter of time before the politics of the South "joins the union." It remains to be seen, however, whether this trend reflects the "southernization" of national politics, with race as the major political cleavage, or the "nationalization" of southern politics. Nonetheless, as recently as 1964, there were still marked differences between North and South in the relationships between environment and party behavior. While the similarity in direction of these relationships, if not their magnitudes, provides some reason to predict a convergence of the South upon the empirical patterns of the North where environment and party are concerned, the South retains its distinctiveness as a political region in this study.

Conclusion

This research has been designed to make two principal contributions to the study of political parties. First, party behavior was measured within a theoretical framework which recognized the multidimensionality of party activities. Unlike many earlier measures of party behavior, these measures were equivalent across environments and, as a result, were well-suited for use as dependent variables in an analysis of the impact of the environment on party behavior. It is hoped that this new approach will foster additional research on determinants of party activity. Second, the impact of certain political and demographic characteristics on performance along three dimensions of party behavior—Precinct Organization, Mobilization, and Persuasion—was examined empirically using data on county party organizations in 1964. The measures of these dimensions can and should be improved, and other campaigns should be analyzed as well. Nonetheless, the research reported in this article provides empirical evidence on the relationship between environment and party and may serve as a guide, as well as a benchmark, for further exploration.

It is unfortunate that, for all the attention paid to political parties, so few researchers have focused on the interaction of a party organization and its environment. Parties are vote-oriented institutions in modern democracies and, as a result, must reflect the nature of their constituencies if they are to survive. One index of political change may well be the behavior of political parties. Not only may counties or nations be arrayed across a range of environmental conditions at any point in time, but both may be arrayed along a temporal dimension as well. In fact, in the continuing absence of longitudinal data on party behavior, variation at any one point in time can be used as a surrogate for temporal variation by assuming that areas are at different stages of development along a single continuum.[37]

The twentieth century has witnessed massive changes in party organizations. Political machines have declined and, indeed, have been pronounced dead in most American cities. More recently, a new style of political campaigning has emerged with a heavy emphasis on the "marketing" of candidates via the mass media—a technique with the clear purpose of persuasion. Campaigning has come a long way since the 1896 election in which William Jennings Bryan gave more than 3,000 speeches and traveled to the most remote parts of the nation and William McKinley entertained thousands on his front porch. Even the resurgence of personal contact with voters as an important campaign activity, best exemplified by the extraordinary successes of the primary organizations of McCarthy in 1968 and McGovern in 1972, has a new flavor. It has retreated from the old methods of mobilization and continuous service to an emphasis on persuasive appeals. Most students of politics who have attempted to explain these changes have attributed them to the changing nature of the electorate. Yet no one has undertaken to provide empirical foundations for this explanation by examining the relationship between environment, particularly the demographic characteristics of the electorate, and party over time. While the theoretical frame developed in this study has been applied to cross-sectional data here, it seems to have a clear applicability to research focusing on such questions of political change.

APPENDIX I

The measure of political environment was constructed in four separate steps. (1) The vote division for each office in each year was reduced to a single index number by subtracting the Republican percentage from the Democratic percentage and adding one hundred to remove the minus sign. (2) An estimate of the partisan division in each year was computed by averaging the index number for each contest in that year and, where appropriate, the index number for the gubernatorial contest in the previous year. (3) These yearly averages of partisan balance were regressed on time to provide an estimate of the trend in partisanship during the time period. This trend was represented by the slope of the regression line. That is, time was the independent variable, and the index of partisan division for that year the dependent variable. The equation may be expressed as follows:

$$Y_i = a + bX_i + e_i$$

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[37] For example, the finding that both parties in the North do not perform persuasion activities very well in those counties which contain large concentrations of nonwhites and foreign-borns suggests that persuasion was a rarely practiced activity in major American cities during the late 1800s and early 1900s. To the extent to which these county characteristics reflect parochial orientations to political life, one may argue also that as group identifications become less salient, persuasion will be practiced more by both parties. Similar inferences could be drawn from the impact of concentrations of these types of citizens on Democratic mobilization activities. There has probably been a decline in the emphasis on mobilization as immigrant groups have been assimilated into the mainstream of American society, although this decline has been slowed partially by the emergence of "new" immigrant groups—blacks from the rural South and Puerto Ricans. As these new immigrants are integrated into American society (if that ever happens) and their places are not taken by new waves of immigrants, performance of mobilization activities will decline and political machines will finally vanish.

where $i=46, 48, \cdots, 62$; $X_i=i$; and $Y_i=$ the index of partisan division for year i. (4) the final measure of the political environment, called Democratic concentration, was computed by taking into account both the average partisan division during the time period and the trend. The precise functional relationship is the following:

$$\text{Democratic concentration} = \overline{Y} + 5b$$

where Y=1946–1962 mean partisan division and b=slope of the regression line. The coefficient "5" is used because 1964 (the year which is being projected) is five units past the mean year (1954) in the time series.

APPENDIX II

The first-order partial correlation coefficients discussed in the text are contained in the following table. Coefficients which are significant at or beyond the .05 level have been underlined. (The minimum coefficient for significance in the North is .24, while the minimum in the South is .43.) The conventional "dot" notation is used to symbolize each partial, the subscripts are x, y, and z for each type of party activity, respectively, and

1 = Competitiveness
2 = Democratic Concentration
3 = Parochialism in North/Nonwhite Concentration in South
4 = Poverty

NORTH

Precinct Organization (x)

Democrats	Republicans	Relative
—	$r_x1\cdot2=\ .23$	$r_x1\cdot2=-.12$
	$r_x2\cdot1=-.41$	$r_x2\cdot1=\ .\underline{38}$
		$r_x2\cdot4=\ .\underline{54}$
		$r_x4\cdot2=\ .16$

Mobilization (y)

Democrats	Republicans	Relative
$r_y2\cdot3=\ .14$	$r_y1\cdot3=\ .23$	—
$r_y3.2=\ .\underline{31}$	$r_y3.1=\ .\underline{25}$	

Persuasion (z)

Democrats	Republicans	Relative
$r_z1\cdot2=\ .12$	$r_z2\cdot3=-.18$	—
$r_z2\cdot1=-.\underline{30}$	$r_z3\cdot2=-.\underline{26}$	
$r_z2\cdot3=-.22$	$r_z2\cdot4=-.\underline{32}$	
$r_z3\cdot2=-.18$	$r_z4\cdot2=\ .\underline{33}$	

SOUTH

Precinct Organization (x)

Democrats	Republicans	Relative
—	$r_x1\cdot2=\ .28$	—
	$r_x2\cdot1=-.04$	
	$r_x1.3=\ .\underline{50}$	
	$r_x3.1=-.\underline{47}$	
	$r_x2.3=-.\underline{44}$	
	$r_x3.2=-.\underline{50}$	

Conflict in the Community: A Theory of the Effects of Community Size

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Introduction

The decade of the 1960s has been a decade of urban conflict and violence, and, as a result, the study of community conflict has suddenly emerged as an important subfield in the discipline of political science. After years of neglect, the study of conflict, especially violence and urban conflict, has become fashionable, but like so many fashions, the "styles" of inquiry have been dictated more by the concerns of the moment than by a desire to build toward more general theories of conflict.[1]

Although it is certainly understandable that the violence in our great urban centers would attract intensive study, the academic rush to supply the public with "scientific" understanding of such phenomena has led to several problems with the study of conflict. One problem has been the tendency to focus rather exclusively on *violent* forms of conflict. The difficulty is, of course, that violence is only a single form of conflict (and certainly not the most common form of conflict in the urban setting), and an exclusive focus on violence will almost certainly lead to a rather one-sided view of conflict.

Another problem has been the equally understandable tendency to focus the violence in the large cities. For the most part, both the smaller cities and the cities in which riots did not occur have been excluded from study. This is unfortunate from two perspectives. On the one hand, the almost exclusive emphasis on the large city provides us with little information about whether the dynamics of community conflict operate differently in other kinds of communities. On the other hand, we have almost no information about the effects of nonviolent conflict—effects that might include the political instability of decision-makers, opinion polarization, and a host of other phenomena.

Older traditions in this area, however, examine both the nonviolent dimensions of conflict and the effects of conflict under various environmental circumstances. One such tradition involves a theory of intra-group conflict that links both the incidence and the intensity of conflict within a group to the size of the group. Proposed first by Georg Simmel and later elaborated by Lewis Coser, the theory suggests that the *frequency* of intra-group conflict is positively related to the size of a group while the *intensity* of conflict is negatively related to the size of the group.[2] The implication is, of course, that large groups should tend to have frequent conflicts, but of low or moderate intensity, while small groups should tend to have infrequent conflicts, but conflicts that tend to become severe when they occur. Although it was originally developed for groups, this theory may be applicable for communities as well; and one variant of this theory will be examined in this paper.

There is a second and more recent approach to the study of conflict that also has relevance to this analysis. This approach suggests that conflictual behavior can best be understood by assuming that the actors who are party to a conflict are attempting to behave in a rational manner; that conflict, even severe conflict, is not the product of some set of blindly irrational forces within man, but the result of people trying to do the best for themselves given the circumstances in which they must operate.[3] This is not to suggest that politics is entirely free of lunatics; there have been too many assassinations for that. The primary point is that conflict, and certainly community conflict, is the outcome of differences in preferences and tastes rather than the inevitable consequence of the "dark" motives that supposedly lurk within people.

The setting for this study comprises the 89 cities of the San Francisco Bay Area.[4] The data, collected under the auspices of the City Council Research Project, include election statistics covering approximately a ten year period in each city, demographic information obtained from United States Census reports, and interview material obtained in an extended interview with 435 of the city councilmen in the area.[5]

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[1] There are a number of exceptions to this generalization, but these "theories" of conflict were developed before the decade of riots. Cf. Kenneth E. Boulding, *Conflict and Defense: A General Theory* (New York: Harper and Row, 1962); James S. Coleman, *Community Conflict* (New York: The Free Press, 1967); Lewis Coser, *The Functions of Social Conflict* (New York: The Free Press, 1956); Ralf Dahrendorf, *Class and Class Conflict in Industrial Society* (Stanford: Stanford University Press, 1959); Thomas Schelling, *The Strategy of Conflict* (Cambridge, Mass.: Harvard University Press, 1960); and Anatol Rapoport, *Fights, Games, and Debates* (Ann Arbor, Mich.: The University of Michigan Press, 1960).

[2] Georg Simmel, *Conflict and the Web of Group Affiliations*, trans. by Reinhard Bendix and Kurt Wolff (Glencoe, Ill.: The Free Press, 1955); Georg Simmel, *The Sociology of Georg Simmel*, trans. and ed. Kurt Wolff (Glencoe, Ill.: The Free Press, 1950); and Coser, pp. 73–80.

[3] Every author cited in the first footnote adopts this perspective.

[4] The larger project of which this analysis is a

A Theory of Community Conflict

Three Propositions. The Simmel-Coser theory of group conflict rests on three general hypotheses about the characteristics of groups of differing sizes. The first is a simple proposition about the distribution of preferences within a group: the larger the group, the more likely is that group to contain diverse social elements.[6]

The second proposition describes the relationship between the size of a group and the character of the interaction that occurs among group members. The small group, Simmel and Coser suggest, is characterized by face-to-face relationships in which the interaction between group members is frequent, direct, affectively charged, and diffuse. In secondary associations, by contrast, interaction between group members tends to be less frequent, more indirect, emotionally neutral, and specific. In the former case, for example, they argue that relationships are likely to involve emotional ties between group members that tend to encompass the total personalities of the individuals, whereas in the latter the relationships tend to be functionally specific, i.e., the interaction of group members is limited to the group roles they occupy and the interests they represent.[7]

Simmel and Coser's third hypothesis is that the involvement of the members in group activities is likely to be greater in small groups than in larger associations. Coser, however, criticizes Simmel because he failed analytically to separate group size and the involvement of group members.

> Simmel tends to equate size of group with degree of involvement of its members. Although there is much evidence to suggest that the smaller the group the greater the involvement of the participants, since interaction among a few members tends to be more intense than interaction among many, we cannot assume that this relationship holds invariably. . . . We must therefore distinguish between two aspects of group structure: (1) the numerical size of the group and (2) the degree of involvement of group members—although generally we can expect a simultaneous variation of these two factors.[8]

Neither Simmel nor Coser, however, provide any firm theoretical reason for the greater involvement of members of small groups as compared to the members of larger groups, and neither author delineates the principal factors that might lead to more intensive involvement by group members in general. We will turn to an analysis of this question in a moment, but for now we will accept the hypothesis that group size and the involvement of group members are positively related.

The Implications for Community Conflict. From these hypotheses, Simmel and Coser proceed to formulate a theory about the frequency and intensity of conflict in groups of varying sizes. In the small group, the members interact frequently and face-to-face, and they tend to be more highly involved in group life than the members of larger associations of the same type. Coser initially argues that the intensive interaction in the small group provides many *occasions* for conflict,[9] an idea he later modifies by suggesting that: "Close relationships, though providing frequent occasions for conflict, exhibit tendencies toward the supression of these conflicts. If, however, conflicts occur despite suppression, they tend to disrupt the relationship because they are likely to assume peculiar intensity due to the total involvement of the personality and the accumulation of suppressed hostilities."[10]

Thus, although there may be frequent *occasions* for conflict in small groups, the actual *frequency* of conflict is lessened by a supposed tendency for the suppression of conflict in the small group. At the same time, the closeness of the interpersonal relationships and the tendency for hostilities to go unexpressed intensifies those conflicts *that do occur*.

Conflicts are less frequent in small groups not only because they are suppressed, however, but also because the interests of group members are likely to be less divergent. Interaction will produce conflict only when there is something to fight about.

The general argument for large groups is the

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part, the City Council Research Project, is sponsored by the Institute of Political Studies, Stanford University, and is supported by the National Science Foundation under contract GS 496 and GS 1898.

[5] The interviews are with 435 out of a possible 490 subjects. San Francisco was not included because it has a local government which combines city and county functions and structures. *All of the cities are nonpartisan.*

[6] Coser, *Functions of Social Conflict*, p. 144; Simmel, *The Sociology of Georg Simmel*, p. 397.

[7] Coser, chapter 4; Simmel, *The Sociology . . .*, p. 320.

[8] Coser, p. 97.

[9] *Ibid.*, p. 73.

[10] *Ibid.*, Coser also states (p. 79): "Yet, frequent *occasions* do not necessarily eventuate in frequent conflicts. It is precisely the closeness of the relationship and the strong affective mutual attachment of the participants which may induce them to avoid further intensification of the conflict once it breaks out."

converse of the argument for small groups. In the large group, the diversity of interests creates numerous conflicts, but contrary to having a disrupting effect, these conflicts are less intense than those within smaller groups.[11] In large groups, as the argument goes, the involvement of members in group life will tend to be low, interaction among group members will tend to be less frequent and more specific, and the conflicts will tend to be less intense.[12]

Drawing on this theory by Georg Simmel and Lewis Coser, I have examined the data from the cities in this study to see if they reflect two general propositions inferred from Simmel and Coser: (1) the larger the city, the more *frequent* will be the incidence of conflict and contention, while (2) the smaller the city, the more *intense* will be the conflict or contention.[13] I will return to these propositions after exploring an alternate approach to community conflict.

Electoral Structures and Electoral Costs: the Rationalistic Theory

Pluralism and the Number of Minorities: An alternative theoretical approach, involving *costs* and *risks*, may prove to have greater generality than the theory of Simmel and Coser. The initial hypothesis in this theory is the same as the first hypothesis for Simmel and Coser; namely that the larger a community, the more heterogeneous the attitudinal distributions within the community.[14]

But the relationship between city size and heterogeneity goes beyond a simple, linear increase in attitudinal diversity. Attitudinal differences are linked to group differences, and groups provide the foundation for coalition formation in city politics. Racial, ethnic, occupational, and religious groups tend to vote as groups, but they also have associations and representatives who speak "for" the group in the process of community coalition formation.[15] Size, therefore, tends to be associated with the *number* of minorities in a community as well as with the simple increase in the overall diversity in political preferences; and the number of minorities in a community is likely to have a profound effect on the whole process of coalition formation.[16]

The importance of the number of minorities in a community derives from the effects that the number of minorities has on two different types of political calculations: the costs that individuals and groups must sustain in attempting to mobilize other groups for political action and the risks that are associated with the various possible outcomes of community controversy. The first factor will be referred to as the *political mobilization cost* whereas the second factor will be described as the

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[11] Coser argues: "Loosely structured groups and open societies, by allowing conflicts, institute safeguards against the type of conflict which would endanger basic consensus and thereby minimize the danger of divergences touching core values. The interdependence of antagonistic groups and the crisscrossing within such societies of conflicts, which serve to "sew the social system together by cancelling each other out, thus prevent disintegration along one primary line of cleavage," p. 80.

[12] This is not a new idea; and it can be found in a large number of places. Peter Blau, for example, says about the same thing: "Without cross affiliations, conflicts tend to be cumulative as many involve the same split in the community, and the predominant communications within each opposition camp may lead to intense hostility and endeavors not merely to defeat the opposition but to destroy it." Peter M. Blau, *Exchange and Power in Social Life* (New York: John Wiley and Sons, 1964), p. 306; see also, Coleman, pp. 21–23; Robert A. Dahl, *Pluralist Democracy in the United States* (Chicago: Rand McNally and Co., 1967); William Kornhauser, *The Politics of Mass Society* (Glencoe, Ill.: The Free Press, 1959), pp. 80–81; and Sidney Verba, "Organizational Membership and Democratic Consensus," *The Journal of Politics,* Vol. 27 (August, 1965), 468–470.

[13] The Coser/Simmel theory was propounded for groups, not for cities, and it remains questionable whether Simmel's and Coser's ideas can be applied to the cities of this study. Their analysis is based on the qualitative character of relationships in small groups; groups which are characterized by intense, face-to-face interaction, where the relationships are personal, affective, and relatively stable. This kind of relationship is contrasted with large groups in which relationships are affectively neutral and distant and lack intensity. Although all of our cities would, on the face of it, seem to fall into the category of the large secondary association, the *politically active stratum,* within which conflicts are fought out, is much smaller and may have all the characteristics of a small group. Take for example a city with 30,000 people. The number of registered voters in such a city will be around twelve thousand, and in any given election one might expect a turnout of about five to six thousand. If 5 per cent of the voters are active on a regular basis, a generous figure, that means the city will have a "politically active stratum" of about 300. Politically active people interact with great regularity, and that number is not too large to have many of the characteristics of the Coser/Simmel group.

[14] For extensive empirical evidence of such a relationship between city size and social diversity, see Jeffrey K. Hadden and Edgar F. Borgatta, *American Cities: Their Social Characteristics* (Chicago: Rand McNally, 1965).

[15] Cf. Michael Parenti, "Ethnic Politics and the Persistence of Ethnic Identification," *American Political Science Review,* 61 (September, 1967), 717–726; Raymond E. Wolfinger, "The Development and Persistence of Ethnic Voting," *American Political Science Review,* 59 (December, 1965), 896–908.

[16] Parenti, pp. 720–23; Robert C. Wood, *Suburbia: Its People and Their Politics* (Boston: Houghton Mifflin Co., 1958), p. 178; Scott Greer, "Catholic Voters and the Democratic Party," *Public Opinion Quarterly,* 25 (Winter, 1961), 624; and Stanley Lieberson, "Suburbs and Ethnic Residential Patterns," *American Journal of Sociology,* 67 (May, 1962), 673–681.

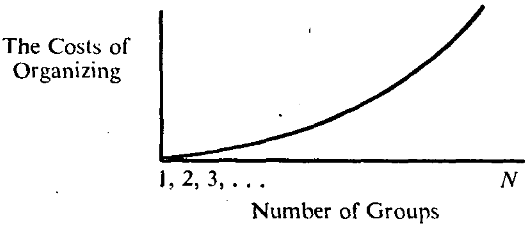

Figure 1. The Costs of Organizing as a Function of Number of Groups

expected political risk. Both these factors are directly related to the number of minorities in a community.

The Cost of Political Mobilization. The first, and perhaps the most difficult, problem of political life is that of organizing a coalition to pursue political action. Organizing a coalition involves a sizeable expenditure of time, effort, and money; and the more factions or minorities in a community, the greater will be the cost of mobilizing one's coalition for a conflict. Because the number of factions or minorities in a community is partly a function of the size of a community, the average costs involved in political mobilization also will be related to the size of a community. Such costs undoubtedly have a dramatic effect on the degree to which conflicts escalate into community battles.[17]

In the small community, where mobilizational costs are relatively low, the participants in a conflict can, in their search for additional allies, easily involve most of the interested individuals and groups in the community. In the large community, by contrast, mobilization is both expensive and difficult, and the costs associated with total community mobilization probably inhibit such attempts except under unusual circumstances.[18]

The problems of mobilizing the community are compounded when the informational level and sense of political involvement of the public are low. Problems that arise in a large community will tend to be more complex, involve more bureaucratic and institutional elements within the city, and involve greater numbers of individuals and groups. Thus, the informational requirements for effective political action are likely to be higher while the informational level of the average citizen is likely to be lower. Information is generally costly to acquire, and a higher informational requirement for effective participation is likely to mean that fewer people will take the trouble to acquire the information necessary for successful participation. Thus, in larger communities, one sees the participants to conflicts engaging in the ritual of "informing the public," a pattern of behavior seldom seen in smaller communities.

In addition, if the voter evaluates the prospect of voting partly in terms of the probability that his vote might have some effect on the outcome of elections, then we might expect that the citizen of the small community would be more likely to vote and to participate in community affairs (all other things being equal).[19] The reason is simple (and usually put forward as a defense of the small community): where there are fewer voters, the individual may feel that he has more of a chance of affecting the decisions that are made in his community. This may account for greater citizen involvement in the small community, if greater involvement is in fact the case.

All three of these arguments can be summarized in slightly different language:

Proposition I: The larger the population of a community, the greater the number of distinct groups, factions, and minorities that will be found within the community's boundaries.

Proposition II: The expected costs of organizing for collective action will be an increasing function of the number of groups, factions, and minorities that are found in the community.

A. If the average level of information of the citizenry in a community is a decreasing function of the size of a community, then the cost per unit of organization will be marginally increasing.[20]
B. If the average level of citizen involvement in a community is a decreasing function of the size of a community, then the cost per unit of organization will be marginally increasing.

The combined function of Proposition II, A & B, is shown graphically in Figure 1, with a curve that is sloped in a marginally increasing fashion.

This argument suggests that there are diseconomies of scale in the mobilizing of coalitions such that the cost of organizing a winning coalition increases exponentially as a function of community size. Thus mobilizational costs constitute a barrier that discourages the formation of political coalitions and inhibits the expansion of such coalitions once they form.[21]

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[17] For some empirical evidence to support this point for these cities, see Gordon S. Black, "A Theory of Political Ambition: Career Choices and the Role of Structural Incentives," *American Political Science Review*, 66 (March, 1972), 147–148.

[18] Coleman describes the process of searching for allies in some detail; pp. 5–9.

[19] William H. Riker and Peter C. Ordeshook, "A Theory of the Calculus of Voting," *American Political Science Review*, 62 (March, 1968), 25–42.

[20] The hypothesis about "marginal increases" in costs is supported by the data from these cities which show that there is a dramatic increase in costs as one moves from the smaller cities to the larger cities, an increase which is not a simple linear function of city size.

The Rewards and Risks of Community Conflict. The number of minorities may also be related to the kinds of *risks* that different groups face when they seek to achieve their goals through the political process. Specifically, it seems likely that a group's response to a potential conflict is a function of two factors (in addition to the cost of mobilization): (1) the net difference between the value of "winning" and the cost of "losing" for the group (which will be called the differential value of the outcome); and (2) the probability that the participation of the group will alter the likelihood that the group will obtain its preferred outcomes. If the differential value for the group is small or if the group's participation is of little consequence, the group will tend to sit on the sidelines and ignore whatever contest exists. Conversely, one should expect conflict to intensify when groups perceive that political outcomes ininvolve large scale value differences and when groups perceive that by participating they can affect the likelihood that their values will win out in the conflict. Both these variables tend to be related to the number of groups in a community and correspondingly to the size of a community.

Groups, like individuals, are likely to feel that they have more opportunity to affect political outcomes when they face few competitors rather than many. Assuming that groups are of relatively equal size, the importance of any given group declines as the number of groups increases.[22] Where many groups are present, as in almost every large city, one is likely to find that political contests involve both a smaller proportion of the total group life in the community (even though many groups may be involved) and correspondingly a smaller proportion of the total population than in the smaller communities.

At the same time, the value of the "stakes" of a political contest is also likely to rest partially on the number of groups around which the contest takes place. Stakes are most "valuable" when two groups, of relatively equal size, confront each other over a wide range of issues about which they are in bitter disagreement. When this occurs, there is little room for compromise because the groups lack a common ground on which to negotiate their positions. Such groups will tend to fight it out on a winner-take-all basis.

A second factor is equally important, however. If a third group is introduced into this situation, a different pattern of behavior is likely to emerge. For one thing, the third group will tend to have somewhat different preferences from either of the two groups, and this difference sets the stage for a bargaining process that centers around the competition of each of the groups for a coalition partner. It also establishes a basis for a bilateral set of agreements that modifies the more extreme demands made by either of the two original groups. The result is compromise, and compromise tends to reduce both what the groups might expect to gain from the conflict and what they might expect to lose. Each of the coalition partners must modify at least some of his demands in order to forge a coalition with some other partner, and the resultant coalition is likely to be less divergent from the third group than was the case earlier. Each additional group increases both the opportunity for bargaining and the likelihood of compromise, so that the expected outcome for each group tends to be both potentially *less profitable* and potentially *less costly* than when the political contest was polarized. With *N* groups the situation is analogous to an electoral competition with *N* voters, and the dominant coalition is likely to be one that can appeal to groups lying near the center of the political spectrum.[23]

The implication of these arguments, an implication which is consistent with the cross-pressure theory of overlapping groups, is that the intensity of a conflict should be inversely related to the number of distinct "parties" to the conflict.[24] A large number of groups, however, will not eliminate conflict, but the addition of other groups should encourage coalition formation, bargaining, and compromise; and compromise will tend to mute the risks to the various groups. These argu-

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[21] This is a very important part of Mancur Olson's theory of collective action. Mancur Olson, Jr., *The Logic of Collective Action: Public Goods and the Theory of Goods* (Cambridge, Mass.: Harvard University Press, 1965), Schocken Books, 1968), chapter 1; see also, James M. Buchanan and Gordon Tullock, *The Calculus of Consent* (Ann Arbor: The University of Michigan Press, 1962), pp. 63–84; Anthony Downs, *An Economic Theory of Democracy* (New York: Harper and Row, 1957), chapter 14, pp. 260–276; Robert H. Salisbury, "An Exchange Theory of Interest Groups," *Midwest Journal of Political Science*, 13 (February, 1969), 1–32; Charles R. Adrian and Charles Press, "Decision Costs in Coalition Formation," *American Political Science Review*, 62 (June, 1968), 556–563.

[22] The probability of affecting the outcome is a partial function of the number of participants to the collective decision, whether those participants are individuals or groups.

[23] Downs, pp. 114–142; Otto A. Davis, Melvin J. Hinich, and Peter C. Ordeshook, "An Expository Development of a Mathematical Model of the Electoral Process," *American Political Science Review*, 64 (June, 1970), 426–449.

[24] For an excellent review of the cross pressure theory, see Verba, "Organizational Membership and Democratic Consensus," *The Journal of Politics*, Vol. 27 (August, 1965), 467–469.

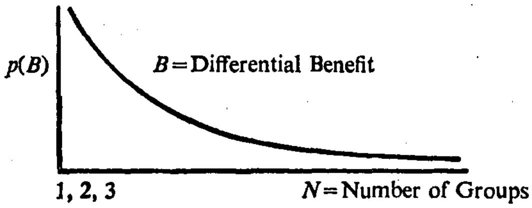

Figure 2. The Benefit from Winning as a Function of the Number of Groups

ments can also be codified in formal propositions and they are shown graphically in Figure 2.[25]

Proposition III: The expected differential value to a group in a conflict is a decreasing function of the number of groups, factions, and minorities that are found in the community.

Proposition IV: The probability that any group will have a noticeable effect on community outcomes is a decreasing function of the number of groups in a community, all other things being equal.

Community Conflict and Electoral Competition

Although some kinds of community conflict undoubtedly do not find their expression through the electoral system, it seems likely that most of the political conflict in a community eventually revolves around who will make decisions for a community. Thus, the election presents us with a useful arena for examining both the character and the effects of community conflict in different kinds of cities, and it also provides us with a set of data with which to examine the relative validity and usefulness of the two different theories outlined above.

In particular, both theories suggest a simple typology of community and electoral conflict that can serve as an organizing device for the subsequent data analysis.[26]

The Pluralistic City: Mobilizational Politics. Both the Simmel/Coser theory and the rationalistic theory of community conflict suggest that the style of political conflict in the large community will be shaped by the necessity of investing great effort in the organizing of political coalitions. Almost all large communities are sufficiently heterogeneous to insure regular electoral competition, but the difficulties of organizing coalitions will retard the development of most conflicts, keeping them at a moderate level of intensity.

In addition, both theories suggest that neither the potential benefits nor the potential losses from a conflict are likely to be particularly large in a city characterized by great diversity. Simmel and Coser argue that the cross cutting cleavages in large groups tend to reduce the intensity with which conflicts are fought out. In the rationalistic theory of conflict, the argument is that the addition of new groups will tend to reduce the differential benefit that groups can expect from a conflict; and as the difference between the value of winning and the value of losing declines, the motivation to intensify the struggle will decline as well. Both theories thus tend to have a similar view of conflict in large communities. In the rationalistic theory, however, this effect is compounded by the presumed ability of individuals and groups to discern that their participation "makes a difference," and this inability undermines the rationale for political action.

In the rationalistic theory, the costs of organizing play a central role in explaining why political actors might choose either to intensify or to avoid a conflict. Those costs include the cost of mobilizing a coalition, the cost of attracting votes, the cost of informing the public, etc., and each of these costs must be paid by the individuals en-

Chart 1

SIZE AND SOCIAL PLURALISM: STYLES OF ELECTORAL CONTESTING

Size	Social Pluralism Heterogeneous	Homogeneous
Large	*City Type:* Pluralistic *Electoral Style:* Mobilizational: Frequent competition, but Mod-intensity	(Does Not Exist)
Small	*City Type:* Fragmented *Electoral Style:* Discontented: Frequent competition, and high intensity	*City Type:* Communal *Electoral Style:* Affirmative: Infrequent competition, Intensity irrelevant

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[25] All of these arguments are cast in terms of continuous variables which are continuous in character; and although this typology is being presented for purposes of exposition, the hypotheses that have been stated must be tested independently. The purpose of the typology is to summarize what has been stated earlier; that the variables under consideration create a pattern of relationships that tend to go together, and these patterns constitute the typology.

[26] An example of the "pluralistic city" would be New Haven, Connecticut, studied by Robert A. Dahl and his associates, in *Who Governs?* (New Haven: Yale University Press, 1961). Coleman cites numerous examples of the second type of community, the "fragmented city" in his study, *Community Conflict.* The best description of the "communal city" is in Robert C. Wood, *Suburbia: Its People and Their Politics* (Boston: Houghton Mifflin Co., 1958).

gaged in the conflict. The theory by Georg Simmel and Lewis Coser, by contrast, does not discuss these costs at all. Rather, they stress the relatively neutral character of interpersonal relationships in groups involving large numbers of people, a factor which the second theory ignores almost entirely.

The Communal City: The Politics of Affirmation. At the other end of the scale of communities is the "communal" city. The communal city is small and homogeneous, and the dominant style of political activity is the "politics of affirmation."

Simmel and Coser argue that conflict is less frequent in the small group because it is suppressed by other group members, suppressed because interpersonal conflict is particularly destructive in groups whose members are on familiar terms with each other. In the rationalistic theory, however, the *incidence* of conflict in the small group is simply a function of the diversity of interests that abound in a group, and because some small communities are quite homogeneous, these communities are unlikely to demonstrate a high level of conflict over time. Elections in communal cities should be decidedly tranquil affairs, so tranquil that the character of such elections is not so much a competition for leadership as it is an *affirmation* of the existing order of things.

The Fragmented City: The Politics of Discontent. The third class of cities, those that are relatively small yet heterogeneous, are the critical cities for both theories. Both the Simmel/Coser theory and the rationalistic theory imply that a pattern of politics should be considerably different from the political behavior exhibited in the other two sets of cities. In these cities, which will be referred to as *fragmented* communities, social and political diversity produces many opportunities for conflict, but when the conflict erupts it tends to become more severe than in the other two types of cities. For this reason, I will refer to the electoral style in this kind of city as the "politics of discontent."

The test of these two theories rests on a series of implied empirical hypotheses that can be examined using the data from the cities of this study. Although the empirical indicators that are employed here are not direct measures of the central theoretical propositions outlined above, they are closely enough related to those phenomena so that they should tend to follow the pattern of relationships that comprise the two theories.

Analysis

The Predisposition to Participate. A hypothesis central to both theories is the contention that the willingness of people to participate voluntarily in community life is a function of the size of the community in which they live—the larger the community, the less inclined are citizens to participate. In the Simmel/Coser theory, this relationship is asserted without explanation, but one assumes that the involvement is due to the closeness of relationships within the community. In the rationalistic theory, the willingness to participate declines because people believe that their participation counts for less in the larger community; i.e., neither they nor others can perceive a change in the probabilities of various outcomes as a result of their participation. Although the willingness to participate is obviously affected by factors other than the individual's impact on outcomes, the expected variation of participation with city size is central to both theories.

Table 1. The Relationship Between the Size of the Population of a City and the Turnout in Municipal Elections

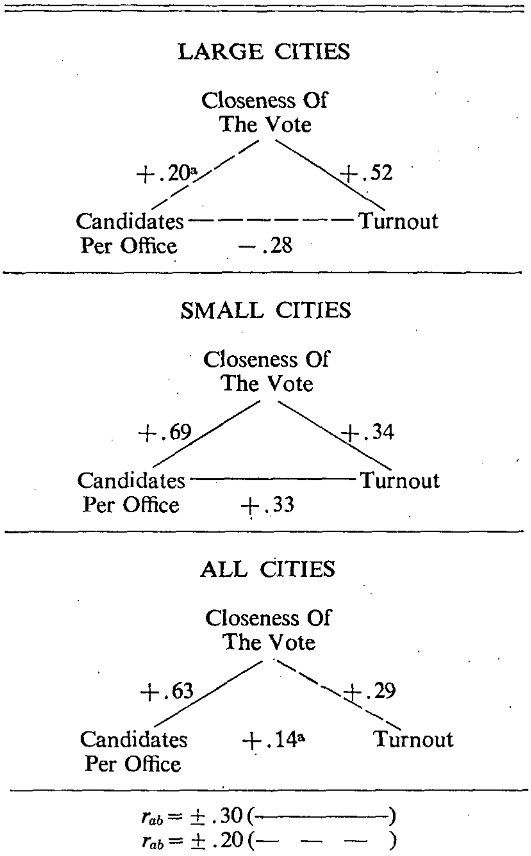
Turnout in Elections	Population Greater Than 30,000 %	 Less Than 30,000 %	
Above 50%	16	38	
Between 50% and 40%	32	34	
Below 40%	53	28	
	101%[a]	100%	
Cities	(19)	(68)	(87)

[a] Rounding Error.

The data bearing on this relationship are clear: the large cities are about half as likely to have an average turnout in elections of more than 50 per cent of the registered voters and about twice as likely to have an average turnout of less than 40 per cent. The citizens of the larger communities apparently feel less inclined to participate in elections than do the citizens in the smaller communities. This relationship would be even stronger if the five small communities with uncontested elections were removed from the sample. These five cities have the lowest turnout rates of all (all less than 15 per cent), but the lack of citizen participation in the electional process in these communities is simply due to the complete lack of choice. Where a choice is available, the turnout is considerably higher in the smaller communities. Voting, of course, is not the only form of citizen participation, but these data do not include other forms of participation.

A second issue, however, can be explored with these data. The rationalistic theory suggests explicitly that the declining participation in the

Table 2. Intercorrelations Between the Closeness of the Vote, the Candidates Per Office, and the Turnout Among 'Large' and 'Small' Cities



[a] Not significant at the .05 level; all other r's significant at or below .05 level.

larger communities is due to the feeling that people cannot affect outcomes as readily in the larger communities. If the lower rates of participation in the large communities are due to what one might call this "probability effect," the *data ought to also show that this effect is mitigated by other variables which would increase one's estimate that his vote might make a difference.* Moreover, the effect of any such variable should be more pronounced in the larger cities where the base level of participation is less than in the smaller cities.

A variable which should obviously affect such probability estimates is the closeness of the vote in elections. Riker and Ordeshook have already shown that the voter's estimate that the election will be close increases his likelihood of voting in national elections, and it seems reasonable to expect that the actual closeness of votes in a community will have a comparable effect.[27]

Table 2 shows the intercorrelations among three variables central to the electoral process: the closeness of the vote in city elections, the number of candidates seeking office, and the turnout of registered voters. Each of the variables is an average for each city based on ten years of electoral data.

In the first place, the closeness of the vote is significantly related to turnout both when all the cities are examined and when the cities are divided according to size. Moreover, when the cities are divided according to size, the relationship is substantially stronger among the large cities than among the small cities ($r^2 = .28$ to $r^2 = .12$). The pattern, therefore, is precisely what was predicted: i.e., the relationship exists among all cities but it is stronger in the large cities.

A second set of relationships lends further support to the rationalistic interpretation of the electoral process. When one compares the relationship between the number of candidates per office and turnout in both the large and the small cities, one finds a complete reversal in the relationship. Among the smaller cities, the greater diversity of choice associated with more candidates seems to be somewhat related to a higher turnout in election. Among the larger cities, however, the more candidates seeking office, the less likely are people to vote.

Although this finding might seem surprising, it is consistent with the discussion of information costs in the rationalistic theory. One of the problems for the voter is that he requires some information on which to base a voting decision, and the acquisition of information costs the voter time and effort. These costs for the average voter probably increase with the size of the city, particularly in these cities which lack a strong mayor and where public attention is spread across a number of city councilmen. In the smaller cities, where many people are likely to know some if not all of the candidates, the presence of additional candidates would do two things: it would give the voters a greater range of choice and each candidate could appeal to a slightly different sector of the public that was already well acquainted with the candidate. In the large city, by contrast, a proliferation of candidates would increase the informational problems for the voter, dissipate the clarity of a two candidate choice, and force the media to disperse its attention too broadly. The net effect would be to discourage the voter by making his choice difficult and by forcing the voter to pay more attention to the campaign than he might like. In other words, voter turnout should decline as the number of candidates increases.

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[27] One wonders what the precise relationship might be between the number of voters and the sense one has that one's own vote counts for something. Undoubtedly, the relationship reaches a saturation point such that increases in the number of voters has no appreciable effect on one's subjective feeling of the importance of one's vote. Also, it is probably true that voters systematically overestimate the importance of their vote. See Riker and Ordeshook, pp. 38–40.

The relationship between the number of candidates per office and the closeness of the vote is also consistent with this interpretation. In the large cities, the closeness of the vote is apparently only slightly affected by the number of candidates, whereas in the smaller cities the two variables are closely related. Because the measure for the closeness of the vote is a measure of dispersion (the *mean deviation* of the vote), the strong relationship among the smaller cities means that additional candidates do tend, in fact, to attract rather sizeable blocs of voters, thus reducing the dispersion of the vote among the various candidates. In the large communities, on the other hand, this logic does not appear to hold, and one reason for the lack of a relationship is that many of the candidates in a multiple candidate race cannot establish a sufficient political identity with the public because they are relatively unknown.

All three sets of relationships tend to support the participation propositions in both theories, but there one begins to see some difference in the explanatory power of the two approaches. The Simmel/Coser theoretical approach really does not imply anything directly about the interrelationships in Table 2; but the rationalistic approach clearly does provide an explanation for these findings that is consistent with the central propositions of the theory discussed earlier. One can validly argue, of course, that the implications mentioned here are not derivable in a formal, mathematical sense, but whether or not the explanation offered here is fully acceptable, the pattern of relationships strongly supports the existence of a "size effect" in electoral politics. A more thorough examination of these questions can wait until other related topics have been examined.

The Emergence of Electoral Competition. Conflict in a community may erupt over a wide range of issues, but one of the most likely places for conflict to be expressed is in the competition for political offices. Elected office holders are ultimately held responsible for the decisions from which the seeds of controversy are sown, and the primary mechanism for holding them responsible is the election. Although electoral competition and conflict are obviously not the same phenomenon, they share many similar characteristics that are pertinent to this study. For one thing, they both involve a contest in which two or more "parties" are struggling to obtain a scarce resource. Similarly, the individuals involved have an incentive to mobilize support for their goals with the understanding that such support will aid them in their struggle to obtain those goals. Finally, both conflict and competition are rooted in the differences that exist in the opinions and values of citizens, and neither could exist in a community in which all individuals felt alike about community decisions.

For these reasons, theories of community conflict should also serve to describe much of the behavior that occurs in the arena of electoral competition, and the data on elections should enable one to test many of the central propositions of theories of conflict. The application of conflict theory to elections, however, does not imply that conflict and competition are exactly alike; it only implies that the two kinds of phenomena have a great deal in common. The intent here is to explore some of these common elements.

Both theories of community conflict suggest that the central factor in the emergence of conflict is the presence of social and political diversity in a community. Diversity breeds disagreement, and disagreement breeds both conflict and electoral competition. Larger communities, because they are almost inevitably heterogeneous, should tend rather uniformly to have competitive elections, particularly in communities such as these which lack the dominance of political parties. The smaller communities, by contrast, range from being quite heterogeneous to a small number of upper class suburbs which are quite homogeneous. Among the smaller communities, the competitiveness of local elections should vary rather strongly with the extent of the social and political diversity within the community.

This in fact seems to be the case. In research reported elsewhere, several measures of social diversity were correlated with the closeness of the vote in city elections, and they were all found to be related to competition both when all the cities were considered and when only the smaller communities were examined. At the same time, social diversity was found generally unrelated to competition among the larger cities.[28]

Political Mobilization. Another of the expected differences between the large and the small cities derives from the problem of mobilizing voters and groups in elections. The last two tables have already shown that the voters of small communities seem more predisposed to participate than do the citizens in larger communities, and differences in electoral competition have been shown to have less impact on turnout in the small than in the large cities. These data support the participation hypothesis in both theories.

The problem with this inference, however, is

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[28] Gordon S. Black, "Patterns of Politics in Small Democracies: An Analysis of Community Politics" (doctoral dissertation, Stanford University, 1970), p. 123.

Table 3. The Mobilizational Effects of Size and Competition

	Large Cities		Small Cities	
	Last Election Closeness-of-Vote		Last Election Closeness-of-Vote	
	High	Low	High	Low
Personal Campaigning	%	%	%	%
Fairly Hard	82	52	51	39
Some or Not Too Much	18	48	49	61
Respondents (N)	(82)	(21) (103)	(116)	(123) (239)

	Large Cities		Small Cities	
	Last Election Closeness-of-Vote		Last Election Closeness-of-Vote	
	High	Low	High	Low
Average Group Support Per Candidate	1.38	1.24[a]	.48	.50[a]
Respondents (N)	(82)	(21) (103)	(132)	(141) (273)

	Large Cities		Small Cities	
	Hard Campaigning	Some & Little Campaigning	Hard Campaigning	Some & Little Campaigning
Average Group Support Per Candidate	1.54	1.00[b]	.65	.44[b]
Respondents (N)	(82)	(32) (114)	(126)	(166) (292)

[a] Not significant.
[b] Significant at .05 level.

that the intervening role of the candidate is ignored. The voter is not only responding to the closeness of the vote, he is also responding to the efforts of candidates to mobilize his vote; and one can assume that the mobilizational efforts of prospective office holders are partly determined by how close they expect the vote to be. Although the evidence on this variable is somewhat fragmentary, it all points to the fact that neither the certain winner nor the certain loser invests very much time and effort in his campaign.[29]

Two hypotheses here are relevant to the earlier analysis. First, the mobilizational efforts of candidates should be a function both of the size of a community and of the degree of electoral competition, large communities requiring more electoral effort than small communities. Second, one should be able to demonstrate that the efforts of the candidates actually increase the extent to which groups participate in elections. If these hypotheses can be confirmed, it will lend support to the argument that the central problem for the candidate in the large community is that of mobilizing his constituency, a problem that is not nearly as severe in smaller communities.

The first two hypotheses have been confirmed elsewhere and need not concern us here.[30] Electoral campaigning is strongly related to city size and electoral competition, and the candidates in the large communities are nearly three times as likely to campaign extensively as the candidates from the smaller, less competitive cities and about twice as likely as those candidates from the small competitive cities.

The second hypothesis suggests both that group mobilization is more extensive in the larger communities and that it is primarily a response to the campaign efforts of candidates.

The first part of the table clearly shows that the campaign efforts of candidates are structured by the size of the community and by the closeness of the vote in elections. Even the three less competitive large cities require as much effort as the more competitive small cities. The other two parts of the table demonstrate the importance of the efforts made by the candidates. Electoral competition is slightly related to an increase in group activity among the large cities, but it is quite unrelated to group activity among the smaller cities. When, however, one examines the impact of the candi-

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[29] Cf., Alexander Heard, *The Costs of Democracy* (Chapel Hill, University of North Carolina Press, 1960); Herbert E. Alexander, *Financing the 1960 Election* (Princeton, N.J.: Citizens Research Foundation); Alexander, *Financing the 1964 Election* (Princeton, N.J.: Citizen's Research Foundation, 1966; and Alexander, *Financing the 1968 Election* (Boston: Heath, 1971).

[30] Black, "A Theory of Political Ambition: Career Choices and the Role of Structural Incentives," p. 148.

Table 4. Candidate Hostility by the Closeness of the Vote, by City Size and by the Closeness of the Vote Controlling for City Size

	Population			Closeness-of-Vote		
Candidate Hostility	Large %	Small %	*N*	High %	Low %	*N*
Much	16	16		20	9	
Some	29	30		35	22	
None	55	54		44[a]	69	
Respondents	(82)	(224)	(306)	(186)	(120)	(306)

	Large Cities			Small Cities		
	Closeness of the Vote			Closeness of the Vote		
Candidate Hostility	High %	Low %	*N*	High %	Low %	*N*
Much or Some	49	—		60	32	
None	51	—		40	68	
Respondents	(69)	(13)[b]	(82)	(117)	(107)	(224)

[a] Rounding Error.

[b] This table was collapsed into two categories on the variable of interpersonal hostility because of the small number of cases in this column. Even collapsing the table, however, does not increase the number of cases in the cells sufficiently to allow an interpretation of this column of the table.

date's own efforts—efforts that are strongly influenced by electoral competition—it can be seen that the campaign efforts can substantially increase the number of groups supporting candidates. Moreover, the group activity among the large cities is considerably higher than that found in the smaller cities, and the level of that activity apparently can be sharply increased by the candidates.

These findings, coupled with those discussed earlier, tend to confirm several of the hypotheses in both theories of community conflict. Electoral competition, our surrogate for conflict, seems to arise as a response to social and political diversity; and the large cities, being more heterogeneous, are also more competitive. At the same time, citizen involvement does appear to be less in the larger cities, but electoral participation, our measure of involvement, tends to increase with the competitiveness of the milieu and the mobilizational efforts of the candidates. Among the small cities, on the other hand, citizen involvement is higher and less dependent upon the mobilizational efforts of candidates. These findings support the contention that the central campaign problem in the larger communities is the problem of mobilizing a constituency, but it does not confirm the contention that the electoral style of competition tends to be more intense among the smaller cities. For that we must turn to additional data.

Additional Evidence on the Intensity of Electoral Conflict

Electoral Conflict. One measure of the intensity of conflict in elections is the extent of hostility between the candidates. Some hostility between candidates should be expected whenever there are contested elections simply because someone has to lose. If hostility between candidates were merely a function of candidates losing, however, then we might expect that the greatest hostility would exist in the "large" cities where the candidates stand to lose the greater investment they must make in elections because of the higher costs of campaigning. If, however, the size of a community affects the conflictual level of a campaign in the manner discussed above, then we should find that intercandidate hostility is greater in the "small" and competitive cities. Table 4 shows the relationships between city size and the closeness of the vote, and the extent of the interpersonal hostility between candidates at the time of the candidates' last election.

When the variables of city size and the closeness of the vote are employed independently, only the closeness of the vote is related to the level of interpersonal hostility among candidates. When city size is employed as a control variable, the predicted pattern emerges. The councilmen from the "small" and competitive cities are most likely to indicate that the hostility existed between the candidates, while the councilmen from the "small" cities with less competitive elections are least likely to indicate that such hostility existed.

A more direct measure of what I have termed the "politics of discontent" is the perception of the extent to which incumbents are defeated in a city. Such data are, of course, subject to a high

Table 5. Frequency Incumbents Defeated by City Size, by Closeness of the Vote to the Closeness of the Vote Controlling for City Size

	Population			Closeness of the Vote		
Frequency Incumbents Defeated	Large %	Small %		High %	Low %	
Sometimes	37	35		44	25	
Seldom	63	65		56	75	
Respondents (*N*)	(99)	(232)	(331)	(190)	(141)	(331)

	Large Cities			Small Cities		
	Closeness of the Vote			Closeness of the Vote		
Frequency Incumbents Defeated	High %	Low %		High %	Low %	
Sometimes	39	30		48	24	
Seldom	61	70		52	76	
Respondents (*N*)	(79)	(20)	(99)	(111)	(121)	(232)

degree of judgment error; but this analysis will be supported with the actual incidence of incumbent defeat in a moment.

In Table 5 both city size and the closeness of the vote have been correlated with the councilmen's perceptions of how frequently incumbents are defeated in their cities; and then size is employed as a control variable while closeness of the vote is correlated with the perception of incumbent defeat. Again, as before, city size is not related to the perception of incumbent defeat, while the closeness of the vote does show a relationship with that variable. But, as before, the predicted pattern emerges when city size is employed as a control variable. The "large" and competitive cities tend to fall between both sets of "small" cities, and 39 per cent of the councilmen in those cities indicate the incumbents are sometimes defeated in their cities as compared to 48 per cent of the councilmen in the "small" cities with closely contested elections and 24 per cent of the councilmen in the "small" but less competitive cities.

Both of the variables employed above seem to suggest that a qualitative difference does exist in at least some aspects of the expressive style of electoral politics in the small cities. Two variables, however, are not enough to establish that such a difference in style exists, even though the relationships are in the predicted directions.

A third indicator of differences in the expressive style of electoral competition is the extent to which councilmen disliked some group during the campaign. Intergroup hostility is a phenomenon often discussed by students of intergroup relations, including Simmel and Coser, and such hostility is usually expressed as some function of intergroup conflict. Interpreting this relationship, however, is somewhat more difficult than with the previous two variables, because the relationship is less obvious.

We know from Table 3 that there are more than twice as many groups active in elections in the "large" cities as in the "small" cities; the average number of groups named per councilman in the "large" cities is 2.73 times as great as that average for the councilmen in the "small" cities. That table also shows that the average number of groups per councilman is not associated with the closeness of the vote in the "small" cities and only slightly related to the closeness of the vote in the "large" cities.

In order to determine whether the disliking of groups is proportionally greater in the "small" cities with closely contested elections, we need to examine three different hypotheses. One hypothesis is that the incidence of disliking groups is *strictly* a function of the number of groups active in elections; the more groups, the more likely that councilmen will indicate that they particularly disliked a group. In this case, the proportion of councilmen indicating that they disliked groups should be related to the size of the communities from which they come, and this relationship should follow roughly the same pattern as that found in Table 3. A second hypothesis is that disliking groups is *strictly* a function of the closeness with which elections are contested, and the increase in the number of councilmen indicating an especially disliked group *that is a function of competition* should be about the same among both the "large" and the "small" communities.

The third hypothesis, the one of importance for our analysis, is that there exists a special "size effect" among the "small" communities—a *stronger* relationship between the closeness of the vote and the disliking of groups among the

Table 6. Groups Disliked by City Size, by the Closeness of the Vote, and by the Closeness of the Vote Controlling City Size

	Population			Closeness of the Vote		
Groups Disliked	Large %	Small %		High %	Low %	
Yes	47	24		39	0	
No	53	76		61	80	
Respondents (*N*)	(102)	(246)	(348)	(200)	(118)	(348)

	Large Cities Closeness of the Vote			Small Cities Closeness of the Vote		
Groups Disliked	High %	Low %		High %	Low %	
Yes	49	40		31	17	
No	51	60		69	83	
Respondents (*N*)	(82)	(20)	(102)	(118)	(128)	(246)

"small" communities than among the "large" communities. In Table 6, the councilman's dislike of groups is correlated both with the size of his city and with the closeness of the vote in city elections; and in the latter part of the table, the closeness of the vote is correlated with the disliking of a group, again controlling for city size.

In the first part of the table, both city size and the closeness of the vote in the councilman's election are related to the proportion of councilmen indicating that they disliked some group.

By employing city size as a control variable, it is possible to examine the relative validity of the three hypotheses. Compared to the "large" cities, the "small" cities in the second part of the table show a much greater increase in the proportion of councilmen who disliked a particular group that is associated with the closeness of the vote in city elections, *even though from Table 3 we know that there is no increase in the average number of groups present when one moves from the "small" cities with less closely contested elections to those "small" cities that do have closely contested elections.* Among the "large" cities, however, there is only a 9 per cent increase in the proportion of councilmen indicating that they disliked a particular group when one moves from less competitive to the more competitive "large" cities. This increase is proportionally about the same as the increase in group support across the two sets of cities, as reported in Table 3.

Although this table tends to support the first hypothesis, i.e., that the disliking of groups is partly a function of the number of groups active in elections, it also suggests that there is a "size effect" operating among the "small" cities. The second hypothesis that the disliking of groups is strictly a function of electoral competition is not confirmed because the relationship between the closeness of the vote and the disliking of groups is much weaker among the "large" cities than among the "small" cities.

Thus, among the "small" cities, the incidence of group dislike appears to be accentuated by the "smallness" of the communities. Community conflict, translated into the electoral arena through electoral competition, appears to engender animosity towards the groups that participate in elections in the "small" cities, at least a higher incidence of animosity than one would expect simply on the basis of the number of groups present.

These three tables all tend to support the contention that elections in the "small" and competitive communities provide an avenue through which discontent is expressed in communities. The data for these tables, however, were based on interviews with the councilmen, and we need not rely exclusively on their judgment about the quality of the electoral process in their communities. The following tables are based on electoral statistics gathered in each city, and they reflect the actual extent to which councilmen seek re-election and the actual frequency with which incumbents are defeated in these cities.

Incumbents Seeking Re-election. Up to now I have suggested that among the "small" communities a "size effect" operates to produce an expressive style of politics that is characterized by high intensity and widespread discontent. At the same time, however, a second "size effect" is associated with the larger communities and is related to the mobilizational requirements of the larger communities. The original evidence that such an effect might occur was the finding discussed earlier

Table 7. Incumbents Seeking Re-election by City Size, and by Closeness of the Vote

Percentage of Incumbents Seeking Re-election[a]	Population Large %	 Small %	
70% or Greater	63	43[b]	
Less than 70%	37	57	
Number of Cities (*N*)	(19)	(69)	(88)

Percentage of Incumbents Seeking Re-election	Average Closeness of the Vote High %	 Low %	
70% or Greater	45	47[c]	
Less than 70%	55	53	
Number of Cities (*N*)	(49)	(34)	(83)

[a] These figures are calculated from election materials obtained from the various cities; in most cases representing the average from the five previous elections. The calculation for this average is quite simple: Total number of incumbents seeking re-election divided by the total number of incumbents.

[b] Significant at .05 level.

[c] Not significant.

that the closeness of the vote was highly associated with increases in turnout and group participation in the larger communities whereas these same relationships were much weaker in the smaller communities. This suggested that electoral competition might be strongly related to the electoral costs facing an incumbent in the "large" communities but substantially less related to those costs in the "small" communities.

At the same time, however, incumbents in the "large" communities have already made a rather sizeable investment in obtaining the offices they hold. They got to be incumbents by paying a higher initial electoral cost than the incumbents in the smaller communities, and for this reason they are more reluctant to retire from office.

These two factors might be expected to operate in the following manner. In general, incumbents from the "large" communities should be more likely to run for re-election than incumbents from the "small" communities because their original investment in obtaining their office was greater than was the case for incumbents in the "small" communities. At the same time, however, when faced with the proportionally greater costs of running for re-election in a highly competitive (and risky) "large" city, they would be less willing to pay the cost of winning re-election. If this is the case, we should expect to find that city size is positively related to the tendency of incumbents to seek re-elections but that electoral competition is negatively associated with incumbents seeking re-election in the "large" communities.

Among the "small" communities, on the other hand, the costs of seeking re-election are not closely associated with the degree of electoral competition, and consequently electoral competition should not affect the tendency of incumbents to seek re-election among these cities. In Table 7 city size and the closeness of the vote in city elections are correlated with the percentage of incumbents who seek re-election in these cities.

One can see from Table 7 that city size is somewhat related to the tendency of incumbents to seek re-election, and, as expected, incumbents tend to seek re-election somewhat more in the "large" cities than in the "small" cities. The measure of the closeness of the vote, however, is unrelated to the tendency of incumbents to seek re-election, but the reason for this will be clear in Table 8.

In Table 8 the variable of city size is employed as a control variable, and, among the "small" cities, the closeness of the vote is not related to any decline in the percentage of incumbents seeking reelection. Because the number of cities in one column are so few, it isn't possible to interpret the relationship between the closeness of the vote and incumbents seeking re-election among the "large" cities.

When the dividing point for the closeness of the vote variable is lowered from a mean deviation of .13 to one of .11, however, the predicted relationship emerges and it is very strong. Almost all of the "large" cities with closely contested elections in Table 9 have an average of less than 70 per cent of the incumbents seeking re-election

Table 8. Incumbents Seeking Re-Election by Closeness of the Vote Controlling for City Size

Percentage of Incumbents seeking Re-election	Large Cities Average Closeness of the vote High %	 Low %	 *N*	Small Cities Average Closeness of the Vote High %	 Low %	 *N*
70% or Greater	50	—		43	42	
Less than 70%	50	—		57	58	
Number of Cities	(14)	(3)	(17)	(35)	(31)	(66)

(there is one deviant case), whereas all of the cities with less closely contested elections have an average of more than 70 per cent of their incumbents seeking re-election. Thus the closeness of the vote, as predicted, appears to be strongly related to the tendency of incumbents not to seek re-election among the "large" cities, but the closeness of the vote is *not* related to incumbents seeking re-election in the "small" cities. This finding seems to confirm the argument made above that competition increases the mobilizational requirements of candidates in the "large" communities and thereby increases the costs of seeking re-election in these cities. This increase in costs induces incumbents to decline the opportunity to seek an additional term. At the same time, however, the lack of a relationship between the closeness of the vote and the tendency to seek re-election in the "small" cities may occur because competition does not significantly increase the costs of running for office in these cities.

Incumbent Winning Re-election. The argument concerning the defeat of incumbents has been made earlier. The variable of city size should be unrelated to the extent to which incumbents are defeated when they seek reelection while the closeness of the vote should be related to the incidence of incumbent defeat in cities. The results from the interview data follow this pattern and the same pattern should occur here.

The critical hypotheses as discussed earlier are: (1) the closeness of the vote should be strongly related to the incidence of incumbent defeat among the "small" cities but not in the "large" cities, and (2) the incidence of incumbent defeat in the "large" cities should be higher than among the "small" cities with less contested elections and lower than among the "small" cities with closely contested elections.

The relationships in both parts of Table 10 are the same as those we examined in Table 5. City size appears to be unrelated to the incumbent win ratios for the cities, while the closeness of the vote is negatively related to those ratios. Table 5 indicated that councilmen from the cities with closely contested elections were more likely to perceive that incumbents were often defeated in their cities than was the case for the less competitive cities, and indeed the actual incumbent win ratios are lower in the cities with closely contested elections. The critical hypothesis, however, is that the "small" cities with closely contested elections should have, on the whole, lower incumbent win ratios than any other set of cities. In Table 11 the closeness of the vote is correlated with the incumbent win ratios for the cities while controlling for the size of the cities.

These tables give evidence on several points. In the first place, the "large" cities with closely contested elections have a distribution of incumbent win ratios that falls exactly halfway between the two sets of "small" cities. As was predicted, the "small" and competitive cities tend to have, on the whole, the lowest incumbent win ratios (i.e., incumbents are defeated more often in these cities) while the "small" cities with less closely contested elections tend to have the highest incumbent win ratios (i.e., incumbents are defeated least often in these cities).

In addition to these findings the closeness of the election in the "small" cities appears to be strongly related to the frequency of incumbent victories in

Table 9. Incumbents Seeking Re-election by Closeness of the Vote in "Large" Cities Only[a]

Percentage of Incumbents Seeking Reelection	Large Cities Average Closeness of the Vote High %	 Low %	
70% or Greater	13	100	
Less than 70%	87	0	
Number of Cities (*N*)	(8)	(9)	(17)

[a] In order to show the relationship for the large cities, it was necessary to reduce the cutting point for the measure of the closeness of the vote from a mean deviation of .13 to a mean deviation of .11.

Table 10. Relationship of Incumbent Win Ratio to City Size and to Closeness of the Vote

Incumbent Win Ratio[a]	Population Large %	 Small %	
Above 90%	29	26	
70% thru 90%	41	47	
Below 70%	29[b]	26[b]	
Number of cities	(69)	(19)	(87)
Incumbent Win Ratio	**Average Closeness of the Vote High %**	** Low %**	
Above 90%	17	41	
70% thru 90%	48	35	
Below 70%	35	24	
Number of Cities	(48)	(34)	(82)

[a] This index is constructed by dividing the total number of incumbents who win re-election in a city by the total number of incumbents who seek re-election.

[b] Rounding Error.

Table 11. Incumbent Win Ratio by Closeness of the Vote Controlling for City Size

	Large Cities			Small Cities		
	Average Closeness of the Vote			Average Closeness of the Vote		
Incumbent Win Ratio	High %	Low %		High %	Low %	
Above 90%	21	—		15	45	
70% thru 90%	57	—		44	36	
Below 70%	21[a]	—		41	19	
Number of Cities	(14)	(13)	(17)	(34)	(31)	(65)

	Large Cities		
	Average Closeness of the Vote[b]		
	High %	Low %	N
Above 80%	38	44	
Below 80%	62	56	
Number of Cities	(8)	(9)	(17)

[a] Rounding Error.

[b] This part of the table employs the same measure of the average closeness of the vote as Table 9; the cutting point is a mean deviation of .11 as opposed to the figure of .13 used above. The number of categories of the dependent variable, the incumbent win ratios, has been reduced because of the limited number of cases in this table.

these cities. In fact, the relationship between the closeness of elections and the incumbent win ratios is stronger among the "small" cities than among the whole set of cities.

As in a previous table it is necessary to lower the cutting point for the closeness of the vote when examining the "large" cities because there are too few cases in one column of the table. When that change is made, the table that results suggests that the closeness of the vote is unrelated to the incumbent win ratios in the "large" cities. The perceptual data in Table 5 suggested that the closeness of the vote was largely unassociated with the councilmen's perception of incumbent defeat, and that finding is now supported with aggregate electoral statistics from the cities.

The parallel results from two different data sources is striking and suggest that the character of electoral competition is quite different in the "small" cities. Whereas in the "large" cities electoral competition appears to deter many incumbents from seeking re-election, electoral competition in the "small" cities appears to be associated with incumbents losing their bids for re-election. At the same time, electoral competition is largely unrelated to the incidence of incumbents winning in the "large" cities and unrelated to the incumbents' decision to seek re-election in the "small" cities. Thus, electoral competition appears to have a quite different character in the two sets of cities; in the "large" cities it forces extensive political mobilization while in the "small" cities it is related to the expression of discontent with incumbent decision-makers. If these patterns are really divergent in character, that is to say, if there are really *two* separate effects on electoral politics caused by community size, then we should find that the incumbent win ratios for the cities are unrelated to the tendency of incumbents to seek re-election. Table 12 shows the relationship between these two measures.

In Table 12, the tendency of incumbents to seek re-election is only very slightly related to whether or not incumbents are defeated in those cities. Where incumbents seek re-election, they are both slightly more likely to win in some cities *and* slightly less likely to win in other cities. The relationship is not strong enough to warrant any speculation.

Conclusions

This analysis has explored two different, and competing, theories concerning the patterns of politics that one should expect in cities of varying sizes. Both the Simmel/Coser theory and the rationalistic theory suggest that conflict should become more intense in heterogeneous small communities than in larger cities; and the data presented above support that assertion. In particular, the greater frequency of incumbents' losses in such cities, coupled with the high levels of electoral hostility, indicates that elections serve to express discontent with incumbent decision-makers, a discontent which is less visible both in the larger communities and in the more homogeneous smaller communities. Thus, in the most general sense, the data support the central conclusions of both approaches to the study of community conflict.

That question that arises immediately, of course, is how we can choose between two theories, based on somewhat different internal constructs, which account for the same phenomena? One point of difference between the two theories arises from the *criterion of generality;* or which theory accounts for the most findings. On this score, the rationalistic theory accounts for more of the findings presented in this analysis. In particular, the rationalistic theory suggests explanations for incumbents not seeking re-election, for the relationships between the number of candidates and electoral turnout, and for several other factors; and these cannot easily be subsumed within the Simmel/Coser approach. The concepts associated with interpersonal relationships, so central to Simmel and Coser, cannot logically be extended to cover these additional phenomena. The rationalistic approach, using concepts of information costs and mobilizational costs, does permit an extension of the theory to cover these phenomena.

A second major point of difference is the precision with which the central terms of the two theories can be expressed. The Simmel/Coser approach rests on assumed relationships between group size and the interpersonal relations with the group. Such relations are subjective, and they are affected by such factors as the age of the group, the intensity of group interaction, the nature of the interaction, etc.—all of which modify the central propositions in their theory. The rationalistic theory, while resting on propositions which cannot be easily examined directly, has a degree of specificity that the Simmel/Coser theory cannot match. While the concept of "cost" is difficult to measure precisely, its theoretical and empirical meaning is both simpler and clearer than any of the terms normally employed to describe the difference between the primary group and the secondary association.

A third point of difference is the manner in which inference is derived in the two theories. With Simmel and Coser, conflict is the product of a clash of interests and personalities in a relationship which is "close." The conflict intensifies because initial conflicts are suppressed, for such conflicts may threaten the relationship (or the relationships within the group as a whole.) The problem is that the essential assumptions about the behavior of actors remain unexpressed, and it is not clear whether conflict intensification is a function of "suppression," "interpersonal closeness," or "threat." In the rationalistic theory, by contrast, the empirical implications are directly inferred from a set of relationships based on an underlying set of assumptions concerning rational behavior. The patterns emerge because the relationships described covary with city size. Thus, it does partially establish the conditions under which severe conflicts can arise.

Table 12. Incumbents Seeking Re-Election by Incumbent Win Ratios

	Incumbents Seeking Re-election		
	70% or Greater %	Less than 70% %	
Above 90%	33	24	
70% thru 90%	36	49	
Below 70%	31	27	
Number of Cities	(42)	(45)	(87)

Both theories, however, accurately suggest that the *size* of a community dramatically affects the character of the conflict, competition, and contention that occurs within the community. The popular view of small communities has always been that they provide a haven from which the weary can escape from the conflicts of larger communities. To some extent, this view is true, but it is only true for those small communities which are so homogeneous that controversies cannot erupt into full-scale community warfare. If one looks at the community conflicts of the past two decades, many of the most bitter conflicts involving fluoridation, tainted books in libraries, leftist teachers, and civil rights tended to occur in small towns, not the large cities.

There is now a movement afoot to decentralize our cities, to produce smaller units of government that maximize participation. The implication of this paper is that smaller units *will* encourage participation, but the result of that participation may be severe conflict. While I personally approve of such attempts for other reasons, there are dangers involved and these dangers are largely wrapped in ignorance. These data and the analysis demonstrate that the virtues of "smallness" may be greatly overstated, particularly because the proponents of "smallness" usually assume that "smallness" implies homogeneity.

COMMUNICATIONS

To the Editor:

Any analysis of a system, whether it is a city real estate market, a valley's hydrological system before and after a dam or drainage system is installed, the ecological system of a dune area, the traffic system of a city, is a theory about the world. One reason to call such theories "systems analyses" rather than, respectively, urban land economics, hydraulic engineering, ecology, or traffic engineering, is because they are fraudulent as scientific theories, and could not stand up to scientific inspection and debate among professionals. If a theory of biological balances in the dunes can be inspected by other systems theorists, instead of by people who know something of entomology, hydrology, microclimatology, the effects of different plant covers on the evaporation rate, and so on, then the systems analyst can get away with being an expert in a subject of which he knows nothing. My purpose here is to suggest some symptoms in the work of a systems analyst which very often indicate such scientific fraud, to help the practical bureaucrat determine for himself whether he is being counseled by an astrologer or by a sage. He may then choose to follow astrological advice, but at least he need not think astrology a science.

(1) References to Scientific Disputes on Sizes of Parameters. In no system which is too complicated for intuitive comprehension are scientists in agreement about the size, and range of variation in size, of crucial causal parameters in the system. If the report on a systems analysis finds no such controversy, it is usually fraudulent. If its analysis of this controversy does not fairly represent both sides of the controversy, and convey to the user what is at stake in the controversy, it is probably misrepresenting the degree of scientific validity it has.

(2) Reports on Data Sources. Many data used in practical analysis are of low quality. Even cost accounts of routine repetitive mechanical production processes are well known, to accountants who work with them, to have serious sources of error. More complicated and erratic systems with less professional attention than cost accounting systems have correspondingly worse data. If the main sources of data are not reported, and the probable sources of error in the data are not analyzed in technical notes, the analysis is probably scientifically fraudulent.

(3) Sensitivity Analysis. Since almost all systems analyses are partly made up of guesses or of choices of estimates that are in controversy, a range of parameter values are possible. Because one generally does not use systems analyses if he understands the whole affair already, the consequences of such possible errors are not obvious. If the systems analyst has not conducted studies of how sensitive his recommendations are (that is, how much the crucial results would vary) to errors in various assumed parameters, it is probably scientifically fraudulent.

(4) Clarity of Definition and Exposition. The assumption of the scientific community is that other experts can read an account of an investigation or "analysis," and tell exactly what was done and on what reasoning processes the argument depends. It is characteristic of scientific discourse, therefore, that the closer one comes to the specific details, the clearer everything becomes. It is the overarching theory that is problematic, not whether parameter *p* was estimated by regression analysis of census table 3.14-P 31. It is characteristic of scientific fraud that the opposite is the case, that the overarching theory has a specious concreteness and persuasiveness, but exactly what was done at specific steps in the argument disappears into the fog. Some reasonably good indicators of such specificity of detail are:

a) A conveniently organized list of variables in the model, with specific definitions.

b) An appendix describing estimation procedures and probable errors for the crucial equations of the system.

c) A conveniently organized presentation of the notation used, both in the mathematical presentation of the theory (if there is no mathematical presentation of a mathematical model, and all systems analyses are mathematical theories, then of course it cannot be regarded as serious) and in the program.

d) A documentation of the computer programs used in the analysis, sufficiently detailed so that an experienced programmer can find out for you exactly how a particular part of the process was calculated.

e) The use of clear language, and a recognition that the report is directed to nonexperts. This is actually a processual matter, for all experts talk in a jargon that must be translated, and do not realize they are doing it. But an expert can define his terms or tell you where to get a reference where they are defined. A fraud usually defines one abstract and woolly term by another equally abstract and woolly, so that one cannot even tell *what* he is reasoning about, let alone *how* he is reasoning about it. One must expect real experts to be impatient and abrupt with nonexperts. But they got to be experts by being nonexperts who had experts explain things to them, and so ultimately can be driven to be specific. If one feels on examining a purported expert that the more one asks what the expert is talking about, the woollier it becomes, he is probably a fraud. One experienced in the frauds of a particular field in a particular culture will become attuned to clichés which indicate fraudulent and unconsidered reasoning. For instance, in the United States, in this particular field of systems analysis, a man who is not going to give you an analysis of anything in particular will almost always, at some point or other, urge upon you an "open system" rather than a "closed system" point of view. Unfortunately the dull clink of such linguistic coins that marks a counterfeit has to be learned for each of the linguistic currencies of each field, and I suppose in each language. All I can suggest in this regard is that you make your own judgments of which systems analysts are fake and which are not, and keep track of how the fakes talk so you can tell more quickly the next time.

(5) Computer Mysticism. One of the favorite clichés in the computer business is that a computer can only do what you tell it to. While I do not think this is exactly true, it is true for the kind of work systems analysts do. What this means in turn is that anything *intelligent* that the systems analyst has had the computer do was understandable *in the analyst's mind*. This means that *if* the analyst has understood the structure of the problem and the structure of the method of solution, then it is simple enough for a mere human mind to understand. For example, the mathematicians that design the hours-long huge computations that estimate the orbits of satellites can describe to anyone with elementary differential equations what they are up to, and the general structure of the method of computation. Whenever somebody gives you a lecture on how the computer has enormously increased our capacity to analyze complex systems (a true statement, as witness the orbit computations) without being able to describe to you what he proposes to have the computer do, it is suspicious. That is, what the orbit computation does is to approach a known scientific goal with known means, but means requiring tremendous capacity.

What many systems analysts do is to store one guess after another about how things might work in a computer, connect them together by further guesses about how the system might work, let it run for enough hours to be impressively costly, and call wherever they have happened to get to a scientific goal. One of the symptoms of this procedure is that they can't quite explain what the computer is doing, so it seems that the computer is the magical connection between their disorganized ideas to start with and the remarkable clarity of their conclusions. It is true that whatever magic was done was done by the computer, rather than by the mind of the analyst.

As a practical matter, if an overall summary in reasonably sensible form about what, generally speaking, the computations are for, what goal is being approached by what means in the computations, is not provided, but instead the computer is represented as being able to combine things too complex for us to understand, the combination is likely to be scientifically fraudulent.

(6) Objective Referees. It is characteristic of scientific work that a judgment on the merits of a scientific theory or investigation (and I repeat that a systems analysis is nothing other than a scientific theory of a particular system) ought to be made by experts who have no stake in the outcome. Five out of six of the papers submitted to the *American Sociological Review* are rejected as of not enough scientific value to be worth publishing. If the ratio of work worth reading is as high as one-sixth in systems analysis, I would be surprised. The way we protect ourselves against fraud in a science, then, is to systematically provide for professional level critiques by independent disinterested authorities. I would say that an ordinary science involved in reasonably complicated systems employs about a sixth of its research resources in such independent professional review. As a rule of thumb, then, if less than about 10 per cent of the budget of a system analysis is devoted to independent professional review (with the result of the review submitted to the client rather than the analyst), it is probably fraudulent.

The above six criteria are, I believe, hardly ever met by reports that call themselves systems analysis. I think they are routinely met by scientific papers in the fields that analyze systems: ecology, sociology, economics, geography, metereology, and so on. There is no inherent reason that government officials should not get scientifically valid analysis of policy alternatives. But a man does not become a scientist serving policy makers by proclaiming that he is one. He would become a systems analyst for policy purposes by following well established standards for how to analyze systems, established in the scientific disciplines that know how to do it.

Why then is fraudulent science so prevalent in the field? I believe that three factors explain the prevalence of fraud.

The first, and most important, is that true science is expensive, fraudulent science is cheap. A policy maker with a million-dollar decision in hand cannot accept the answer that it will cost him three million dollars to find the right answer. But if someone will fake an answer, and spend $100,000 painting the cardboard and chicken wire to look like science, the policy maker can afford it. The second factor is the analyst's self-deception, self-deception institutionalized in the scientific community. It is so much fun to play with computers that it must be valuable. It would be so nice to have a scheme of analysis that makes us all geographers as well as economists as well as sociologists as well as traffic engineers, that general systems theory must be possible. And the gilded coin of general systems theory is so pretty that maybe we can ignore the dull clink of counterfeit.

The third reason is internal to public bureaucracy. Consider the social function of an astrologer. A king gets worried that he might not be doing the right thing in a certain problem area. It is time to engage in search behavior for a new policy. But we are faced with a traditional royal bureaucracy, knowing exactly how to do things according to royal edict, in triplicate, with seven seals. A function of saying, more or less at random, that things would be better if they were changed, is a valuable social function. An astrologer says, more or less at random, that policy ought to be changed. An intelligent king uses this random input in strategic places. An innovator inside an education bureaucracy, for example, who wants to shake things up a bit may not much care whether the man from outside on whom he calls says "Saturn in Pisces" or "student in a general system," as long as the bureaucrat gets from it some legitimation for change. Something that poses as science is particularly good for legitimating change. One can clearly understand why many successful kings consulted astrologers, rather than setting up research projects to study the effects of the planets on the affairs of men.

In short, astrology is cheaper than astronomy, more fun than astronomy, and more useful in bureaucratic politics than astronomy. It is only in that rare case when it matters whether one has the right answer, that one would be ill-advised to consult a systems analyst or an astrologer.

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TO THE EDITOR:

There is some irony in defending the importance of studying state and local political systems in the pages of the *American Political Science Review.* But the thrust of Professor Douglas D. Rose's recent article, "National and Local Forces in State Politics," *APSR*, 67 (December, 1973), 1162–1173, is that studies of policy differences among the states are wrongheaded. Rose argues that variance among states is less than variance among individuals, and that as a consequence, studies of state politics are "severely limited in their usefulness" (p. 1173). He also offers the observation that national forces impact state politics and that as a consequence "states cannot usefully by viewed as political systems" (p. 1169).

It is, of course, a *non sequitur* to argue that because variance among states or other aggregates is less than the variance among individuals, the study of state politics is useless. Professor Rose documents (to the fourth decimal point) the fact that standard measures of variance for the whole U.S. population are greater than similar measures for policy variables for the fifty states. But this fact has no relation to the importance of developing explanatory theory about variations among state political systems. Using Rose's logic, one might conclude that the study of comparative government is "severely limited in its usefulness" because the variance among individuals in the world population is less than the variance among nations. It is not the size of the variance that makes a topic theoretically interesting; scholars study state politics because they are interested in explaining differences among states, however modest these differences may be in relation to differences among individuals.

It is also a *non sequitur* to argue that states "cannot usefully be viewed as political systems" because federal policy affects the activities of states. Even if Rose's hunch that the most influential determinants of state policies are national forces rather than characteristics of states turns out to be correct, this would not render systematic models of state and local government useless. Indeed, systematic models are probably essential in testing Rose's untested assertions on this point. And it is noteworthy that the relevant literature on the impact of federal policy in the states treats states as political systems responding to national policy.[1] Of course, most of this research has been done by economists, and perhaps we should not have expected Professor Rose or the *Review* editors to be familiar with it. But economists have managed to learn a great deal about states and municipalities, and the nation as well, by employing models whose assumptions are not fully congruent with reality.

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[1] See Seymour Sacks and Robert Harris, "The Determinants of State and Local Government Expenditures and Intergovernmental Flows of Funds," *National Tax Journal,* 17 (March, 1964), 75–85; Roy W. Bahl and Robert J. Sanders, "Determinants of Changes in State and Local Government Expenditures," *National Tax Journal,* 18 (March, 1965), 50–57; Jack W. Osman, "The Dual Impact of Federal Aid on State and Local Government Expenditures," *National Tax Journal,* 19 (December, 1966), 362–372; Louis J. James, "The Stimulation and Substitution Effects of Grants-In-Aid: A General Equilibrium Analysis," *National Tax Journal,* 26 (June, 1973), 251–265.

The political science literature on state and municipal government activity is not as advanced as the related literature in economics. Of course, if we accept Professor Rose's contention that studies of state policy differences are "trivial" (p. 1166), then this should not really concern us. But we have made considerable progress over the last decade in developing a systematic understanding of the behavior of state and local governments. That understanding was developed by employing, explicitly or implicitly, models which viewed state and local governments as political systems. The test of the utility of a model is its contribution to understanding, not its isomorphism with reality.

THOMAS R. DYE

Florida State University

TO THE EDITOR:

In disputing my conclusions, Professor Dye misrepresents my position. It is not my position that states cannot usefully be viewed as political systems simply because of federal policies. Nor is it my position that the study of state politics is useless or trivial. In attributing these arguments to me, Professor Dye so considerably jumbles my reasoning that I wish to make clear a few of my basic points.

A variety of evidence—over time, cross-section, individual, county, state, region, national, social, economic, electoral, governmental, policy and policy impact—indicates that the systematic causes of state policy occur at many levels. If we consider only state-level variables and relations, then we ignore the major causes of state policy and most of the policy differences; because this selection process controls for the major causes of policy, we cannot make inferences from what is left to what has been omitted and thus cannot make valid statements about state policy as a whole. Even if Professor Dye is only interested in the state-level causes of state policy differences, he must consider other levels to arrive at reliable conclusions because causes at other levels determine some of the state-level relations: his results will depend on how he treats national and local forces.

The problem is not that state systems models are somewhat unrealistic but that they misconstrue relations in a way which produces confusion and error. Econometric reasoning, the part of the economic literature which has proved most adaptable to political science, lays out the difficulty: strong associations dominate weak associations and produce spurious correlations, and the only way to deal with the spurious correlations is to take the strong associations into account. States are governmental units within a national society; this fact requires the study of state policy to consider non-state factors and also allows the study of states to make distinctive contributions to our understanding of politics.

DOUGLAS D. ROSE

Tulane University

TO THE EDITOR:

"Political ethos" is one of the most significant concepts to have emerged in the theoretically barren terrain of ethnic politics.[1] Until recently, the empirical basis for this concept rested solely on aggregate data—with their attendant structural and inferential limitations. Wilson and Banfield have currently sought to reinforce both the conceptual and empirical buttressing by presenting their own test of the concept against individual-level data.[2]

We contend, however, that the individual-level data analysis presented by Wilson and Banfield constitutes the strongest evidence yet accumulated for *rejecting* the concept of political ethos. This assertion stems from the discovery that the percentages on which their evidence is based are generally, although not always, calculated with the independent and dependent variables reversed. When proper distinctions are made between the independent and dependent variables and the tables are appropriately calculated, the results disconfirm the basic tenets on which the conceptual structure rests. This reconsidered evidence, coupled with the criticisms leveled in other recent studies,[3] appears to go a long way toward rejecting

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[1] The ethos theory was first articulated in Edward C. Banfield and James Q. Wilson, *City Politics* (New York: Vintage Books, 1963, reprint), esp. chaps. 3, 16. Shortly thereafter, the thesis appeared in slightly altered form: James Q. Wilson and Edward C. Banfield, "Public-Regardingness as a Value Premise in Voting Behavior," *American Political Science Review*, 58 (December, 1964), 876–887. For an early controversy over the application of the theory, *cf.* Raymond E. Wolfinger and John Osgood Field, "Political Ethos and the Structure of City Government," *ibid.*, 60 (June, 1966), 306–326; and James Q. Wilson and Edward C. Banfield, "Communications," *ibid.*, 60 (December, 1966), 989–999.

[2] James Q. Wilson and Edward C. Banfield, "Political Ethos Revisited," *American Political Science Review*, 65 (December, 1971), 1048–1062.

[3] Roger Durand's test of the concept against individual-level data has raised serious doubts about its empirical validity. "Ethnicity, 'Public-Regardingness,' and Referenda Voting," *Midwest Journal of Political Science*, 16, 2 (May, 1972), 259–268. And a cogent and well-reasoned analytical incision by Timothy Hennessy has raised serious doubts about ethos theory's conceptual validity. "Problems in Concept Formation: The Ethos 'Theory' and the Comparative Study of Urban Politics," *ibid.*, 14, 4 (November, 1970), 537–564.

Table 1. Characteristics of Voters with "Holistic" and "Localistic" Conceptions of the Local Political Process (Wilson and Banfield's Table 2)

Characteristics	Holistic (*n*-475)	Localistic (*n*-315)	Nondescript (*n*-269)
**Income			
Under $5,000	13%	26%	23%
$5,000–7,499	17	33	29
$7,500–9,999	24	17	18
$10,000 and over	40	15	25
NA	4	9	7
**Schooling			
Less than high school	27	50	37
High school	30	36	37
Some college	17	8	13
College graduate	26	6	13
NA	0	2	1
**Ethnicity			
Negro	8	17	15
Jewish	24	11	17
Italian	7	18	12
Polish	8	13	8
Yankee	10	2	3
Irish	8	15	11
Other	35	25	34
**Religion			
Protestant	27	21	27
Catholic	44	67	49
Jewish	26	11	18
Other, NA	3	1	5
Immigration generation			
Immigrant	19	27	22
First generation	45	39	45
Second generation or more	32	29	29
NA	4	5	4
Religiosity			
**Strong religious feeling	27	37	32
**Very religious upbringing	47	61	53
**Attend church weekly or oftener	47	67	45

** Percentages significant at the .005 level in the chi-square test.

the concept of political ethos as a means of explaining the impact of sociopolitical forces on the shape and substance of American political life.

As students of ethnicity our primary concern revolves about the ethnic basis of the concept of political ethos. Consequently, our attention will be directed at the distributions of ethnicity and social class and their incorporation into the conceptual structure. We will also demonstrate that the improper calculation of per cents has led Wilson and Banfield to draw erroneous conclusions.

A View from Wilson and Banfield's Individual-level Data

In their most recent article, Wilson and Banfield define each ethos in terms of three attitudinal components, and then describe the incidence of each as it appeared in a sample of voters in a large city a few years ago. One of their purposes is to demonstrate the proportion of the voters having each attitudinal component, and especially the proportion having the "right" combination of attitudes that constitutes an ethos. A second goal is to identify the attributes (especially ethnicity, religion, income, and years of schooling) of voters having the relevant attitudes or combinations of them.

In the execution of the second objective, the causal linkage is lost. It is not political cultural responses that produce sociocultural characteristics; rather, it is sociocultural characteristics which produce political cultural responses. Or, translated directly into the framework of analysis used by Wilson and Banfield: the question is not what proportion of those possessing the attribute came from various groups, but rather what proportion of those in various groups possess the attribute. They note:

> We should expect holists [a term now used to denote "public regardingness"] to be drawn mostly from the upper-middle class and localists [a term now used to denote "private regardingness"] to be drawn mostly from the lower-middle and working classes. In income and schooling, then, holists should be clustered at one end of the scale and localists at another. We should also expect localists to be disproportionately Catholic and Negro. As can be seen from Table 2, [our Table 1] these expectations are in general borne out by the data. Holists have more schooling and higher income than localists. They are five times more likely than localists to be Yankees and twice as likely to be Jewish. Localists on the other hand are twice as likely as holists to be Italian, Irish or Negro. Contrary to what we should expect differences associated with immigrant status are not statistically significant.[4]

We have no quarrel with the authors' reading of their own table, per se. But we emphatically argue that their Table 2 (our Table 1) has put the cart before the horse in reversing the temporal and

[4] Wilson and Banfield, "Political Ethos Revisited," p. 1051. The conclusion based on immigrants is drawn from the lack of statistical significance. We are compelled to call attention to this all-too-common error of using significance tests on data gathered with nonsimple random sample designs. *Cf.* Leslie Kish, *Survey Sampling* (New York: John Wiley & Sons, 1965), pp. 582–593. As for the distinction between immigrant generations, ignoring the issue of calculation, the differences seem to be substantively trivial.

causal sequences of the variables, and as it stands is substantively meaningless.

If the per cents in Table 1 are recomputed in the proper direction, giving due consideration to the causal sequence, then the conclusions reached by Wilson and Banfield, which sustain the ethos theory, are not supported, although some general trends persist. Let us view Wilson and Banfield's data as they appear when properly recalculated in Table 2.

Catholics, contrary to the implications of Wilson and Banfield's expectation and conclusion, have as high a probability of being holistic as they have of being localistic. This fact is far more meaningful than saying, as Wilson and Banfield have done, that localists are disproportionately Catholic and Negro, without even establishing an appropriate frame of comparison by controlling for the proportion of Catholics and Negroes in the population. It is true that Protestants and Jews have a greater tendency to be holistic than localistic. And, if Catholics are compared to Protestants and Jews, they have a lesser tendency to be holistic. But localism per se hardly seems to be a Catholic trait, as Wilson and Banfield imply.

When we move from religion to ethnicity, we find that the Poles and Irish, contrary to Wilson and Banfield's belief, are as likely to have holistic attitudes as localistic orientations. The remaining groups do manifest feelings congruent with the theoretical predictions (although some questions will be raised below about even this). Moreover, it is one thing to note that the Poles and Irish fall below Yankees and Jews in their manifestation of holistic attitudes. It is quite another to argue that sociocultural forces produce a distinct political cultural response when the groups in question are no more likely to manifest one type of political cultural response than the other. If localism is part of what, until the most recent depiction of the theory, has been associated with the immigrant ethos, how does one account for the fact that the immigrant generation itself is just as likely to be holistic as localistic? There is nothing in the recomputed data that suggests that localistic attitudes are a product of a sociocultural force generated by America's experience with latter-day immigrant culture, as the ethos theory would have us believe.

What is most troubling about the Wilson and Banfield analysis is to be garnered from the positive association between income and schooling on the one hand and holistic attitudes on the other. The linear relationship is now observable in the recomputed distributions, and a gamma for the two associations yields +.32 and +.49 respectively.

Table 2. Characteristics of Voters with "Holistic" and "Localistic" Conceptions of the Local Political Process (Recalculated)

Characteristic	Holistic	Localistic	Nondescript	*N*
Income				
Under $5,000	33%	38%	29%	215
$5,000–7,499	31	40	30*	263
$7,500–9,999	53	25	22	216
$10,000 and over	63	15	22	304
NA	29	42	29	66
Schooling				
Less than high school	33	41	26	386
High school	40	32	28*	355
Some college	57	18	25	141
College graduate	70	11	20*	178
NA	0	66	33	9
Ethnicity				
Negro	29	41	30	132
Jewish	58	18	24	195
Italian	27	47	26	121
Polish	38	41	22*	101
Yankee	77	10	13	62
Irish	33	41	26	115
Other	49	24	27	336
Religion				
Protestant	48	25	27	267
Catholic	38	38	24	552
Jewish	60	17	23	207
Other, NA	47	10	43	30
Immigrant Generation				
Immigrant	38	36	25*	234
First Generation	47	27	26	458
Second Generation or more	47	28	24	321
NA	10	84	6	188
Religiosity				
Strong Religious Feeling	39	35	26	331
Very Religious Upbringing	40	34	26	558
Attend Church Weekly or Oftener	40	38	22	555

* Does not sum to 100% due to rounding errors.

Consequently, it becomes apparent that any group possessing disproportionate amounts of the two attributes so positively associated with the dependent variable will show a strong holistic response. In such cases, it is not sociocultural forces but income and schooling that contribute to a political outlook that is ostensibly directed toward the interests of society as a whole. The varying distributions of such demographic characteristics between groups is well known, and some partition control procedures for these effects are performed by Wilson and Banfield in the course of their analysis. However, the partitioning is of limited value, for the definition of ethnic groups confounds class and ethnicity in the case of Yankees, and the samples of ethnic Catholics and Negroes are restricted in class terms. "Yankee" is defined as a respondent who "describes himself as a Protestant, was born in the United States with both of his parents and at least three out of four of his grandparents born in the United States, had an Anglo-Saxon last name, lived in the Beacon Hill area of Boston, and earned more than $10,000 a year."[5]

While such definition might be appropriate, it sorely confuses the analysis, for one is unable to determine if the decidedly holistic responses of the Yankees are due to an ethnic cultural tradition, to class, or to both, in some interactive setting. On a set of valued normative characteristics which are positively related to income and schooling, the juxtaposition of upper-class Protestants against a socially heterogeneous constituency of other ethnic groups appears to predetermine the results One might inquire whether such analyses unobtrusively say more about the value orientations of the researchers than about sociocultural factors and their impact on American political culture. Indeed, one might ask if we similarly restricted the sample of Poles, Italians, Irish, and Blacks and collected the White Anglo-Saxon Protestants from the varied social strata of the community, might we not have determined that the political culture associated with the impact of the WASP ethos was localistic rather than holistic?

While the authors are able to impose partition design controls for education and income for part of their data and reveal that Catholics are less holistic in attitude than Yankees or Jews, the samples of Negroes, Poles, Italians and Irish are so skewed to the bottom end of the education and income continua that there are not enough respondents for meaningful comparisons with Yankees and Jews. This further illustrates that when the Negro, Italian, Irish or Pole is compared to the Yankee or Jew, it is not simply ethnicity that is being compared but education and income as well. As to what is influencing the response on the dependent variable, one is at a loss to comprehend, for the experimental design is confounded with multiple treatments.

Community Serving vs. People Helping; Good Government vs. Benefits

Although considerations of space preclude a detailed analysis of the other two attitudinal components of the public and private regarding ethics, we might briefly note that these are also handled by Wilson and Banfield with the dependent and independent variables reversed. Recomputing the distributions for community serving vs. people helping does not markedly change the original results. There is, however, a clearer demarcation, once the variables are properly ordered, of the relationship between income and schooling on the one hand and community serving on the other. Gammas for this pair of relationships are +.35 and +.55 respectively.

Focusing on the recomputing of the percentages for good government vs. benefits, we note some important differences with the original version. The original version saw no relationship between ethnicity and attitude, except for the Jews and Italians, who were benefit minded. The recomputed version shows ethnicity is related to attitude, but in a fashion that differs from the results deduced from ethos theory. Poles tend to be incredibly good-government minded, almost as much so as Yankees. The Irish follow suit, and the Negroes are very close to them in attitude preference. Moreover, Wilson and Banfield's conclusion that the benefit minded are disproportionately Italian is untenable when the data are recalculated.

The assault on the ethnic component of the theory, already manifest in the above distributions, is reinforced by the patterns for the immigrant generation. More than 60 per cent of the immigrant generation is good government minded. Consequently, it is difficult to see how ethos theory is a depiction of the impact of an immigrant-based, sociocultural force on the political environment.

Ethos and Attitudes

The next step of Wilson and Banfield's analysis is to combine the three attitudinal components that comprise the two ethics—holistic vs. localistic, community serving vs. people helping, and good government vs. benefits—and look at the demographic distributions over the total index. We not only fault this procedure because it once again confuses the dependent and independent variables, but it also assumes that the three attitudinal components are conceptually related when only face validity has been established. In fact, the authors' correlations between holism and community serving is only +.135 and between holism and good government is a trivial +.058. This is hardly the kind of consistency that warrants an index, and argues rather substantially in favor of the conclusions presented by Hawkins and Prather. Their factor analysis demonstrated factorial separability, with private regardingness, localism, intolerance of conflict and opposition to good government loading on independent factors.[6] Rather than follow the logical conclusions of their own correlation coefficients, which mesh with Hawkins and Prather's multivariate analysis, and accede to the guidance of the data, Wilson and Banfield refuse to acknowledge that the three attitudinal components do not share the same conceptual space and, hence, do not constitute an index. Instead, they incredibly throw out 88 percent of their 1,059 respondents and look (again backwards) at the characteristics of those voters for whom there is some sharing of the conceptual space for the three attitudes.

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[5] Wilson and Banfield, "Political Ethos Revisited," p. 1050.

Of course, we are not too surprised that Table 3 (Wilson and Banfield's Table 8) demonstrates the Yankees and Jews to be disproportionately public regarding (or unitaristic as it is now called), and the immigrant Catholics and Negroes to be disproportionately private regarding (or individualistic), and the theory of political ethos is confirmed. This, however, is sheer nonsense. When Table 3 is properly recomputed (see our Table 4) it sharply underscores the notion that Yankees and Jews have a unitary ethos while the "newer races" (as the authors, taking a leaf from James Curley, refer to the immigrant Catholic groups) and Negroes have an individualistic ethos. Let us look first at the one case which is most distorted by the authors' miscalculations. This is the case of the Polish respondents. Although none of the Polish respondents demonstrated an individualistic ethos and every single Polish respondent did manifest a unitary ethos, the erroneous calculation of percentages cause the authors to conclude, "Table 8 [our Table 3] shows that the unitarists include a disproportionate number of Yankees and Jews and also of the well-schooled and well-off; it shows also that the individualists include a disproportionate number of Irish, Italian and Polish Catholics, as well as the less well-schooled and the relatively low income. . . ."[7]

Looking at the data when they are properly computed one can see that Wilson and Banfield are incorrect in their assessment not only of the Poles but of the Irish and Negroes as well. Two-thirds of the Negroes and two-thirds of the Irish demonstrate the unitary ethos, and the Jews are no more likely to be unitarists than are the Irish or Negroes.

Table 3. (Wilson and Banfield's Table 8) Characteristics of Voters Who Are "Holistic/Community-Serving/Good Government-minded" or "Localistic/People-Helping/Benefit-Minded"

Characteristics	Holistic/ Community-Serving/Good Government Minded (*n*=97)	Localistic/ People-Helping/ Benefit-Minded (*n*=26)
Income		
Under $5,000	12%	27%
$5,000–7,499	8	15
$7,500–9,999	17	19
$10,000 and over	59	31
NA	4	8
**Schooling		
Less than high school	12	27
High school	22	54
Some college	19	8
College graduate	46	8
NA	1	4
**Ethnicity		
Negro	4	8
Jewish	14	35
Italian	3	23
Polish	9	0
Yankee	24	0
Irish	8	15
Other	37	19
**Religion		
Protestant	42	15
Catholic	38	46
Jewish	18	35
Other, NA	2	4
*Immigrant generation		
Immigrant	11	27
First generation	35	54
Second generation or more	47	15
NA	6	4
Religiosity		
Strong religious feelings	24	23
Very religious upbringing	44	54
*Attend church weekly or oftener	49	54

* Percentages significant at the .05 level in the chi-square test.

** Percentages significant at the .005 level in the chi-square test.

Another crack in the theoretical structure is exposed when the distributions of immigrants are viewed. The private regarding (individualistic) ethos is supposed to be characteristic of the impact of the sociocultural forces of latter-day immigration on the political structure. But how can this be the case if 60 percent of the immigrant

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[6] Brett W. Hawkins and James E. Prather, "Measuring Components of the Ethos Theory: A First Step," *Journal of Politics*, 33, 3 (August, 1971), pp. 642–658.

[7] Wilson and Banfield, "Political Ethos Revisited," p. 1057.

Table 4. Characteristics of Voters who are "Holistic/ Community-Serving/ Good Government-Minded" or "Localistic/People-Helping/Benefit-Minded" (Recalculated)

Characteristics	Holistic/ Community-Serving/Good Government Minded ($n=97$)	Localistic/ People-Helping/ Benefit-Minded ($n=26$)	N
Income			
Under $5,000	63	37	19
$5,000–7,499	67	33	12
$7,500–9,999	77	23	22
$10,000 and over	88	12	65
NA	67	33	6
Schooling			
Less than high school	63	37	19
High school	60	40	35
Some college	90	10	20
College graduate	90	4	47
NA	50	50	2
Ethnicity			
Negro	67	33	6
Jewish	61	39	23
Italian	33	67	9
Polish	100	0	9
Yankee	100	0	23
Irish	67	33	12
Other	88	12	41
Religion			
Protestant	91	9	45
Catholic	76	25*	49
Jewish	67	33	27
Other, NA	67	33	3
Immigrant Generation			
Immigrant	61	39	18
First Generation	71	29	48
Second Generation and more	92	8	50
NA	86	14	7
Religiosity			
Strong Religious Feeling	79	21	29
Very Religious Upbringing	75	25	57
Attend Church Weekly or Oftener	77	23	62

* Does not sum to 100% due to rounding error.

generation already possess a public regarding (unitary) ethos?

The recomputed table runs strongly counter to the argument sustaining the political ethos theory; however, we must not lose sight of the criticism we leveled when we opened this section. Specifically, the amalgamation of the three items does not constitute an index, and by performing the analysis on the twelve per cent of the sample displaying a consistent pattern among the three items, the authors appear to be measuring the attitudinal structure of those respondents who have been able to form a consistent cognitive pattern. Such respondents are atypical,[8] and perhaps this accounts for the pattern which emerges within each category of Table 4 and across the entire table. Namely, public-regarding (unitaristic) responses occur disproportionately within virtually every group, and 79 percent of the respondents demonstrate this response. Consequently, Table 4 may well be saying far more about the political cultural patterns of people capable of cognitive consistency than about the relationship between sociocultural forces and political attitudes. *For it is rather intriguing that people with consistent cognitive patterns are disproportionately unitarists regardless of income, schooling, ethnicity, religion, immigrant generation or strength of religiosity.*

Conclusion

The general impression the reanalysis imparts is that the ethnic basis of political ethos is not sustained by individual-level data marshalled in its behalf by its creators. Looking at the reanalysis in the light of existent theoretical and empirical criticism, it is clear that the concept of political ethos has led to an unwarranted oversimplification of the influence of sociocultural characteristics on political behavior.[9] Indeed, it is not inappropriate to ask whether or not such basic methodological flaws as are evident in "Political Ethos Revisited" are a function of the authors' preconceived impressions of the political cultural manifestations of America's experience with unrestricted European immigration. Instead of the data guiding the analysis, it appears that the authors' theoretical impressions have guided the data.

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[8] A substantial body of literature has been developed which indicates that highly differentiated, wide-ranging, and coherent political belief systems of the type Wilson and Banfield refer to are generally restricted to the upper SES echelons of the populace. *Cf.* e.g., Philip E. Converse, "The Nature of Belief Systems in Mass Publics," in *Ideology and Discontent*, ed. David E. Apter (New York: The Free Press, 1964), pp. 206–261; Angus Campbell, Philip E. Converse, Warren E. Miller, and Donald E. Stokes, *The American Voter* (New York: John Wiley & Sons, 1960), pp. 168–265; Herbert McClosky, Paul J. Hoffman, and Rosemary O'Hara, "Issue Conflict and Consensus among Party Leaders and Followers," *American Political Science Review*, 54 (June, 1960), 406–427; and Herbert McClosky, "Consensus and Ideology in American Politics," *ibid.*, 58 (June, 1964), 361–379.

[9] *Cf.* Herbert Kaufman, "Review of *City Politics*," *American Political Science Review*, 58 (June, 1964), 422.

ABRAHAM H. MILLER
STEPHEN E. BENNETT

University of Cincinnati

TO THE EDITOR:

Like others before them, Professors Miller and Bennett have rushed in where they should fear to tread. They write, correctly, that one of our objects was to identify the attributes (ethnicity, religion, and so on) of voters having certain attitudes or combinations of them. They then find that in the "execution of this objective the causal linkage is lost": we failed to address ourselves to the proper question, namely, what proportion of those in the various attitudinal groups possess the listed attributes? By calculating percentages to sum across rather than down the tables they claim to have restored the "causal linkage" and disconfirmed our findings.

Apparently Professors Miller and Bennett either think that we asserted the existence of a causal linkage between attributes and attitudes or that whenever two variables are found to be associated there *must* be such a linkage. Whichever they think, they are mistaken. We did not assert such a relation; on the contrary, throughout the article we were at pains to speak of the variables only as "associated." Any text on statistics will explain, most probably in the introductory chapter, that association does not entail causality.

Our critics wish to impose on us the logic of causal analysis, to insist that ethnicity and other attributes be considered the independent variables, and to require that our theory be rejected if a *majority* of any given ethnic (or religious, or income, or educational) group does not display the predicted attitude. This is not our view. We think that the relationships among attributes and attitudes are so complex and our knowledge so incomplete as to make any choice of an "independent" variable arbitrary, and we reject as unscientific—indeed, as silly—the claim that the association among variables is meaningless unless a majority of respondents having a particular attribute have the expected attitudes.

Professors Miller and Bennett seem unaware that some social science is causal-analytic and some historical-descriptive. Once or twice we stepped close to the line and we may have stepped over it momentarily when we referred to a "quasi-logical" relationship among attitudes, but in all our writings we have repeatedly made it plain that our theory of the association of attitudes is historical rather than causal. We have, for example, remarked that the association of certain attitudes with certain ethnic groups appears to have weakened over time in consequence of social mobility, mass education, the decline of the reform movement, and other historical developments. This being the sort of theory that we have offered, it is entirely proper for us to ask: "What are the social characteristics of individuals who hold particular attitudes?"

If our theory is properly understood, the criticisms that Professors Miller and Bennett make of our tables are all irrelevant.

Let us, however, pursue the (incorrect) assumption that the theory is a causal-analytic one. Would it follow that, when the tables are recalculated and displayed with the percentages summing across, the data provide evidence for rejecting the theory? Professors Miller and Bennett answer yes on the grounds that in the groups which we hypothesized would display individualistic attitudes, more than half the respondents failed to do so. The speciousness of this claim is obvious. Suppose we had hypothesized that sickle-cell anemia is associated with being a Negro and, on the basis of a random sample of the whole population, had found that only 10 per cent of Negroes have the disease. Professors Miller and Bennett would say, "Sickle cell anemia per se hardly seems to be a Negro trait," but they would be wrong if our data showed the incidence to be significantly greater among Negroes than others. It is clear from our analysis (but obscured in their recalculated tables) that this is the case with the groups that we hypothesize will display individualist attitudes: they *do* display them more frequently than do the groups we hypothesize will display unitarist ones.

Take another example, perhaps closer to the collegiate heart. It can be shown that the proportion of persons saying that the penalty for using marihuana should be less severe increases with higher levels of education (Erickson and Luttbeg, *American Public Opinion*, p. 171). At no educational level, however, does a majority of respondents favor the reduction in marihuana penalties. Furthermore, there is no way of being certain which variable—education or attitude toward marihuana—is "independent." Marihuana smokers may be more likely to go to college, college students may be more likely to learn to smoke it, or college graduates may learn tolerance for marihuana though they do not use it themselves. Perhaps all three relationships hold. But whatever the relationship, surely Professors Miller and Bennett would not deny the association between education and attitudes toward marihuana smoking simply because a majority of college graduates do not favor reducing the penalties.

Professors Miller and Bennett should not be so concerned with the absolute percentages. We have never claimed that a *large* proportion—say a majority—of this or that group has this or that ethos. Instead we have said that we expect to find this or that ethos more closely associated with some groups than with others. Even when rearranged, the data support this hypothesis.

We acknowledge that we misinterpreted Table 8 with respect to Poles and Jews, but the matter is clarified in our discussion of Table 9, and the major conclusions of the article remain unaffected.

One other criticism has some—but not much—merit. In footnote 4, Professors Miller and Bennett say that our significance tests are inappropriate because our data were not gathered by means of a simple random sample. It is true that a simple random sample of homeowners was supplemented with a quota sample of certain ethnic groups. To be methodologically beyond reproach we should have weighted the supplemental sample so that the incidence of relevant characteristics would be the same in it as in the simple random sample. We doubt that our failure to do so materially affected the measures of significance, however, and, in any case, the idea of statistical significance was of small importance in our argument. Indeed, not being uncritically enamoured of tests of significance, we included them in these tables only at the insistence of this *Review*. We should add, lest some readers be unaware, that tests of significance and such measures of association as *gamma* are not affected by the direction in which a table adds to 100 or by how the table is "recomputed."

We feel obliged to call attention to the unpleasant implications of the next to last sentence in Professor Miller and Bennett's comment. If they are suggesting that our views on ethos are the product of a preconceived hostility toward European immigrants, they are wrong, and no fair reading of our works could possibly support such a view.

Edward C. Banfield
University of Pennsylvania
James Q. Wilson
Harvard University

To the Editor:

Among the many interesting points at issue in the Katz-Stokes debate on components of variance in congressional elections (*APSR*, 67 [September, 1973], 817–34), one seems susceptible of solution. I refer to the bias in Katz's estimates due to small sample size.

The Katz method is based on regression analysis. In the first step, a congressional district's Republican vote percentages in five elections are made the dependent variable, and the corresponding national vote percentages serve as the lone independent variable. The resulting R^2 in each district is then taken as an estimate of the per cent of the variance "explained" by national forces, and these are averaged to form a nationwide estimate.

Such an estimate, as Stokes points out, is biased upward. In the case where the residuals are normally distributed, A. P. Barten, "Note on Unbiased Estimation of the Squared Multiple Correlation Coefficient" (*Statistica Neerlandica* 16 [1962], 151–63), has provided an improved estimator, unbiased to order n^{-1}. The new estimator, R_1^2, is defined as:

$$R_1^2 = 1 - (1 - R^2) \cdot \left[1 + \frac{K - (1 - R^2)(1 + 2R^2)}{T}\right],$$

where R^2 is the uncorrected squared multiple correlation coefficient, T is the number of observations, and K the number of independent variables, including the constant term. This formula allows us to calculate unbiased estimates of the "variance explained" for any uncorrected estimate of R^2 obtained from a regression analysis. With $T=5$ and $K=2$, as in Katz's case, some representative values are as follows:

Uncorrected R^2	Corrected R_1^2
1.00	1.000
0.90	0.866
0.80	0.741
0.70	0.623
0.60	0.510
0.50	0.400
0.40	0.290
0.30	0.177
0.20	0.059
0.15	0.000

In the case of Katz's nationwide estimate of the national component of the variance, his uncorrected estimate is 0.546. Use of Barten's formula shows that an unbiased estimate is 0.451, meaning that, on Katz's assumptions, 45.1 per cent of the variance should be attributed to national forces. This corrected estimate falls approximately halfway between Katz's uncorrected value and Stokes's earlier estimate, which was based on different techniques and assumptions.

Actually, even 45.1 per cent is too high an estimate, since 0.546 is an *average* of uncorrected R^2's. Because Barten's formula is nonlinear in R^2, applying it to the uncorrected average will not generally yield the desired quantity, namely, the average of the *corrected* R^2's. To obtain this latter value, Barten's formula would have to be applied *before* averaging Katz's constituency-level R^2's. The result should be a few points lower, yielding an unbiased estimate of the per cent of the variance explained by national forces in Katz's model of perhaps 42 per cent or 43 per cent. By contrast, Stokes's somewhat simpler model employs the usual techniques for estimation of a variance, and his basic estimates are unbiased. (However, if serial correlation is present, as seems possible with this times-series data, both Stokes and Katz are subject to an additional upward bias.)

Of course, this correction is a technical one in what remains a fresh and significant generalization of Stokes's work. And none of Katz's substantive conclusions are affected.

Christopher H. Achen
The University of Rochester

To the Editor:

Two observations are, I think, in order with regard to Achen's suggestion. The first is that it only applies if one accepts the assumption that the elections analyzed are a random sample drawn from a large universe. If they are a "better" than random sample, or no sample at all, then the bias resulting from small sample size will be considerably smaller than the correction proposed. Nonetheless, this correction may offer an increase in comparability when data from two series of different lengths are to be analyzed.

The second observation is that even after the substantial correction which Achen suggests has been made, the national variance component which my method yields is still considerably larger than that which Stokes reports, as is only to be expected considering the differences in our models. In this respect, Achen's correction underlines the fact that, quite aside from limitations of methodology, different theories will give different results.

Richard S. Katz
Queens College,
CUNY

To the Editor:

Peter Eisinger's article, "The Conditions of Protest Behavior in American Cities" (*APSR*, 67 [March, 1973], 11–28) points out that the correlation between the number of protests and the population of cities is positive (.46). This is not surprising, since one would expect to find more protests where there are more people to protest. The dependent variable of interest would be the number of protests per capita, or perhaps the number of protesters per capita. Unfortunately, if its use yields significantly different results in the above-noted correlation, the correlations of other variables with the raw number of protests are matters of indeterminate meaning whenever these other variables are correlated with population. Quick calculation reveals that the correlation between protests per capita and city size in the cities examined in the article is *negative*—approximately −.28. This figure might be interesting and suggestive in some way to students of protest behavior and city government, but, sadly, unless the other variables examined are uncorrelated with city size, it probably indicates most of the article is invalid. I don't have the data to ascertain this with certainty, but Dr. Eisinger will probably be able to do the necessary testing rather easily. There are other matters in the article which might provoke discussion, but it is senseless to raise them in the face of this problem.

Ken Allen
University of Maryland
Baltimore County

To the Editor:

I think that the point of Ken Allen's criticism is that most of the correlations upon which I base some of my argument may reflect the effects of city size, thus making the relationships I discuss in my article spurious. By controlling for city size, however, we find that in most cases the relationships between the different independent variables and the protest incidents still hold, as the table shows. In a number of cases—notably where black protests are concerned—the relationships are stronger when size of city is controlled.

Both the zero-order and the partial correlation coefficients are small, as I recognize in my article.

Table 1. Zero-order Correlations (r) and Partial Correlations Controlling for City Population Between Protest Incidents and Selected Independent Variables

	All protest incidents		Black protest incidents	
	r	Partial *r*	*r*	Partial *r*
Percentage black	—	—	−.02	.43
Number of blacks	—	—	.14	.38
Model Cities grant	.26	.16	.23	.35
Number of Black Elected officials	—	—	.07	.34
MPO ratio	−.30	−.33	−.28	−.33
Percentage of minority police	.20	.28	.03	.31
Mayor-Council government	.35	.25	.42	.14
Ward Aldermanic elections	−.01	−.07	.08	.02
Partisanship	.07	.17	.11	.17
Incidence of riots	.23	.07	.22	.07
Black representation ratio	—	—	.30	.54

Nevertheless, set in the theoretical context of my argument, they make considerable sense. I make quite clear that I have used the statistics in a cautious way. I think the reader is not required to suspend belief.

In regard to other parts of the letter, I suppose I agree with Professor Allen that the number of protests per capita might be interesting in itself, but I do not see how it fits into my argument regarding opportunity structures. If he has suggestions, I would have appreciated them. All he has done is to assert that the number of protests per capita is "the dependent variable of interest." For whom is it interesting and why?

Second, I find it odd that the writer would dismiss "most" of the article on the basis of a negative correlation involving a variable he does not relate to the argument. I not only manipulate my data in other ways which support the argument, but I suggest a perspective on protest in a discussion which comes prior to the data analysis and, I think, probably merits some discussion. Professor Allen takes an easy and, perhaps, irresponsible route when he claims that it is "senseless" to discuss other provocative matters in the article in the face of the negative correlation he finds.

PETER K. EISINGER
University of Wisconsin, Madison

TO THE EDITOR:

In her review of my book *The Private Nuclear Strategists* (*APSR*, 67 [December, 1973], 1377–78), Anne Cahn states that it lacks feeling for its subject and is mechanically sloppy. I feel rather the same way about her review, since her specific criticisms are misguided, and she totally ignores the book's content, focus, and conclusions.

One major criticism is that the book represents "blind or crude empiricism." The example cited is a factor analysis with factors which do not seem to make much sense; Cahn thinks I should have explained them. In fact I concluded that there was no clear pattern, so it would be absurd to analyze the separate factors. A related charge is that a large number of tables are included which are not statistically significant. My rule of thumb was to include such tables when they contradicted hypotheses which had been advanced elsewhere.

Cahn points out that the book does not deal extensively with the RAND Corporation or the Summer Study-Daedalus-Brennan volume. This is true. But chapter five studies how people employed by different sorts of institutions, including non-profit research institutes, differ from one another. (The differences tend to be small, producing more of the "not significant" tables which Cahn dislikes but which I find substantively interesting.) More importantly, Cahn misses the point. The book is not an intellectual history, although it naturally draws on the work of Lyons and Morton, Smith, Brodie, Kidd, Green and Gray in this genre.

Four examples are cited in support of the opinion that "senseless data manipulation is here compounded by carelessness." While the book is not particularly easy to read, the carelessness in several of these problems is Cahn's rather than mine. As explained on page 94, 14 per cent of the respondents could not be usefully classified and were therefore omitted from part of the analysis; thus in Table 31 the percentage figures do total 86 per cent and Table 38 cites a total N of 145 even though the columns add up to 125. In the same table the significance figure of .02 concerns the difference between the disciplinary affiliation of highly influential respondents and the rest of the sample; it is therefore calculated with a N of 177, not 9. The fourth point is more substantial but is a data presentation problem, stemming from my attempt to reduce the sheer amount of data in the book; I will be glad to send the figures to anyone interested.

More important than these particular points, however, Cahn's review manages to totally ignore the substantive questions discussed in the book, such as: who is influential within the field, the role of ex-military personnel, motives for entering the field, whether the motives are fulfilled, differences among individuals employed by different types of institutions, the role of academic disciplines, part-time vs. full-time work, corporateness, moral responsibility and research contracts, future supply and demand, and the significance of the private nuclear strategists for American democracy. None of these is even mentioned in this review. If one test of a book review is that it accepts a book on its own terms and evaluates how well it accomplishes its author's goals, this one is a clear failure. Ole R. Holsti demonstrates that this can be done with this book in his review in *Science*, September, 1972.

ROY E. LICKLIDER
Douglass College
Rutgers University

TO THE EDITOR:

Professor Licklider's first few paragraphs elucidate the arguments of my review better than the review itself did. As for the substantive questions he feels the review should have been addressed to, a few quotes from his book might illustrate why they weren't.

Q: Who is influential in the field?
A: p. 165—"Because of the nature of the study, the amount of influence the private nuclear strategists have had on American policy

in strategy and disarmament cannot be determined."

Q: The role of ex-military personnel?

A: p. 51—"It would be unwise to overestimate the magnitude or significance of the differences between the ex-military and the remainder of the population."

Q: Motives for entering the field?

A: p. 54—"The motives for entering the field are undoubtedly as numerous as the population."

Q: Differences among individuals employed by different types of institutions?

A: p. 75—"Differences in motivation between individuals employed by different institutions are unimportant."

ANNE H. CAHN

Harvard University

TO THE EDITOR:

How can Peter Taylor (*APSR*, 67 [Sept., 1973], 948) claim that the number of reflex angles of a closed shape cannot be larger than the number of nonreflex angles? Consider a district of which three sides are like three sides of a rectangle, and the remainder of the boundary approximates a semicircular indentation (Fig. 1). Such a figure clearly has four nonreflex angles. If the remainder consists of more than five line segments, there will be more reflex than nonreflex angles, and Taylor's proposed I_A will drop below zero—in principle, arbitrarily far below zero.

Such a shape presents a special case of problems arising because I_A is insensitive to distinctions among angles beyond the dichotomy of reflex and nonreflex. I_A therefore evaluates a district with one deep indentation (e.g., Fig. 2A) more compact than one with several shallow indentations (e.g., Fig. 2B). I_A also suffers as a summary measure of compactness because it is insensitive to elongation and noncontiguity, listed by Taylor as criteria of noncompactness. Neither a very long ellipse nor two circles have more indentedness than one circle.

On the other hand, I question whether puncturedness is necessary as a criterion of noncompactness, as Taylor proposes. Enclaves within a district either represent unalterable boundaries, in which case they need not be considered, or are detached portions of another district, in which case they already affect measures of that other district's compactness, or are entirely enclosed districts such as Colorado's First Congressional District (Denver), in which case they still affect measures of compactness for the surrounding district based on other criteria, including Taylor's I_A.

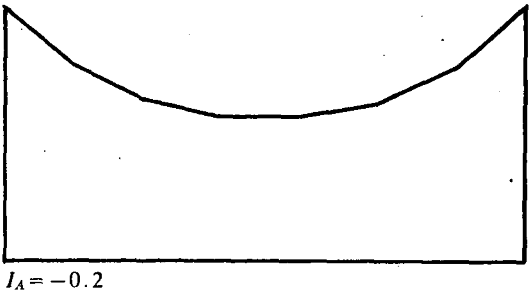

Figure 1

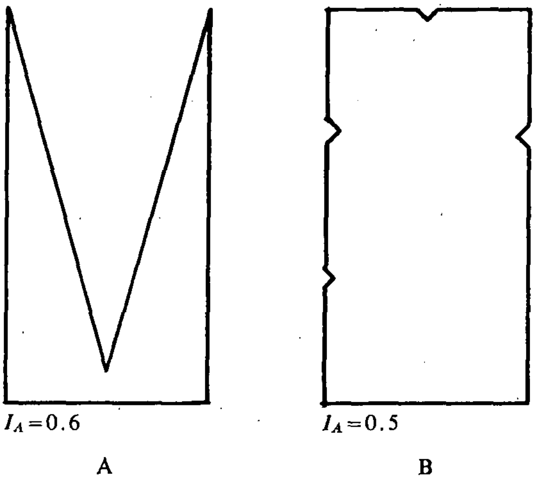

Figure 2

In light of the above, I doubt not only that I_A satisfactorily measures compactness generally (which Taylor does not claim), but even that it satisfactorily measures indentation. Although my objection might be met by taking into account the magnitude of the angles, the ratio of lengths of indented sides to total perimeter, and similar quantities, to do so would render the measure complex. One might also measure indentation (and compactness generally) by the proportion of the area of the smallest circumscribed circle lying outside the district (Fig. 3). But it is not clear to me that measures of aggregate divergence from center of gravity, which Taylor mentions, are not equally satisfactory indicators of compactness.

A simpler and more elegant measure than any of these is the ratio of a district's area to its perimeter. This quantity is minimized in the circle, the most compact shape possible. Since the area of a circle, A, equals πr^2, and its circumference, C, equals $2\pi r$, it follows that $A/C=r/2$. Therefore, for a circle, $2A/rC=1$. For any other shape, calculate the radius of a circle of the same area as the shape: $r=\sqrt{A/\pi}$. Then, letting $P=$ perimeter, $2A/rP$ approaches 1.0 as the shape approximates a circle, and 0.0 as it becomes less compact. Thus the maximum of this measure is

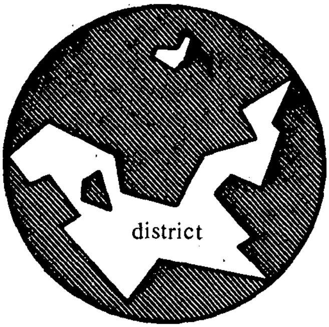

Figure 3

one and its minimum zero, as Taylor desires, and the measure is sensitive to all his criteria of non-compactness.

If a mathematically natural measure of compactness is desired, this ratio is the one I would unhesitatingly propose. On the other hand, it may be felt that a measure of compactness in politics should evaluate not geometric relations, but the locations of inhabitants of the district. After all, politics is concerned with people, as Taylor recalls to us by saying, "very often the shape of districts has been associated with attempts to produce areas within which contact is relatively easy. This contact in turn may foster a sense of community and identity" (p. 948). The geographic appearance of a district on a map is merely a surrogate for such considerations. In increasing order of appositeness to political concerns, a measure of compactness could express the average distance of citizens from: a) center of gravity; b) center of population; c) business or civic center (or central polling place, if any); d) each other.

Concern for community could be further taken into account, and the effects of indentation implicitly recognized, by measuring the relevant distances not by geometric straight lines, but over existing roads lying entirely within the district. Appropriate simplifications could be introduced to make the calculations manageable (e.g., calculating only distances among centers of subcommunities). Finally, such a measure would have to be standardized to the geographic extent of the district, for which density of population might be an appropriate criterion.

RICHARD S. BETH

Boston University

TO THE EDITOR:

I read the comments on the shape measure article with interest. This reply is not so much a rejoinder, but rather an explanation and suggested solution. Of course Figure 1 clearly shows that I_A, as defined in *APSR*, 67:3, 948, can fall below zero and as such is quite unsatisfactory. I must thank the author for pointing this out to me. Quite simply there was a hidden assumption in my reasoning which should have been made explicit. In all the shape experiments carried out in this work, irregular shapes were approximated by polygons made up of straight lines *of equal length*. In this special situation it is correct to say that the number of reflex angles cannot exceed the number of nonreflex angles and $I_A > 0$.

When we generalize an electoral district boundary as a series of straight line segments between base area junctions, the above assumption of equal lengths is normally violated and so the inappropriateness of I_A in this context still obtains. However, now that we have uncovered this assumption, it does lead us to a solution to the problem. Quite simply we define a *weighted* I_A value where each angle is weighted by the total length of the two sides which define it. If we define l reflex angles and m nonreflex angles and measure the total length of sides defining each angle (W_i) then

$$R = \sum_{i=1}^{l} W_i$$

$$N = \sum_{i=1}^{m} W_i$$

We can now define a weighted measure

$$I_A = \frac{N - R}{T},$$

where $T = N + R$ as before.

We can illustrate this weighted measure of indentation by using Figure 1. In this case there are six reflex angles each of which are defined by two lines of total length 0.8 inches. Hence $R = 4.8$. The four nonreflex angles are weighted 1.65, 3.5, 3.5, 1.65 so that

$$N = 10.3. \qquad T = 15.1;$$

$$I_A = (10.3 - 4.8)/15.1 = 0.36.$$

This weighted measure of indentation also solves the criticism of unweighted I_A illustrated in Figure 2. Using the weighted approach these shapes now produce values of $I_A = 0.59$ and $I_A = 0.75$ for diagrams *A* and *B* respectively.

Finally, let me reiterate the purpose of the original article. It was to produce a simple measure of compactness in terms of divergence along an indentation scale. Thus elongation and other shape concepts are not measured by I_A. The above modifications, and those suggested in the comments above, complicate this measurement exercise so that it can no longer be carried out by inspection from maps. However, even in its weighted form I_A is a relatively easy measure to compute and work with.

Peter J. Taylor
University of Newcastle upon Tyne

To the Editor:

Just a note to let you know that the highly unprofessional review of my *The Higher Circles* in your journal last year by Earl Latham did not go unnoticed. At the time it appeared, though, I thought it better to ignore such a nasty little diatribe. Lately, however, I have been in a more combative mood toward critics, and I would like to say a few words in response to the most unfair of Latham's many unfair remarks. These remarks have to do with my alleged affinity for right-wingers.

Latham gets in many little digs on this subject in his original review, but his most tasteless remark along these lines is in his reply to the criticism of his review by Daniel Hoffman. In this reply he says I differ from ultra-conservatives only in that I think they "go too far when they talk about an 'international Communist-Jewish conspiracy'." But my chapter on ultra-conservatives shows I think the ultra-conservatives are totally wrong conceptually even though they have more or less pinpointed the "who" of the power elite. On page 286, early in my essay, I say I am going to show how I differ from them by analyzing their root concept, "the international Jewish-Communist conspiracy." I then analyze each aspect of this concept and conclude on page 307 that "In my view, then, there is nothing like an international Jewish-Communist conspiracy, although I do not dismiss the notion out of hand because it is supposedly the emanation of a particular style which is said to be more associated with some ideas than others and with more bad causes than good ones." This is clearly not a matter of their going "too far." I differ totally from them in my theory, and Latham goes beyond the boundaries of fairness in his distortions.

Indeed, a main point of the chapter on ultra-conservatives was to show the utterly irresponsible, ad hominem way in which pluralists have treated ultra-conservatives. I had come to realize the nastiness of this psychologizing because of the fact that I was being treated in the same way by pluralists, and I thought that a serious essay on the ultra-conservatives might jolt pluralists into an awareness of just how badly they behave towards anyone who dares to disagree with them. In the case of Earl Latham, my plan obviously did not work.

Now I want to turn to a gross distortion in Latham's original review. In this instance he accuses me directly of being like an ultra-conservative: "Like the McCarran committee also, Professor Domhoff is suspicious of the academy, and in a piece of insinuation worthy of *his predecessors* [my italics; GWD] in the early 1950's he says, 'While I do not believe for a minute that the power elite dictate to those scholars [who participate in CFR activities] as to what they should say, it should be clear that members of the power elite see no reason to discontinue their support of such efforts'." The reader should reread the brackets in that sentence, which were added by Latham, for I will show that they are a distortion of the "scholarly" activities to which I am making reference.

In any event, this one sentence by Latham contains so much malice that I hardly know where to begin. First, he does not let his readers know that the sentence followed a brief discussion of secretly financed books that scholars write for the CIA, producing one version for the CIA and a "sanitized" version for the general public! Thus, his readers did not know that my readers at that point would be thinking: does the CIA dictate what these scholars write? If these scholars are so compromised that they do not openly present all their results to the public, have they taken the next step and written what they have been told to write? I answer the implicit question by saying No, and I wrote the sentence Latham quotes in preparation for a quote from none other than David Easton, who expresses my view perfectly on this matter. But Latham does not let his readers know that he is taking on David Easton when he mocks my view. For what followed the sentence Latham ripped out of context was the following very reasonable sentence: "The relationship has been explained by political scientist David Easton: '. . . a deeper social reason for the failure of political science to transcend its limitations . . . lies in the proximity of political research to social forces that determine social policy. . . . Entrenched power groups in society, those who have a firm hold on a particular pattern of distribution of social goods, material and spiritual, have a special reason to look askance at this probing into the nature and source of their social positions and activities. They are prone to stimulate research of a kind that does not inquire into the fundamentals of the existing arrangement of things' " (pp. 127–128). If I can still read the English language, Easton has said in a nice way that political science is a superficial discipline which supinely avoids research into anything its corporate masters don't want studied.

So, consider. Professors are lending themselves to the undercover purposes of the CIA, David Easton says that political science doesn't probe very deeply into the existing arrangement of things. And reviewer Latham concludes that I am a McCarranite who unfairly attacks professors!

But there is more. Latham not only left out sentences, he distorted the sentence he quoted, for he inserted brackets into my sentence in which he says my remarks refer to Council on Foreign Relations activities. That is a complete distortion, and I think Latham owes me a personal apology for making it. To repeat, my sentence was in the context of professors who write the "real" thing for the CIA and a "sanitized" version for the public, not in the context of the open relationships to CFR and its study groups. By making this false insertion into my sentence Latham takes the spotlight off the underhanded behavior of orthodox liberal professors and tries to put it on me as an alleged McCarranite. Latham is too preoccupied with saving our "pluralistic democracy" to worry about the fact that our "liberal" colleagues are busily undermining the values of American academia and, indeed, as I show in a later chapter of the same book, the open nature of the society. Despite all the startling revelations about the academy between the years 1965 and 1969 (the years before the book appeared in 1970), Latham can only think of my cynicism about the alleged highmindedness and decency of my fellow social scientists in terms of the ultraconservative attacks of the 1950s.

I will not in this letter try to respond to Latham's many distortions and mockeries concerning my work on social class, class cohesion, and policy formation. For those of you who are interested in seeing for yourselves whether or not Latham is a fairminded critic of my efforts, you can read my most recent and brief statement of them in the third and final chapter of *The Bohemian Grove & Other Retreats: A Study in Ruling-Class Cohesiveness* (Harper and Row, 1974). You also can read my brief chapter in a book edited by former Senator Fred Harris which is scheduled to appear in 1974 with Grossman. I think that people who take the trouble to read either of these chapters with an open mind will find that I have a clear and cogent set of disposition concepts that I have empirically supported by a variety of operations and indicators.

G. WILLIAM DOMHOFF
University of California, Santa Cruz

TO THE EDITOR:

In the phrase of John Marshall, it is an "absurdity too gross to be insisted on" for Professor Domhoff to suggest that a review printed in 1972 about a book published in 1970 must have been unfair in 1972 because of something that Professor Domhoff said in 1974, or was about to say in 1974. Perhaps this confusion about what time it is explains his very belated response to the original review, a response littered with shards of accusation ("tasteless," "unfair," "nasty," "unprofessional," "distortion," "malice," etc.). Good manners require us all to overlook these lapses of self-control, and charity will surely enfold them into the forgetfulness that much of his book deserves.

The gravamen of Professor Domhoff's complaint is evidently that he is not now nor has he ever been an ultraconservative ("my alleged affinity for right-wingers"), and he thinks that I have not made this clear. Now, it is pure tedium to engage at any length in a "He said—But I said" exchange over a few sentences or phrases, especially when the complaining party is as often off point as a ballerina in ski-boots. But in the interest of accuracy, posterity should note that I did not say in the missive to which Professor Domhoff refers, that I thought he differed from ultraconservatives "only" in that he thinks that they go too far in talking about an international Communist-Jewish conspiracy. I have no doubt that there are other differences, but he *does* do them honor when he agrees with them, that "they have more or less pin-pointed the 'who' of the power elite." And, as to the "conspiracy," he repeats the statement in his book that he "does not dismiss the notion out of hand." I find this to be a very curious admission, and I find no clarification in the rest of the sentence from which the phrase is quoted. He says that "it" (the "notion" of an international Communist-Jewish conspiracy) is not dismissed out of hand because it "is supposedly the emanation of a particular style which is said to be more associated with some ideas than others and with more bad causes than good ones." I do not think that this clot of words is biodegradable. In any case, I had a little trouble finding it again because the page reference in his letter is wrong.

The confessions in the third paragraph of Professor Domhoff's letter are interesting, but only faintly. He came to realize how nasty the "pluralists" were to the ultraconservatives because he "was being treated in the same way" by the "pluralists," another rather odd association of himself with ultraconservatives. He intended then "to jolt pluralists into an awareness of just how badly they behave towards anyone who dares to disagree with them." This presumptuous scenario was certainly an ambitious one—shock therapy at a distance to correct the patient's manners, as much, perhaps, to promote the doctor's peace of mind, as to benefit the patient's health. I am sorry that this "plan" for the improvement of my behavior "obviously did not work," but there are bound to be disappointments. Maybe, like B. F. Skinner's plan for man, Professor Domhoff's will work better with pigeons than with people.

As to the fourth paragraph of his letter, I was not likening Professor Domhoff to ultraconservatives. I was comparing his approach to that of the McCarran committee; and his insinuation that the power elite does not dictate to scholars but that members of the power elite see no reason for discontinuing their support *is* an insinuation worthy of the McCarran committee.

The fifth paragraph of Professor Domhoff's complaint suggests the need for remedial writing. In his book, he talks about the relationship of "corporate-controlled foundations, think factories, and university research institutes" (p. 126). So—he says that Gabriel Almond offers thanks to the Carnegie Corporation for the money that made *The Appeals of Communism* possible; Carnegie supplied the funds for Almond and Verba's *Civic Culture;* Thomas Schelling wrote *The Strategy of Conflict* while at the Rand Corporation; Herman Kahn did most of the research for *On Thermonuclear War* while at the Rand Corporation; Lucian Pye had the help of Carnegie money for *Aspects of Political Development;* W. W. Rostow wrote *Stages of Economic Growth* and had Carnegie money; and Harry Eckstein had Carnegie money to help with the editing of *Internal War*. None of this has anything to do with the private and "sanitized" versions of the same work that Professor Domhoff refers to in his letter, and the CIA is not mentioned at all. What Professor Domhoff did say after reciting the list of works indicated was that "Eckstein's own contribution, 'On the Etiology of Internal Wars'" had appeared in an earlier version in *Social Science and National Security*—"a book," he says, "which had government circulation only." (*Internal War*, incidentally, contains no such contribution by Eckstein, or by any other author.)

It is at this point that Professor Domhoff then says, "Apparently such secret books are not unusual" (p. 127), and he cites a secondary source for the proposition. That's about all there is on the "secret books." If he thought that he was talking about "secretly financed books that scholars write for the CIA, producing one version for the CIA and a 'sanitized' version for the general public," he never said so. If, however, he is now saying that all of the works mentioned were secretly financed books written for the CIA in two versions, one private and one public, he has supplied no evidence. Indeed, it seems to me that if this is his present position, there are many questions without answers. What kind of a secret is it if he knows about it? Is a Carnegie grant evidence of CIA influence? Whatever made Gabriel Almond spill the beans when he offered thanks to the Carnegie Corporation?

He then confronts me with David Easton or, rather, "none other than David Easton." I did not let my readers know, he suggests, that I was really "taking on David Easton." Such an undertaking should dismay all but those of the most abandoned and malignant heart, and I should long have hesitated before entering upon such a trial. Happily, it was not then necessary to do so, nor is it now, for two reasons. The first is that I have no disagreement with David Easton in the matters cited by Professor Domhoff; and the second is that Professor Domhoff's elision from Easton's text misrepresents Easton's position.

First, there is no disagreement because it is obvious that political science does concern itself with the most contended area of human experience, namely, the basic distribution of power in society. As Easton says, "psychology, sociology, or economics" are "less intimately connected with revealing the actual locus of power in the community," or the channels through which social policy is made, than is political science. I certainly agree that political science is a dangerous subject, and it is clear that the possibility of reprisals for probing may be inhibiting and that they may, in fact, have been inhibiting. Second, Easton says that however "inadequate its success" in transcending its limitations in this respect, "political science is reaching towards an understanding of the very things that men consider most vital"—their differences over what Easton describes as the authoritative allocation of values. And one who knows the literature must agree that both before Easton, and afterward, there have been many fine studies of who gets what, why, when, and how; what the political structure is; where power resides; and so on. But the remark of Easton just quoted is what Professor Domhoff left out. By leaving it out, he makes inane, and therefore false, the opinion he attributes to Easton, to wit, "that political science is a superficial discipline which supinely avoids research into anything its corporate masters don't want studied."

In his seventh paragraph, Professor Domhoff fusses about the "context" in which he made his remark about private and "sanitized" versions of the same study, and says that he was not talking about the CFR. He says he was talking about the CIA. But I have already shown that he does not mention the CIA at all in the "context" in which he now thinks he was talking about the CIA. In fact he was talking about Eckstein, but if we would really wish to know the context in which he actually mentioned the CIA, it has to be the larger universe of the CFR, CED, international research centers at various universities, Rand, Carnegie, Rockefeller, and so on. His "context" then does include both the CFR and the CIA. He says, for example, that CIA leaders and foreign policy leaders "interact at CFR study groups" (p. 339). He says that the CIA and major foundations supported the Center for International Studies at MIT, which produced the Millikan-Rostow report with which other reports done by Brookings and the National Planning Association were in harmony which was to be expected because the NPA in 1958 had interlocking directors with the CFR and the CED (p. 146). So, the CIA, CFR, CED, NPA, and MIT are all a thick swirl in Professor Domhoff's higher circles.

The fact that Professor Domhoff uses the Easton quotation betrays his confusion about what context he is in. Easton was not talking about writers who publish private and public versions of the same work. He was talking generally about the relation—to use Professor Domhoff's revision—between a "superficial discipline" and its "corporate masters," which is quite a different thing.

Finally, like a previous critic on the exciting subject of my opinions, Professor Domhoff goes in for mind-reading. I am "too preoccupied" with saving our pluralistic democracy, he says, to "worry about" the fact that liberal colleagues are undermining academia; and I "can only think" of his "cynicism" about the "alleged high-mindedness and decency" of his "fellow social scientists." What I am preoccupied with, and worry about, and can only think, is that an author identified by his champion as "a psychologist by training who specialized in personality theory" is simply out of his element in the higher circles of sociology and politics.

EARL LATHAM

Amherst College

EDITORIAL COMMENT

Tempest. We have, for our sins, received a brotherly communication from Professor Arnold Rogow, the editor-in-chief of *Comparative Politics*, hotly disputing certain of the figures in a recent editorial comment (December 1973) concerning the deplorable lack in the *APSR* of articles on China. These figures seem to show—and were so interpreted—that articles on China appear very seldom in any of the main political science journals. Our brother Rogow does not dispute these figures, or indeed anything else bearing on the point of the editorial comment. He does complain, however, that articles purportedly on Latin America appearing in *Comparative Politics* were undercounted, and that the proper number is 7, not 4. For South and Southeast Asia their figures are 11 and ours 4. And for Canada their figures are 3 and ours 0. (For "underdeveloped areas" they had 8 and we counted 10.) Professor Rogow further points out, "The *New York Times* regularly has a column of 'corrections' even if the statement corrected is rather insignificant. I recall a case when a *Times* story referred to someone as the 'brother' of a deceased person. In a subsequent edition the statement was corrected to read 'brother-in-law.' Is the *APSR* less responsible than the *New York Times*?"

Heaven forbid. We are happy, therefore, to print his correction, and we trust our brother-in-law Rogow will rest content.

Unwritten Rules. It has not yet ceased to amaze us to see the extent to which the *Review* is read cabalistically, rather than straightforwardly, by political scientists. People frequently try to figure out how the *Review* does its business by inference rather than by inquiry. For instance, given our backlog, it is easy enough to discern why we would rather print short articles than long ones. Nevertheless, if a long article makes the grade, and more than a few have over the last couple of years, we go ahead and print it. The fact that we do this should be plain enough to any readers of the *Review*.

Yet for some reason people occasionally tell us that they assume the *Review* has some sort of page limit on manuscripts regardless of quality. In recent weeks we have had to assure two different authors of moderately lengthy manuscripts that this was not so, and that we would not reject a manuscript out of hand for reasons of length alone.

Likewise, we do not discourage the submission of different manuscripts either by authors who have recently been published by the *Review* or by those recently rejected. Fortunately, somebody asked us about that one. We take the same view as seems to have been taken by the editor of *Annalen der Physik*, who in the year 1905 was pestered by manuscripts from a twenty-six year old Swiss civil servant named Albert Einstein no less than four times. The published articles that resulted[1] did not particularly advance the career of the editor who accepted them, but three of them turned out to be significant landmarks in the study of modern physics.

Nobody knew ahead of time where and in what ways 20th century physics would develop. Owing to the institutional structure of the time, a forecaster could be confident only that if it was going to develop at all, it would have to do so through the medium of scholarly exchange, and predominantly through the journals.

Although the idea of an embryonic Einstein lurking in our midst is nice to contemplate, it is unnecessary for there actually to be one for us to pursue the *Annalen der Physik* policy of encouraging scholars who have two things to say to say them both. Consequently, every new manuscript, from whatever source, is treated separately, and except in the rare case where a preliminary screening discloses its inappropriateness for the *Review*, it is assigned referees, and proceeds through our editorial process, just like every other new manuscript. Nobody gets a free ride—and as a matter of fact nobody has asked for one. Nobody is turned away without consideration on the merits. That is what we are here for.

On Correcting the Record. Speaking as an author, the three worst things an editor of a scholarly journal ever did to me were: (1) Solicit a book review, which was duly supplied, and then reject the review on the stated grounds that it evaluated the book differently from published reviews the editor had seen. Instead, this editor asked permission to condense the review into a few sentences and run it as an anonymous book note. Permission was refused, whereupon the editor did it anyway. (2) Accept a review-essay for publication and then edit it into partial unintelligibility without informing me, and printing the resulting mess under my name. (3) Summarily refuse to print a letter offering a correction of an article that had flagrantly misquoted my work. This happened to be about a subject on which the editor himself had in his writings adopted the position of the article I was endeavoring to correct.

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[1] As readers will observe from their titles, these articles were not even written in acceptable English: "Über einen die Erzengung und Verwandlung des Lichtes betreffenden heuristichen Gesichtspunkt," *Annalen der Physik*, Series 4, Vol. 17 (1905), pp. 132–148; "Über die von der molekularkinetischen Theorie der Wärme geforderte Bewegung von in ruhenden Flüssigkeiten suspendierten Teilchen," *ibid.*, pp. 549–560; "Zur Elektrodynamik bewegter Körper," *ibid.*, pp. 891–921; "Ist die Trägheit eines Körpers von Seinem Energieinhalt abhängig?," *ibid.*, Vol. 18 (1905), pp. 639–641.

These three editors have a special place in my own personal hall of fame. No one who, after experiences of this sort, himself becomes an editor, can help wondering how many halls of fame he inhabits. This may assist in explaining why we conscientiously strive to minimize at least these three classes of mistakes at the *APSR*. We do not knowingly print material contrary to the expressed wishes of authors. We give authors ample opportunity to monitor all the editorial work we perform. And we bend over backward to accommodate most persons who claim to have been misrepresented by material printed in the *APSR*. There is a risk involved in this last policy, and we do try to discriminate between what appear to us to be reasonable complaints and people merely seizing an occasion for self-advertisement, a problem that crops up with some frequency in the book reviews. Any unfavorable review is likely to be perceived by the author of the book in question as unfair. Yet not all authors of panned books write in. Of those that do, how many have a legitimate claim on our scarce space and our readers' valuable time? Opinions are bound to differ on this point. Some readers have shared with us the view that most of the complainants in our letter columns are merely serving themselves and not the cause of scholarship. Yet if we were to turn a deaf ear to all such complaints, some serious misrepresentations, we are convinced, would go uncorrected. Every reader will have to decide for himself how successfully we walk this particular thin line.

Errata. In Brian D. Silver's "Social Mobilization and the Russification of Soviet Nationalities," a line of type has been transposed: the third line on page 65 (March 1974) is missing, and turns up intact as line 23. The original sentence should read: "But two long-term trends working together favor Russification: not only are natives becoming more urbanized, and thereby more available for Russification, but the probability that the urban resident will be Russified also appears to be growing."

In Gregory Henderson's review of *Korean Development: The Interplay of Politics and Economics* (March 1974), a line of type on page 298 has been transposed; what should be the 9th line is set as line 2. The relevant section should read: "Although the Park government has, in effect, ceased even such attempts as it once made to be genuinely popular and although repression renders the degree of support or opposition alike immeasurable, this economic concentration and its fruits surely continue to give the government whatever degree of genuine stability it has; and, for the moment, this degree appears still to suffice. The story of how this took place, told with some clarity by two men who observed its details, is of great value.

In Ted Robert Gurr's review essay, "The Neo-Alexandrians: A Review Essay on Data Handbooks in Political Science" (March 1974), the sentence at the end of page 243 and at the beginning of page 244 should read "The compilers respond by resorting to cross-checking against multiple sources, double coding, and sometimes defensive codicils. A third common feature of the handbooks is their 'institutionalized' origin."

In Table 3 (page 1217) of "Party and Incumbency in Postwar Senate Elections" by Warren Kostroski in the December 1973 issue, the column totals for the Senate should read 195, 171, and 87.7. The textual reference just above the table should also be changed accordingly.

Articles Accepted for Future Publication

C. Arnold Anderson, University of Chicago, "Conceptual Framework for Political Socialization in Developing Societies"

Neal Andrews, Wayne State University, "Integration and Community in Communist Theory"

Robert L. Ayres, University of California, Berkeley, "Development Policy and the Possibility of A 'Liveable' Future for Latin America"

Richard Allen Chapman, University of Montana, "*Leviathan* Writ Small: Thomas Hobbes on the Family"

John P. Clark, III, City College, Loyola University, "On Anarchism in an Unreal World: Kramnick's View of Godwin and the Anarchists"

Claude S. Colantoni, Terrence J. Levesque and Peter C. Ordeshook, Carnegie-Mellon University, "Campaign Resource Allocations Under the Electoral College"

Andrew T. Cowart, University of Iowa, Tore Hansen and Karl-Erik Brofoss, University of Oslo, "Budgetary Strategies and Success at Multiple Decision Levels in the Norwegian Urban Setting"

Geoffrey Debnam, University of Otago, "Nondecisions and Power: The Two Faces of Bachrach and Baratz"

Douglas Dobson, Northern Illinois University and Douglas St. Angelo, Florida State University, "Party Identification and the Floating Vote: Some Dynamics"

Dennis L. Dresang, University of Wisconsin, Madison, "Ethnic Politics, Representative Bureaucracy, and Development Administration: The Zambian Case"

Claude S. Fischer, University of California, Berkeley, "The City and Political Psychology"

Richard Funston, San Diego State University, "The Supreme Court and Critical Elections"

G. David Garson, Tufts University, "On the Origins of Interest Group Theory: A Critique of a Process"

Mark Gavre, University of California, Los Angeles, "Hobbes and His Audience: The Dynamics of Theorizing"

Sheldon Goldman, University of Massachusetts, Amherst, "Voting Behavior on the U. S. Courts of Appeals Revisited"

Fred I. Greenstein, Princeton University, "The Benevolent Leader Revisited: Children's Images of Political Leaders in Three Democracies"

Fred W. Grupp, Jr., University of Connecticut and Allan R. Richards, Louisiana State University, "Variations in Elite Perceptions of American States as Referents for Public Policy Making"

Ted Robert Gurr, Northwestern University, "Persistence and Change in Political Systems, 1800–1971"

Richard Child Hill, Michigan State University, "Separate and Unequal: Governmental Inequality in the Metropolis"

K. J. Holsti, University of British Columbia, "Underdevelopment and the 'Gap' Theory of International Conflict"

Robert T. Holt and John E. Turner, University of Minnesota, "Crises and Sequences in Collective Theory Development"

M. Kent Jennings, University of Michigan and Richard G. Niemi, University of Rochester, "Continuity and Change in Political Orientations: A Longitudinal Study of Two Generations"

Jae-On Kim, University of Iowa, John R. Petrocik, University of Chicago and Stephen N. Enokson, University of Iowa, "Voter Turnout Among the American States: Systemic and Individualistic Components"

David Koehler, American University, "Vote Trading and the Voting Paradox: A Proof of Logical Equivalence"

Walter Korpi, University of Stockholm, "Conflict, Power and Relative Deprivation"

J. A. Laponce, University of British Columbia, "Prolegomenon to the Study of Spatial Archetypes and Political Perceptions"

Peter M. Leslie, Queen's University, "Interest Groups and Political Integration: The 1972 EEC Decisions in Norway and Denmark"

Alan Marsh, Social Science Research Council, "The 'Silent Revolution,' Value Priorities, and the Quality of Life in Britain"

Joseph A. Massey, Dartmouth College, "The Missing Leader: Japanese Youths' View of Political Authority"

Kenneth John Meier, Syracuse University, "Representative Bureaucracy: An Empirical Analysis"

Patrick J. McGowan, Syracuse University and Robert M. Rood, University of South Carolina, "Alliance Behavior in Balance of Power Systems: Applying a Poisson Model to 19th Century Europe"

Stanton Peele, Harvard University and Stanley J. Morse, Pontifica Universidade Catolica de Sao Paulo, "Ethnic Voting and Political Change in South Africa"

N. Patrick Peritore, University of Missouri, Columbia, "Some Problems in Alfred Schutz's Phenomenological Methodology"

David E. Price, Duke University, "Community Control: Critical Democratic Theory in the Progressive Period"

Adam Przeworski, University of Chicago, "Institutionalization of Voting Patterns or Is Mobilization the Source of Decay?"

Douglas Rae, Yale University, "The Limits of Consensual Decision"

Joseph A. Schlesinger, Michigan State University, "The Primary Goals of Political Parties: A Clarification of Positive Theory"

Brian D. Silver, Florida State University, "Levels of Sociocultural Development Among Soviet Nationalities: A Partial Test of the Equalization Hypothesis"

J. S. Sorzaon, Georgetown University, "David Easton and the Invisible Hand"

Peter G. Stillman, Vassar College, "The Limits of Behaviorism: A Critique of B. F. Skinner's Social and Political Thought"

C. Neal Tate, North Texas State University, "Individual and Contextual Variables in British Voting Behavior: An Exploratory Note"

Kent L. Tedin, College of William and Mary, "The Influence of Parents on the Political Attitudes of New Voters"

Edward R. Tufte, Princeton University, "Determinants of the Outcome of Midterm Congressional Elections"

Eric M. Uslaner and J. Ronnie Davis, University of Florida, "The Paradox of Vote Trading: Effects of Decision Rules and Voting Strategies on Externalities"

Clement E. Vose, Wesleyan University, "Political

Dictionaries: A Bibliographical Essay"

Meredith W. Watts, University of Wisconsin, Milwaukee, "B. F. Skinner and the Language of Technological Control"

J. Weinberger, Michigan State University, "Hobbes's Doctrine of Method"

Herbert Weisberg, University of Michigan, "Models of Statistical Relationship"

Mary B. Welfling, Virginia Polytechnic Institute and State University, "Models, Measurement and Sources of Error: Civil Conflict in Black Africa"

Louis P. Westefield, Southern Illinois University, Edwardsville, "Majority Party Leadership and the Committee System in the House of Representatives"

Chemical and Biological Warfare*

GEORGE H. QUESTER
Cornell University

Why should the world be more averse to chemical and biological warfare (CBW) than to other forms of war, repeatedly attempting to ban such warfare by means of international law? Is there some objective difference between poison gas or plague bacilli on one hand, and steel bullets, or B-52 high explosive bombs, or nuclear weapons on the other? Or does CBW instead illustrate the faddishness and superficiality of disarmament campaigns in general?

The Allied Powers after 1919 may indeed have condemned chemical warfare simply to reinforce the impression that Germany was morally culpable in World War I. The Germans supposedly had initiated the use of gas warfare (although the "first use" here might really have to be pinned on France instead); if Germans had used such weapons first, such use must have been evil. Perhaps instead the aversion to gas reflected the horrors of such warfare for the soldier exposed to it, although casualty statistics suggest that fewer soldiers died or were permanently maimed after being incapacitated by gas, as compared with those stricken by other means.

Perhaps instead the techniques of chemistry and biology have seemed unclean and unmanly, the products of "odd-ball" scientists ensconced in laboratories, reflecting perverted scientific ingenuity rather than the bravery of cold steel. The sheer unpredictability of poisons and of epidemics could thus make all war a little less comparable with the past, and a little less manageable. Some or all of these explanations probably account for the remarkable aversion of much of the professional military to CBW, an aversion noted in several of the works under review.

Most recently, aversions to CBW have been powerfully reinforced in the United States, and in some other advanced industrialized societies, by the more general concern for ecology. We are becoming aware that it is difficult to predict the side effects of any major chemical or biological innovation, side effects which sometimes far outweigh—often negatively—the result originally sought. War of any kind is presumably bad for children and other living things. War fought with esoteric diseases or with herbicides may be particularly bad for the ecology, i.e., for living things far into the future.

Yet the very mention of ecology will suggest a certain faddishness to the political scientist, a certain "here today, gone tomorrow" about such causes which makes us reluctant to bet on them as bases for serious political movements. Much of this faddishness has all along been present in debates on the chemical warfare issue. In 1922, the U.S. Senate ratified a treaty which would have renounced all American options for chemical warfare; the treaty never became binding, but only because the French Parliament failed to ratify it for an entirely separate reason. By 1926, however, the U.S. Senate refused to ratify the Geneva Protocol, precisely because American public opinion had changed on the chemical warfare question. As Volume I of the SIPRI study (*The Rise of CB Weapons*) points out, what had seemed a loathsome German weapon in 1922 had been redefined by 1926 as a potentially humane technology which would allow enemies to be disarmed without first killing or maiming them. Part of this change of feeling, but only part, can be attributed to assiduous public relations work by the U.S. Army's Chemical Warfare Service. Perhaps the rest can be explained by the fact that the American public did not yet have to fear chemical attacks on its cities; in the view of the SIPRI volume, this would similarly account for Japanese resistance to bans on gas warfare.

Yet even this explanation may be too deterministic, since each country in the later 1920s had begun to show some apprehension about air raids in future wars—raids from hostile aircraft carriers or by intercontinental airplanes following the trails blazed by Lindbergh and others. In the speculative analyses of the time, war was expected to be brought home to the civilian population by air raids on cities, with the killer weapon often cast as poison gas. But the literature of the same period shows a persistent strain of analysis, in Britain and on the continent as well as in the United States, favoring the development of nonlethal incapacitating chemical and biological weapons. Volume V (*The Prevention of CB*) of the SIPRI books chronicles some of this literature; H. G. Wells's *Things to Come*, and the movie produced from it, are among the best examples, in which a sleep-inducing gas becomes the wonder-weapon for the great civilizing force which establishes a hegemony to restore peace to the world.

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* *Chemical Warfare: A Study in Restraints*, Frederic J. Brown (Princeton: Princeton University Press, 1968. Pp. 316. $9.00.)

The Control of Chemical and Biological Weapons (New York: Carnegie Endowment for International Peace, 1971. Pp. 130. $1.00.)

A Survey of Chemical and Biological Warfare, John Cookson and Judith Nottingham (New York: Monthly Review Press, 1969. Pp. 420. $3.95.)

Harvest of Death, J. B. Neilands, G. H. Orians, E. W. Pfeiffer, Alje Vennema, and Arthur H. Westing (New York: The Free Press, 1972. Pp. 304. $10.00.)

CBW, Steven Rose, ed. (Boston: Beacon Press, 1969. Pp. 209. $1.95.)

The Problem of Chemical and Biological Warfare, 6 volumes (Stockholm International Peace Research Institute, 1971, 1973)

- I. *The Rise of CB Weapons* (1971, Pp. 395, Sw. Kr. 75)
- II. *CB Weapons Today* (1973, Pp. 420, Sw. Kr. 75)
- III. *CBW and the Law of War* (1973, Pp. 180, Sw. Kr. 40)
- IV. *CB Disarmament Negotiations, 1920–1970* (1971, Pp. 412, Sw. Kr. 75)
- V. *The Prevention of CBW* (1971, Pp. 287, Sw. Kr. 40)
- VI. *Technical Aspects of Early Warning and Verification* (1973, Pp. 350, Sw. Kr. 75)

The world today is obviously moving back into an anti-CBW posture, and it is again necessary therefore to separate the serious motivation from the less serious. Ecology has been already mentioned. The sheer magnitude of what is possible in CBW has been enormously broadened since the 1930s, in part by precautionary research on both sides in World War II, although CBW was generally not used. Such research has clearly been continued in the United States and the Soviet Union since 1945. While some interesting nonlethal weapons have been developed, enormously lethal ones have also been developed in the same effort, making the very range of possibilities frightening.

Poison gas may have been used by the Egyptians in Yemen since 1963, but this probably does not account for so much of current anti-CBW sentiment. Irritant gases and chemical defoliants were extensively used by the United States in South Vietnam, and here we have a more important cause of the latest shift in public attitudes, indeed for the publication of a number of the books under review. Yet again some critical questions might be raised about the straightforwardness of these indictments. Is the American use of CS gas (a "tear gas") in Vietnam under attack because it was "gas," or because it was America in Vietnam? If the historical impression of the immorality of "gas warfare" has been merely another stick with which to beat U. S. Southeast Asian policy over the head, has it been such a sturdy stick? Some of the authors under review here are mainly opposed to chemical warfare per se (although one senses that not a single one of them supported non-CBW American military operations in Vietnam). Yet others were primarily opposed to American presence in Vietnam, and might not even have been so "anti-CBW" in other contexts.

Some New Literature

The six SIPRI volumes, collectively entitled *The Problem of Chemical and Biological Warfare*, are a very scholarly achievement. By drawing together the European and Asian chemical warfare experience of the entire twentieth century, the authors generally manage to avoid the parochialism of other works which focus narrowly on the experience and problems of the 1960s. The SIPRI series is a joint product of a number of competent authors, which causes some problems. There is a substantial amount of repetition, occasionally even within particular volumes. Important segments of the series amount to basically noncontroversial, and close-to/encyclopedic technical handbooks, most particularly Volume II on *CB Weapons Today*, which deals with biochemistry more than with politics. Elsewhere, judgments had to be reached about historical events which will never be certain, for example on the exact extent and nature of Japanese chemical warfare in China in World War II, or alleged U.S. use of biological warfare in Korea, or Egyptian use of poison gas in Yemen. In some sections of the work, one senses an excessively self-conscious policy of nonalignment, whereby, for example, the International Committee of the Red Cross, "ICRC," is made to sound no more reliable on investigations of CBW allegations than the so-called International Scientific Commission, whose charges of American germ warfare in Korea are repeatedly dignified in Volume V by the initials "ISC."

The SIPRI series not only collects a great deal of valuable data on the history of CBW, and of efforts to head it off; it also presents fascinating analyses on why CBW was indeed avoided at various crucial stages of World War II, and on what its military impact was at various stages of World War I. One notes that CBW might have changed World War II in making Panzer offensives less possible. Some fair service is given to the arguments that CBW can be more humane, although the SIPRI volumes seem reluctant to let this line of reasoning flow very far before it is rebutted and terminated. All in all, these volumes are a product of which the Stockholm Institute can be proud, coming up to the fine standard of the *Yearbooks on World Armaments and Disarmament*.

Frederic J. Brown's *Chemical Warfare: A Study in Restraints* is an equally scholarly work, based on a detailed examination of the available government memoranda on decisions about gas warfare. It shows how CBW decisions, especially in the United States, have been based on much of the same bureaucratic infighting and "organizational process" drift that characterizes other public policy. Brown argues that chemical warfare has above all been an excessively demanding form of combat, so technologically and organizationally complicated that the U.S. Army and other armies have always had difficulty in getting prepared to wage it; perhaps lack of preparedness thus has outweighed even the fear of retaliation in explaining why we have seen relatively little CBW since 1918. Brown's work is especially good at recapturing the atmosphere within which chemical warfare campaigns were launched during World War I, and then were narrowly averted in 1940 and 1945. He vividly portrays the biases of professional military officers against this weapon which "civilian chemists" were handing over to them.

The Carnegie Endowment book on *The Control of Chemical and Biological Weapons* is less comprehensive than its title suggests. Rather, it is a collection of four monographs on relatively contemporary issues on CBW. The first, "Legal Aspects of the Geneva Protocol of 1925," amounts to a persuasive legal brief against the Nixon position that the Protocol should allow the use of nonlethal gases. As a brief, it tends to overstate its plea at points, for example in analogizing the use of tear gas in POW riots to the gassing of inmates in German concentration camps, and it brushes past any of the stronger arguments for distinction between "good" and "bad" CBW. "The Use of Herbicides in War" makes a strong case that the U.S. may have been hurting its allies and defeating its own purposes by its defoliation policy in Vietnam. "The Military Value and Political Implications of the Use of Riot Control Agents in Warfare" similarly exposes substantial uncertainties and ambivalences about whether directly nonlethal gases in Vietnam have been used to decrease, or to increase, deaths among the Communist forces. "Limitations on Chemical and Biological Warfare Going Beyond Those of the Geneva Protocol" is the least position-oriented paper in the book, presenting a relatively clear and dispassionate analysis of further opportunities and obstacles for the control or prevention of CBW.

Harvest of Death, by J. B. Neilands et al., is an outgrowth of a study begun under the auspices of the American Association for the Advancement of Science, by an ad hoc group which evidently saw the issues as requiring more pressing advocacy than did the Association itself. The study could be better written, but it makes its points clearly enough, that the use of CS irritant gases and chemical defoliants in Vietnam have been evil in all respects, even as the authors feel that the Vietnam war itself is evil. While the book assembles a great deal of technical data on the direct effects and side effects of these forms of chemical warfare, the data might more often have been allowed to speak for themselves.

CBW, edited by Steven Rose, is a collection of very thorough and well-written papers prepared for a 1968 conference on the topic. As such it makes an excellent introductory volume on the subject, and something more. There are chapters on each of the major forms of CBW now possible, and summations of the events in Vietnam and Yemen. Some profound insights are offered on research policy in the U.S., U.K. and U.S.S.R., and on the inevitable loss of responsibility in the secrecy surrounding such work. Indeed, advocates of greater tolerance for CBW will find it difficult to assure us that any subtle distinctions they proposed would actually be adhered to, when scientists or military officers can always hide their actual practice behind the necessary cloaks of military secrecy. A chapter on "Preventing CBW," by J. H. Humphrey and Matthew Meselson (Meselson also is coauthor of a section of the Carnegie Endowment volume) presents a very strong and commonsensical argument that any relaxation of existing aversions to CBW may lead to an open-ended and uncontrollable escalation. While virtually every author in the Rose volume betrays a commitment to preventing all of CBW, the style of argument is the more convincing because it presumes no instant consensus with the reader on the other moral issues of international relations.

A Survey of Chemical and Biological Warfare, by John Cookson and Judith Nottingham, indeed lives up to its title, cataloging and chronicling virtually every known sort and instance of chemical or biological warfare. The authors, here as elsewhere, are plagued by having much more information about CBW research and procurement in the West than in the Communist world; by "telling all they know," they implicitly give the impression that the West is the primary offender. A perhaps unfortunate reliance on an East German indictment for facts about West German activity mars what is otherwise the calm and scholarly tone of the book. Its examination of the "International Scientific Commission" ("ISC") findings of U.S. biological warfare in Korea virtually erases any remaining plausibility for these charges.

CBW: Good or Bad?

Is there an argument for greater tolerance of CBW, an argument that should be given a greater hearing? Or, after a hearing, might we instead conclude that the world is far better off keeping most or all of chemical and biological warfare illegal and beyond the pale? From the viewpoint of arms control, should we even endorse a conspiracy to deny CBW a fair hearing, if these weapons options are better left undiscussed?

Of the books under review, only an appendix in SIPRI Volume V and some sections of Volume I really make much effort to consider the arguments for legitimating chemical and biological weapons. Basically two strands of argument can be offered: (1) Chemical or biological weapons might make war less horrible; and (2) CBW might make war less likely.

As to making war less horrible, such weapons may enable an army to defeat and incapacitate its enemies without killing as many of them, or as many innocent bystanders. While one can put down this argument simply by suggesting that commanders are congenitally indifferent to the homicide of enemies, this rejoinder does not reflect reality. In the U.S. Civil War, was Lincoln really indifferent to how many Confederate soldiers were killed in the process of restoring the Union? Generals and Presidents typically become indifferent to killing only when there is no other way to win battles, or when this is necessary to keep their own soldiers from being killed. The argument of course becomes even more convincing when the local civilians are assumed to be loyal to "our side," and would suffer in any conventional artillery barrage directed at a hostile army in their midst.

As noted, data from World War I suggest that (as compared with other weapons) far more soldiers on both sides could be eliminated from the battlefield by gas attacks without being killed or permanently disabled. Long-term susceptibility to emphysema may suggest the horror of mustard gas attacks, and (as Brown notes) Adolf Hitler's aversion to chemical warfare is indeed said to have stemmed from his personal experience of being gassed in 1918. Yet one must compare this kind of wound with the waves of men who never returned from the trench war at all, after being subjected to machine gun or artillery fire.

Although far deadlier gases have of course been developed since 1918, less deadly CBW weapons have also been devised, weapons which potentially might keep an enemy soldier from killing without doing the slightest permanent damage to him in the process. If used properly, some CBW weapons might thus make war more humane. Whether they would be used properly is of course a more complicated question, a question which the Carnegie Endowment book addresses rather cavalierly, and SIPRI Volume V addresses rather well. It is a question to which this review will return.

But if CBW could make war less unpleasant, would it therefore ipso facto make it more likely? There is at least some risk that the prospect of "humane war" with "tomorrow's weapons" might make all sides more ready to take to the battlefield in resolving political disputes. Yet the impact of CBW on the likelihood of war may depend even more on a different aspect of these weapons: whether they assist the offense or the defense. Does CBW, like the tank, encourage armies to plunge into enemy territory, or does it like the prepared fortification, encourage each side to sit back and let the other attack first? At the margin, weapons which favor the offense increase the likelihood of war, since whoever strikes first in a politically doubtful situation may win. Weapons which favor the military defense conversely increase the chances of peace even during the gravest political crisis, since either military commander will advise his president that there is no point in striking first.

Which way do CBW weapons work? Since gas can filter down into the airshafts of prepared fortifications, or of natural caves, it may seem to aid the offensive; the very premium that is placed on technological innovation (whereby 1916 gases may make 1915 gas masks outmoded, etc.) may similarly suggest striking while the advantage of an innovation still persists. Gas might thus be undesirable even though it made war more humane, if in the process it had made war more likely.

Yet as the SIPRI Volume I points out, the equipment needed for chemical warfare has historically been heavy and burdensome for the individual soldier and for the army as a whole. Could Hitler's Panzer forces have slashed as quickly through Poland and France if each side had been committed to poison gas war? The answer might well be no. If Hitler had been on the brink of a successful amphibious invasion of England, wouldn't gas clouds on the British beaches have been a most effective way of stimying such an offensive? Would Britain indeed not have escalated to CBW at that point? Would we not all have applauded?

If CBW consistently buffered the defenses of armies confronting each other, peace would tend to be more likely when such weapons were kept legal and in stock. One must of course be very careful here in defining terms such as "offensive" and "defensive" in discussing CBW. As used in the SIPRI series and in the Rose book, all forms of CBW weapons are "offensive," while "defensive" refers exclusively to gas masks, inoculations, etc., which neutralize the impact of chemical and biological agents on the human body. While this distinction will seem relatively clear, it does not help us with extrapolations about the likelihood of war.

Pandora's Box?

CBW might thus discourage war in some cases, as well as reducing the costs of war. Yet one has to watch the strategic interactions very closely here to be sure that anything like this will really happen. Gas weapons can be humane, but they can also be very deadly, more so than any weapon developed before the atomic bomb. If chemical warfare had been legitimate in 1939, Hitler's forces might have been slowed and immobilized by all the protective gear they had to carry with them, but air raids on German and Allied cities might have been carried through by nerve gas, with substantial increases in casualties inflicted on the home front. What in fact kept Hitler from using nerve gas "defensively" to blunt the Allied landings at Normandy? As the SIPRI Volume I describes, it was primarily fear of chemical warfare retaliation on the cities of the Reich.

The gas issue is the clearest reminder of how much World War II was a "limited war," with each side still deterred from climbing this final rung of the "escalation ladder," by fear that the other side would reciprocate. Even when all other restraints on the bombing of cities had been lifted, poison gas was not to be dropped into the streets of Hamburg and Dresden and London and Tokyo. If one today were to see aversions to chemical and biological warfare as simplistic, he still would have to confront the fact that Hitler and Roosevelt and Churchill successfully conspired to spare the world such warfare. Were these leaders simply taken in by the moralisms of 1922? Did they not realize that chemical or biological warfare in principle offered the prospect of less painful (if not painless) war, as suggested by H. G. Wells in 1933?

Perhaps the psychedelic gases were not yet ready (or are still not), to transport soldiers blissfully from the battlefield into the nearest prisoner-of-war compound. Yet more important, there hung over the war a sense that "no gas" was a salient distinction, a distinction enshrined in international law and national practice (despite Italian and Japanese violations), a distinction which should not be thrown away, since the next "firebreak" might be difficult to find. Even if a World War II army might have decided to use gas in a particular battle to reduce casualties on both sides (e.g., in forcing Japanese soldiers out of caves on Saipan), everyone involved sensed that it might not stop at this. Will men *be able* to stop with only the more humane uses of chemical or biological warfare? Perhaps, but only if a clear and intricate groundwork of legal definition and strategic dialogue were prepared well in advance.

Will men indeed *wish* to stop with only the more humane uses, once the option has been opened? Perhaps, but only if they are not tempted to win battles and spare themselves casualties by less humane uses. For example, CS gas has been used in Vietnam by American forces to drive Viet Cong out of the caves in which they have been holding innocent villagers hostage. This tactic presumably spared the lives of the villagers and perhaps even of the Viet Cong, wherever the Americans could have thrown grenades or fired rockets into the caves anyhow.

But CS has also been used to drive Viet Cong out of caves when they would have been secure against American grenade attack, driving them into the open where they can be killed by artillery or aerial bombardment. In the first case, CS added a humane alternative where a deadly conventional means of attack already existed. In the second case, CS instead added deadly means of attack which otherwise would not have existed at all.

There is not just a danger of further escalation once the barrier to chemical warfare is lifted, therefore, but a real temptation to escalate. If a new barrier could be erected to separate the desirable uses of CBW from the undesirable, the world might benefit. But if the barrier were not erected, or if it were erected too weakly, a process of escalation might begin for which no end was in sight.

This possibility at least in part accounts for the willingness of a number of authors to dismiss any and all limited uses of CBW, and to condemn the United States government's efforts to keep legitimate the options of tear gas and chemical defoliants in war. At one point the SIPRI volume comments that tear gas has never been used without leading to the use of poison gas (the U.S. in Vietnam by now has disproved this rule).

As noted, the Baxter and Buergenthal piece on "Legal Aspects of the Geneva Protocol of 1925" in the Carnegie Endowment study goes so far as to compare the use of tear gas in suppressing a POW camp riot to the use of poison gas in Auschwitz. If one allows that tear gas is a well-established riot-control weapon in domestic society (all but the authors of the Neilands book seem willing to accept this) the case of POW's is indeed hard to judge. Are they being tear-gassed as enemy soldiers, or as persons who have submitted themselves to treatment comparable to that of civilians? Would Professors Baxter and Buergenthal really have them shot instead?

The distinction between domestic police action and international war is everywhere fuzzy. Guerrilla wars such as the one in Vietnam confuse issues all the more. All such wars tend to start as domestic police actions, wherein all forms of CBW would presumably still be legal, and some are normal. At what point did the Vietnam conflict become a war? If the Viet Cong after a certain date come to be treated as prisoners of war rather than treasonous rebels, do they win an exemption from poison gas or tear gas as of that date? And what of the IRA in Northern Ireland? There is a seeming paradox in a rule that soldiers in war are rigorously to be spared exposure to CBW, while one's own civilians may be exposed; but this is the law of the Geneva Protocol, and in effect is the customary practice around which an important arms control firebreak has been based. Perhaps it makes sense if we see CBW (as exemplified by tear gas) as "humane." Yet if it makes sense, it still raises difficult questions about when the Viet Cong or the IRA change status from domestic insurgents to international army.

There is of course a different kind of apprehension possible about humane chemical weaponry—i.e., not that it would lead to perennial escalation into the lethal range, but rather that it would make war, and military suppression in general, all too easy and too acceptable. Imagine how ruthless a dictator might be if he could continually immobilize his opposition without doing them any bodily harm. Imagine how much of politics would become a matter of "might makes right," as long as the might remained "humane" might. How indeed can American society continue to condemn LSD and the drug culture, if it allows the development and testing of weapons based on LSD and drugs? Is it forseeable that a new oppressed class will emerge which is continually being drugged into submission by the gas attacks of nonlethal CBW? The tactics of passive resistance and civil disobedience are premised on burdening the police and the state with a choice between unacceptable violence or capitulation to protester demands. As the police of Paris or Alabama or Prague develop a third choice, will protest of this sort be all the more ineffective?

This brings us back once again to the interface between "domestic law enforcement" and "international war." As SIPRI Volume V points out, extensive use of "tear gas" at home tends to legitimate its use on the battlefield. In developing esoteric nonlethal battlefield weapons, we conversely run the risk that these weapons will be brought home for domestic use against our own population. The evaluation of such a trend thus depends heavily on what side one is on in the persistent substantive conflicts at home and abroad.

Some Current Issues

For the moment, the most interesting CBW issue indeed pertains to whether the United States can maintain the legitimacy of tear gas as used against an enemy in wartime. The U.S. is not yet legally bound at all to desist from CBW, although several of the works reviewed cite the argument that the Geneva Protocol has become customary international law and thus binding even on states which failed to ratify it. In response to the recent wave of pressure for an explicit American ratification of the Protocol, President Nixon resubmitted it to the Senate, accompanied however with an explanatory statement that tear gas is not covered by the Protocol. While this may be weak as law (as shown by the Baxter and Buergenthal piece in the Carnegie Endowment book or more exhaustively in the SIPRI Volume III on *CBW and the Law of War*) the United States is strong as a nation, and might by its example make this new legal distinction stick. Opponents of CBW within the U.S. Senate have thus been divided on whether to welcome the U.S. renunciation of lethal CBW activities, or to resist this effort to slice the Protocol by such an exemption. Support for Nixon's policy was clearly increased by his accompanying decisions to declare that the U.S. will unilaterally dispose of all its biological warfare agents, never use lethal chemical weapons first in combat, and never use biological agents under any circumstances.

If CBW weapons were our only weapon for killing millions of civilians, the problems of political and moral analysis would be complicated enough. Yet the complication is compounded by the existence of H-bombs which can inflict the same mass homicides even more effectively. Are H-bombs bad because they make war so horrible; or are they a blessing in disguise because they deter war and make it unthinkable? Could CBW weapons thus provide the "poor man's deterrent," giving a capacity for deadly retaliation to states which might not have mastered nuclear physics? ("Poor men" in international relations may of course be less deterrable themselves, since they have less to lose.)

CBW weapons have not served this purpose as yet in history, because they always would have been difficult to deliver to their target in the last hours of a losing war. If Hitler had possessed the H-bomb in 1945, could he not have insisted that Allied forces stay out of Germany, for fear of his "last-gasp" retaliation when such forces approached Berlin? But the threat of nerve-gas attack on London and Paris was never to be as potent, since no single rocket or airplane could carry enough of the materials to deter an Allied pursuit of total victory.

Yet one consequence of continued research and development in CBW is surely to generate compactly deliverable substances (especially the biological) which will be deadly enough to serve as nuclear weapons equivalents. Research in "painless war" will thus produce additional weapons for infinitely painful war; the risks of proliferation of nuclear weapons will be paired with the risks of proliferation in deadly chemical or biological weapons.

Could such CBW proliferation in any way be desirable? Perhaps the threat of Egyptian nerve gas attack on Tel Aviv will be comparable to the threat of Israeli nuclear attack on Cairo in the future, or even now. Yet the world would be more at ease if neither side here acquired either nuclear or CBW weapons. The world is approaching a consensus that nuclear proliferation is undesirable, because such proliferation more often than not increases the risks of war, because it almost always increases the costs of war, and because such proliferation will hasten the day when small dissident factions or criminal syndicates can threaten to blow up a city if their demands are not satisfied. All of such fears apply equally to a proliferation of deadly biological warfare capabilities. CBW indeed might constitute the greater proliferation risk, if underdeveloped and possibly unstable states, or irresponsible political movements, find biology easier to master than nuclear physics. Which weapon will leak out to Black September first, nuclear warheads, or some sort of deadly biological agent? Further CBW research in the U.S., Britain or U.S.S.R. furthers the proliferation risk, for technology once uncovered will leak out. We might well conclude that the H-bomb is all the deadly potential the world needs, or can handle.

As SIPRI Volume V notes, the U.S. argument against an uninspected ban on chemical warfare at times seems almost to assume that there would be no means of retaliating if the Russians suddenly attacked with deadly gases in violation of bans on CBW manufacture and use. Yet the likelihood is hardly great that such a gross assault on American positions would escape nuclear retaliation. More difficult problems in the control of CBW would of course emerge if nuclear disarmament were someday also gotten under way.

This leads us at the end to the question of "inspection." Could we trust the enemy not to stockpile chemical and biological weapons, while we fulfilled our agreements to dismantle all such weapons? Can we ourselves be trusted to fulfill our agreements? Would a reassuring process of monitoring compliance with such agreements have to be disruptive to normal political, scientific or industrial processes?

The SIPRI Volume VI on *Technical Aspects of Early Warning and Verification* is a thoughtful and eclectic collection of papers on these problems. As in other instances of disarmament verification, for example the Comprehensive Nuclear Test Ban or the Nuclear Non-Proliferation Treaty, the study here echoes a Swedish optimism on such questions: verification is necessary, but it can be accomplished at bearable costs. Yet as in these other instances, some other observers may be more pessimistic about whether verification can really be so effective without being excessively obtrusive, perhaps interfering with the disease-fighting pharmaceutical industries of the countries concerned.

Yet some of such concern about the reliability of verification may even be beside the point. Sometimes an arms-control agreement can be self-enforcing. If we really distrust the chemical or biological weapon enough, our statesmen and generals will simply not want to have it around, lest it come into use accidentally, lest by a shift of the wind or other miscalculation we kill more of our own soldiers than of the enemy's. President Nixon's decision to promise never to use biological weapons, even if the enemy used them first, in effect suggests the conclusion that disease will never be a practical weapon for any army that attempts to exploit it. We might as well ask why armies have never employed trained rattlesnakes against each other; on average we would wound two of our own men for every one of the enemy, and an "arms control" agreement not to use this mode of warfare will mostly be an effort to clear the air.

The residual backup deterrent of the atomic bomb, together with the general inapplicability of chemical and biological warfare, might thus truly make inspection and verification unnecessary for bans of CBW. If so, why has our government still resolutely insisted on verification as part of any general ban on the manufacture of such weapons? As SIPRI Volume IV on *CB Disarmament Negotiations 1920–1970* suggests, this insistence may simply reflect years of arms control negotiations in other areas of weaponry, in which the U.S. has instinctively had to demand inspection, and the Russians have instinctively dismissed the need for it.

Alternatively, it might indeed reflect the U.S. government's reluctance to see all forms of chemical warfare banned, perhaps because the prospect of "humane warfare" cannot yet be finally dismissed. More books on the subject may have to be written before this option is fully and finally foresworn.

Tradition and Nationality: A Classic Revisited*

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Walter Bagehot was a frightened Liberal. The adjective is not too strong. Throughout much of his life he was haunted by the fear of social dissolution. A recent biographer finds him "obsessed" with "the problem of authority in the modern state."[1] This intense concern with disorder appears in his letters supporting the coup d'état of Louis Napoleon written in 1851 when he was only twenty-five. It informs his fear in *The English Constitution* that universal manhood suffrage would make parliamentary government "impossible" (pp. 161–163).[2] It is expressed in his panicky response to the passage of the Reform Act of 1867, which led him to put forward in the 1872 Introduction the highly counter-productive proposal that the business and aristocratic classes form a tacit coalition against the menace of a politicized working class (pp. 277–281).

These anxieties may have been based in family experience or personal psychology—there was a history of mental disorder among his near relatives. I want to argue, however, that they arose from a theory of politics. For Bagehot was not the insouciant empiricist that Mr. Crossman took him to be. His perceptions were controlled by an inner eye which at once clarified some brilliant insights and blinded him to elementary facts of political behavior and of British political experience. This inner eye, it is no surprise to say, was formed by classical Liberalism, and Bagehot's blindspots were blindspots of classical Liberalism. If I can put my thesis in one sentence, it is that while Bagehot rightly receded from the bland and optimistic rationalism of that school, he could not overcome its individualism and so was left in a quandary about how to explain the basis of political order.

The Doctrine of Double Government

The two main themes of *The English Constitution* are stated in its opening pages. One is Bagehot's famous discovery of the fusion of executive and legislative powers. His major and complementary theme is the separation of two parts of the constitution: "those which excite and preserve the reverence of the population—the *dignified* parts, if I may so call them; and, next, the *efficient* parts,—those by which, it, in fact, works and rules" (p. 61). While Bagehot admits that this doctrine of "double government" does not hold "with microscopic accuracy," he is quite serious about it and makes the separation of "efficient" from "dignified" apply to institutions, classes, and attitudes. On the efficient side are found the Cabinet and the House of Commons; on the dignified, the Monarchy and House of Lords. For the effective conduct of public affairs, Bagehot looks to businessmen. Yet these members of the middle classes cannot inspire deference. A "manufacturer's son has no chance" in electoral competition with "an honorable, or a baronet. . . ." While the House of Commons does have some "dignity," just as it retains some aristocratic personnel, it is vastly overshadowed by the Lords with their "theatrical show" and the Queen, toward whom "the multitude" feel "reverence." Apart from this function of inspiring deference, however, the traditional upper classes contribute very little. When they do appear in the Commons, for instance, they "generally cannot speak, and often cannot think."

This doctrine of "double government" is such a misperception of the facts of British political experience that it can be accounted for only on the grounds of overriding theory. For certainly a leading trait of the British polity with regard to institutions, classes, and political culture has been not the separation, but the fusion of the dignified and the efficient, the deferential and the pragmatic, the traditional and the modern. If we can believe Tocqueville, Taine, Mosca, Schumpeter, and Lowell, the role of the traditional upper classes in the 19th and 20th centuries was not only to attract deference, but also to supply leadership. And generally Britain survived with relative success the various crises of modernization in large part because of the fusion of elements whose separation would have promoted institutional deadlock, class conflict, and civil violence.

Bagehot's Concept of Tradition

The immediate source of Bagehot's misperception was his concept of tradition. We can get a clearer view of the ultimate theoretical basis of this idea if we look at *Physics and Politics* (1872), a work that elaborates the theory of political development presupposed by *The English Constitution* and briefly sketched in its concluding chapter. In the later book, Bagehot's central concern was the founding and development of nations. (The title of the German translation was appropriately *Der Ursprung der Nationen.*) The term he used to identify this concern was "nation-making." And given his premises, the making and maintenance of a nation must indeed have appeared as something of a problem.

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* Walter Bagehot, *The English Constitution*. With an Introduction by R. H. S. Crossman, M.P. (New York: Fontana Paperbacks, 1963. Pp. 312. $1.50.)

[1] Alastair Buchan, *The Spare Chancellor: the Life of Walter Bagehot* (London: Chatto & Windus, 1959), p. 186.

[2] Page numbers given in parentheses refer to the Fontana edition.

Early man, according to Bagehot—and this is a crucial assumption—was a creature of unpremeditated impulse, neither social nor rational. In the struggle for existence, however, that group tended to survive which acquired a cohesive polity based on rigid identities of behavior—a "cake of custom." The origin of any cake of custom was imitation, which Bagehot regarded as instinctive. By "chance predominance," to use his Darwinian language, a model of behavior appeared, was imitated and hardened into custom. Once order had been established by such a nation-making tradition, the way was open for the rise of rationality, first embodied in "government by discussion," then in science and business.

Tradition, so conceived, is rigid, repetitive, unreflective, an "hereditary drill"[3] handed down from past generations. Intrinsically, it is opposed to rationality, which operates by eliciting and propagating new ideas. Given this fundamental opposition, the institutions, classes and norms of political culture produced by tradition and reason respectively could never be fused. And the efficient and dignified parts of the constitution must remain separate.

This wrongheaded view of tradition accounts for the error in Bagehot's main thesis in *The English Constitution.* But it has a further theoretical background that is also illuminated by *Physics and Politics.* Bagehot revealed the premises of his analytic thought in the chronology of his developmental scheme. His portrait of early man showed where he agreed with and where he dedeparted from the Liberal creed. With the Liberals he shared a deep-seated individualism. By this I mean the view common at least since Hobbes and Locke that social groups consist of separate, isolated egos, related to one another only externally, not internally. According to this Liberal view, individuals modify their behavior by rational calculation, as when by contracts, individual or social, they create cooperative arrangements. Such relations with others, however, do not change the inner man, his purposes and motives: they change only the means by which he pursues these purposes or expresses these motives. Still, for conventional Liberals such systems of rational calculation and conscious self-control sufficed as the basis for order. Thus, in the polity they trusted to the outcomes of "government by discussion" to produce stability and progress, just as they trusted to analogous processes in the free market to produce economic equity and growing wealth.

In much of what he wrote Bagehot displayed the same confident rationalism. For him the great engine of progress was "government by discussion": it had enabled some men first to break out of the fixed polity and then to develop the remarkable powers of the modern state. But Bagehot was not just an unsolemn version of John Stuart Mill. Departing sharply from Liberal convention, he saw that its bland rationalism did not and could not account for political order and that in the making and maintenance of nations another powerful force was at work. He characterized this force in his vivid and irreverent way variously as "dignity," "deference," "stupidity," and so on, but it comes down to some aspect of the national tradition. Tradition gave "dignity" to institutions, established "deference" between classes and maintained "stupidity" as a body of unquestioned norms and beliefs. He saw the immense power of this nation-building force over emotion and motivation, while the conventional Liberal could only lament its influence and neglect its political function. When Bagehot tried to understand tradition, however, his individualist prepossessions permitted him to see it only as an external relation among isolated egos. The best he could do was to call it imitation and account for its power by postulating an independent instinct.

The Menace of Rationalization

This analysis shows the connection between two curious aspects of Bagehot's writing: his insistence on the separation of the dignified and efficient parts of the constitution and his fears of political disorder. The key is his conception of tradition as imitation. This conception fits with, although it is not a necessary consequence of, his atomistic view of society. To look at tradition in his way is to set it in fundamental opposition to rationality. As Bagehot wrote: " . . . discussion and custom cannot be . . . combined; their 'method' as modern philosophers would say is antagonistic."[4] Since imitation is based on instinct not reflection, once an item of imitated conduct is subjected to rational examination, it loses its imitative character.

This radical incompatibility means that, although a single polity may contain at one time elements of rationality and tradition, they can only be mixed or balanced in some mechanical way, but not combined, or fused in institutions, in the role of a governing class or in the norms of a political culture. In an appropriately mechanical image, Bagehot compares the effect of the dignified parts on the efficiency of the English polity with the "solely-ornamental wheels introduced into the clocks of the Middle Ages, which tell the then age of the moon or the supreme constellation; which make little men or birds come out or in theatrically." "All such ornamental work," he notes, "is a source of friction and error" (p. 207).

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[3] *Physics and Politics* in *The Works and Life of Walter Bagehot,* ed. Mrs. Russell Barrington, 10 volumes (London: Longmans, Green, 1915), VIII, p. 18.

[4] *Physics and Politics,* p. 112.

The process of rationalization is, moreover, irreversible. Rationality can take over subjects from tradition, but tradition cannot reclaim them. "Once effectually submit a subject to that ordeal [i.e. discussion]," he writes, "and you can never withdraw it again; you can never again clothe it with mystery; or fence it with consecration; it remains ever open to free choice, and exposed to profane deliberation."[5] In Bagehot's theory once a society is launched on modernity, rationality and its products will inevitably crowd out and ultimately extinguish tradition and traditionalism. This inexorable process of rationalization erodes the great foundation of order and threatens to release the old barbarous passions. Looking at his world from the vantage point of these prepossessions, Bagehot could not fail to fear Disraeli's bold extension of the suffrage. The same theory that distorted his perception of the constitution also exacerbated his fears for its future.

The errors resulting from this theory were numerous: the inability to perceive the fusion of reason and tradition in British political life; the conclusion that tradition must be continually wasting away under the assault of reason; the implication that the bonds of order represented by tradition cannot be replenished in the modern age. The essence was a quite extraordinary failure to understand the nature and strength of the modern nation-state as a political community.

Misperception of the Cabinet

Bagehot's individualist perspective also distorted his perception of the Cabinet, whose nature and functioning in certain vital respects he did so much to reveal. The lasting power of his analysis of the Cabinet is not the mere empirical discovery of the fusion of executive and legislative powers, but rather his understanding of why this fusion is a necessity of modern government. His premise is an appreciation of the distinctive character of government in modern times. This is an age when policy must be "continually changing" (p. 70). In attempting to meet this constant need for change, the men in control of government may not be able to agree. In particular, if those who make the laws, levy the taxes, and authorize the expenditures are different from those who execute the laws and collect and spend the revenues, there will be disagreement and conflict between them. "The tax imposers are sure to quarrel with the tax-requirers. The executive is crippled by not getting the laws it needs, and the legislature is spoiled by having to act without responsibility . . ." (p. 70).

A more confident rationalist would have trusted to the mechanisms of government by discussion to resolve such differences. Bagehot, on the other hand, saw the fusion of executive and legislative powers as a necessary condition, if British governments were to rule with some degree of coherence. In explaining this fusion, he stressed the norms of collective responsibility and the ties of party. What he refused to consider in his brilliant analysis was the role of tradition. Clinging obsessively to his doctrine of "double government," he argued at length that the Cabinet belonged to the efficient part of the constitution and was not substantially affected in its operation by the dignified parts, the Monarchy and House of Lords.

What this argument obscures is, in the first place, the effect of Britain's monarchic past upon the origin and rise of the Cabinet. As L. S. Amery observed,[6] the British constitution has always had two elements: an initiating, directing, energizing element—formerly the Monarch, and nowadays the Cabinet—and a checking, criticizing element —formerly Parliament as a whole, and nowadays especially the Opposition. This bipolar conception of authority, arising in the early days of the British polity, survived as a force shaping the development of the Cabinet. In sociological language, one might put the point by saying that the monarchic role was assumed by the Cabinet, endowing this plural executive with norms of collective responsibility. It made a vast difference that this plural executive was the offspring of a unitary executive. Serving a common master, ministers in the early Cabinet learned to act in unison before they had freed themselves from that mastery. Before their responsibility to Parliament was established, ministers were taught by the King the responsibility of taking the initiative in policy. Owing their office to a chief because he was in fact the King's first minister, they learned that their tenure depended upon his tenure well before he became the nominee of a party. In short, while the Cabinet was the core of the efficient part of modern British government, it could not have performed this task if it had not inherited a code of conduct from the dignified past. Here as elsewhere tradition was so far fused with rationality as to constitute a necessary condition for the effective operation of rationality.

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[5] *Ibid.*, p. 104.

[6] *Thoughts on the Constitution*, 2nd ed. (London: Oxford University Press, 1953).

The distortions of Bagehot's doctrine of double government are even more serious if one looks toward future rather than past development. For an essential element of his view is that the efficient parts of the constitution cannot acquire "dignity." This conclusion follows logically from the radical opposition he sets up between reason and tradition. In conformity with this theory, the Cabinet is portrayed as an agency of purely instrumental significance, lacking in "theatrical show," and incapable of arousing reverence as an institution.

This is a wrong reading of the facts then and now. Feelings about the Cabinet include more complex elements than merely a clear-eyed assessment of its instrumental operation. Certainly, that is true of the members of the Cabinet themselves. An ex-member recently described how ministers, while gathering at No. 10 Downing St., laugh and joke and call one another by their first names, as one would expect of party colleagues. But once they enter the Cabinet room, and the meeting starts, "the whole atmosphere changes." Members now address their remarks only to the Prime Minister, and even the oldest friends refer to one another in the third person and by their ministerial titles. For "here is no longer a set of individual men, but the collective sovereign power of the state."[7] The Cabinet is "not just a meeting of persons, but a continuing body, that has for centuries been the seat of ultimate authority."[8] Such sentiments, I submit, are what Bagehot meant by "reverence."

Fusion of Tradition and Reason

My argument has been largely negative. The main thrust has been to show that Bagehot's theoretical presuppositions—especially his Liberal individualism—distorted his analysis of British government and politics. His error is also of general interest to political science. The rigid separation of tradition and reason of which Bagehot was guilty still menaces thinking about modernization and political development. The typologies of even the great Max Weber tend to propagate this distortion. The consequent misunderstandings also often color judgments on present day society, leading critics who are fearful of modernity and its effects on social cohesion, to take a far more pessimistic view of our plight and prospects than the facts warrant, just as Bagehot did a hundred years ago.

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[7] Patrick Gordon Walker, *Encounter*, 6 (April, 1956), p. 19.

[8] Walker, *The Cabinet* (London: Jonathan Cape, 1970), p. 106.

BOOK REVIEWS

Youth in Transition, Vol. I: Blueprint for a Longitudinal Study of Adolescent Boys. By Jerald G. Bachman, Robert L. Kahn, Martha T. Mednick, Terrence N. Davidson, and Lloyd D. Johnston. (Ann Arbor: Institute for Social Research of The University of Michigan, Third Printing 1972. Pp. xii, 269. $8.00, cloth; $5.00, paper.)

Youth in Transition, Vol. II: The Impact of Family Background and Intelligence on Tenth-Grade Boys. By Jerald G. Bachman. (Ann Arbor: Institute for Social Research of The University of Michigan, 1970. Pp. xvi, 289. $7.00, cloth; $5.00, paper.)

Youth Look at National Problems: A Special Report from the Youth in Transition Project. By Jerald G. Bachman and Elizabeth Van Duinen. (Ann Arbor: Institute for Social Research of The University of Michigan, 1971. Pp. ix, 98. $2.00, paper.)

Youth in Transition, Vol. V: Young Men and Military Service. By Jerome Johnston and Jerald G. Bachman. (Ann Arbor: Institute for Social Research of The University of Michigan, 1972. Pp. xiv, 254. $8.00, cloth; $5.00, paper.)

The Youth in Transition Project (YIT) was undertaken in 1965 to chart longitudinally the social and psychological development of a cohort of male high school students. Funded principally by the United States Office of Education, and additionally by the Departments of Labor and Defense, the project has sought to assess the causal linkages between the objective environments of an individual and his physical and mental states. The publications listed above constitute approximately half of the output of the project to date.

Volume I provides an overview of the longitudinal design employed by the researchers and a justification for their selection of particular instrumentation. Data were collected by interview, written questionnaire, and standardized test for a nationwide stratified cluster sample of approximately 2200 boys beginning in the fall of 1966 as all were entering tenth grade. Categories of inquiry included, among others, the aptitudes and abilities of each subject, his motivations, affective states, values and attitudes (including a limited number of expressly political items), family and peer relationships, and plans and expectations for the future. This information was supplemented by questioning of principals, teachers, and guidance counselors in the schools attended by the subjects, and by an evaluation of community characteristics. Test items and survey instruments are included in a lengthy set of appendices.

Volume II presents the first substantive results of the YIT inquiry. Since longitudinal analyses were not yet completed at the time of publication, the volume emphasizes the relationships among selected variables at Time 1. Socioeconomic level is found to be the strongest predictor to such dependent variables as intelligence and ability, school grades, college plans, occupational aspirations, feelings of efficacy, and political knowledge. In addition, family size and home life, religious preference, and race are also assigned some explanatory power. The findings on race are particularly interesting since the evidence presented indicates that most of the variance between racial subgroups may be attributed to regional and socioeconomic differences among the subjects. This point might have been made more conclusively, however, had the authors chosen to include a comparison of poor, rural, Southern blacks with poor, rural, Southern whites.

Volume V in the series, which was published some two years later, represents an attempt to apply the YIT informational base to a particular policy problem, the development of an all-volunteer military force. Drawing upon their data on personal and social attributes, the authors assess the motivations for and likelihood of enlistment in the armed services, and the temporal changes in preferences for the military and for other occupations. They conclude that there is no particular "military type" with a distinctive social or psychological profile, but that because there is, in effect, a "college type" (higher SES, more skills and accomplishments) who self-selects out of the pool of potential military manpower, an unrepresentative segment of the grade cohort is ultimately available for enlistment. This finding has implications for the timing of the recruiting effort, the type of incentives which may prove effective in recruiting a larger number of highly competent young men, and the nature of the relationship between recruiters and school personnel.

The final YIT offering to be reviewed here, the special report on youth and national problems, may be of most direct interest to political scientists. This monograph provides an overview of attitudes among the YIT cohort on such issues as Vietnam, national unity, racial tensions, crime, pollution, and population control. While taking advantage of some of the longitudinal data developed by the project, the report is based primarily upon the final data collection of the series, which occurred during the summer of 1970. Although the analysis is at times rather trivial and fails to identify the sources of variation in young men's attitudes, one significant (though not surprising) conclusion does emerge: Treatment of the Vietnam and population issues in particular suggests that an individual's perceived social distance from an issue or problem influences not only the salience which he attaches to his related attitudes, but the direction of those attitudes as well. Thus as the YIT cohort approached high school graduation, not only did the Vietnam War become more important, but attitudes toward it became more unified and increasingly negative. In contrast, attitudes on the more remote (for high school boys) yet highly publicized issue of population control were generally more diverse and frequently inconsistent with one another.

Without question, the Youth in Transition Project has developed a large and diverse set of data which offer much promise for many lines of inquiry. Project variables encompass attitudinal, experiential, behavioral, and environmental phenomena within a highly functional developmental framework. But at the same time, both the project itself and its various publications suffer from several fundamental defects which may seriously compromise the realization of this potential.

To begin with, the authors are aware of, yet somehow insensitive to, differences in the scalar qualities of their data. In particular, correlation coefficients (*r*) are frequently reported for nominal and ordinal data; and when interval data are available, the correlation ratio (*eta*) is employed almost exclusively. While this latter procedure may be justified when the assumption of linearity appears inappropriate, it must be noted that *eta* is not sensitive to ranking, and therefore may not always prove an adequate substitute for Pearson's *r*. In addition, the format selected for the numerous graphs in Volume II tends visually to minimize the dispersion of the data, and may be somewhat deceptive. This "creative" use of statistics constitutes a disservice to the unsophisticated reader.

These statistical indelicacies are accentuated by the authors' penchant for attributing great significance to relatively inconsequential results. In point of fact, very few of the bivariate relationships reported in these volumes account for as much as three per cent of observed variance, and multiple classification analysis using *eight* principal independent variables rarely accounts for even twenty per cent.

Related to this statistical insensitivity but even more fundamental is the authors' rather unsophisticated approach to the construction and evaluation of indexes. Numerous attitudinal scales based upon ordinal responses have been assigned interval scores (means) and treated as interval variables. No efforts have been reported to assess either the validity or the reproducibility of these indexes. At the very least, the authors should have tested for and reported the degree of unidimensionality of their scales, or should have clearly indicated that they did not assume organization around a single factor. Implicit adoption of the latter position may help to account for the relatively low levels of explained variance.

The one variable cluster which does *appear* to have considerable explanatory power is the index of socioeconomic status. Close inspection reveals, however, that the components of this index have in fact been selected not because they accurately reflect the dimensions in question, but precisely because they are the best predictors. Thus the development of the index is circular, and, the authors' appended protestations to the contrary notwithstanding, the reported results are in fact misleading.

Finally, and more substantively, the authors fail in these works to develop adequately their theoretical perspective or to assess the relationships between their own findings and those of other researchers. They offer simplistic explanations for several of their observations (e.g., the increased dissatisfaction and distrust of government noted in the cohort over time is attributed to a parallel increase among the general population), and virtually overlook the significance of others. Indeed, one might characterize the authors as literally wallowing in data, engulfed in so much information that they are unable to organize it in any nontrivial fashion. Thus the weakness of their analysis may be a function of the richness of their data, and the true lesson of YIT may be that manageability must be a central consideration in the planning of any such large-scale research. Despite this organizational shortcoming, YIT data should prove a veritable cornucopia for secondary analysis.

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The Politics of Authenticity: Radical Individualism and the Emergence of Modern Society. By Marshall Berman. (New York: Atheneum, 1970. Pp. 320. $8.95.)

This book is a model of a new kind of scholarship, passionate or committed scholarship. Its author attempts to look at the past by way of what he believes to be the most important issue of our times: the repression of the self by the system. The book, according to its author, comes at a favorable moment when one can again after the disappointments of Stalinism hope for a political solution to the problem of the self, a moment when political and self-consciousness can go hand in hand. The approach to the works he interprets is determined by his view of the world: Marxist in its revolutionary appeal and in its economic understanding of the sources of alienation and false consciousness; Freudian in its apparent assertion that sex is the backbone of the self; existentialist in its treatment of the self and its concentration on authenticity; above all New Left in its commitment to unprogrammatic political change and in having as twin goals freedom, understood to mean being and doing whatever one wants to be or do, and community. In short this small book bears the weight of all the most dazzling intellectual positions of the day. Whether it can make them coherent is another question. And even if, improbably, this question were answered in the affirmative, it would further have to be asked whether this reading of Montesquieu and Rousseau does justice to the integrity of their teachings.

This issue of the adequacy of the interpretations becomes pressing from the beginning of the book with the presentation of Montesquieu, which is a sort of preface to the much fuller treatment of Rousseau. Berman speaks only of the *Persian Letters*, which he asserts, without serious argument, to be in contradiction with the *Spirit of the Laws.* He concentrates largely on the erotic themes connected with Usbek and his harem and draws therefrom that Montesquieu was criticizing the *ancien régime*, which is true, and that he was a radical individualist, which is not true, at least in the sense intended by Berman. Berman is at his strongest in characterizing the ways in which certain kinds of society enslave and manipulate men; at his weakest when he tries to elaborate what either his philosophers or he himself thinks is the good or nonalienated society. This weakness is not fortuitous; it stems from his religious devotion to the self, undefined, undefinable, unlimited and unlimitable, the source of all value and all right. Berman knows what is bad—alienation—but he does not and cannot tell us what is good. With Montesquieu, all to the contrary, a clear view of what is good, what man's nature and natural needs are, is elaborated, and his satire is based on the contrast between established practice and natural right. Berman simply adopts the critical part of Montesquieu's teaching, because it can be misread to imply that anything goes, that any constraint of inclination is tyranny, and rejects as a conservative loss of nerve the positive teaching about what is legitimate behavior and what is not. Montesquieu's teaching concerns civil freedom, not authenticity, for a being that has a nature, not a self.

Berman's mode of interpretation is most pernicious in his treatment of Rousseau. He divides that treatment into three parts: Rousseau on alienation; his politics of authenticity; his politics of inauthenticity. Alienation is in turn divided into two parts, that in traditional society and that in modern society. This division is Marxist in inspiration, based on the different human effects of property relations in feudal and bourgeois society. It is not totally foreign to Rousseau, for he was the first to use the word *bourgeois* in its contemporary meaning, and he described with unrivaled eloquence the differences between aristocratic and bourgeois societies and men. But Berman is totally misleading in giving the impression that property relations are responsible for alienation in Rousseau's teaching. No new society, no upheaval in the modes of the distribution of property, can overcome the tension in man between living for oneself and living for and in terms of others. In an example of his always loose reading of books, Berman gives the impression that in the *Discourse on the Origins of Inequality* Rousseau makes property the source of "inauthenticity," of the distortions resulting from *amour-propre.* This is not so. Men's hostility to one another and their dishonesty to themselves and others began prior to the establishment of anything that could be called property. It is in the nature of man, according to Rousseau, to care only for himself; and when he gets into contact with other men, when he needs them, he must act as if he cares for them and can be useful to them; and thus he begins to define himself in relation to them. This is a contradiction in the *nature* of man's individuality in relation to his sociality. There is a profound difference between Rousseau and Marx on this point, and it explains much of the difference in their solutions to the political problem. For Rousseau, the problem is ultimately in man, not in society. Thus Berman's entire discussion of *amour de soi* and *amour propre*, the key terms in Rousseau's psychology, is, to say the least, defective.

Rousseau does, so to speak, nothing but explain what the self is and is not; but so soon as one does that, certain kinds of belief and conduct must be praised and others condemned. In other words, limits must be set. Rousseau's goals are the natural man and the moral man, not the authentic man. The authentic man, for Berman, is one who does whatever he wants, the inauthentic man the one who does what others or the system wants. For Rousseau, to be sure, a man can be free and whole only if he does what he wants; but he must also want what is in accordance with his nature; freedom without limits imposed by nature or moral law leads only to the worst sort of alienation. Berman manages to hide and distort Rousseau's position by saying he was seeking for authenticity; then, like a magician in a circus, he cuts Rousseau in half; what was once an organic whole becomes two distinct personages, the authentic Rousseau and the inauthentic Rousseau, the former freeing from all constraints, the other imposing the worst of all tyrannies. To perform this work of magic, Berman must take books that are thought-out unities and treat them as though they were written by Dr. Jekyll and Mr. Hyde taking turns sentence by sentence.

Berman's discussion of Rousseau's authentic side is platitudinous, admiringly trotting out all the things from which Rousseau wanted to liberate man. Then, with the ferocity of a hippy Savonarola, he turns on Rousseau for betraying the erotic faith. Rousseau is accused of the most terrible crimes—believing in original sin, suffering from sexual neurosis, being a dropout. In a breathtaking peripety, Rousseau becomes the most totalitarian of totaliarians, not so much the source of the totalitarianisms of the Terror or the Soviet Union, but of that most terrible of totalitarianisms, modern liberal democracy, e.g., the United States.

I shall leave to the reader the excitement of seeing how this comes to pass, but something must be said about the source of Berman's fury. It begins in the limitations Rousseau puts on sex. Berman takes it for granted no harm can come of any form of indulgence whatsoever, so long as it comes from an authentic self. He pays no attention to Rousseau's subtle arguments about the relation between sex on the one hand, and love, art and morality on the other. Rousseau attempts to establish, on the basis of man's fixed nature, what things can go together and what not. He sees in sex one of the prime sources of selfishness, dependence and exploitation of others, as well as one of the possible bases of love, society and happiness. This ambiguity, this notion that man might have something in himself to control and overcome, is intolerable to Berman. Similarly he refuses to see anything but hardness of heart in Rousseau's unwillingness to give way to Emile's tears, although Rousseau insists that tears are the first source of alienation, the root of the desire to master and of the sentiment of enslavement. Finally, the moral demands of a free society, the alienation of the particular will to the general will to avoid its alienation to the particular wills of other men, does not impress Berman. What Kant saw as the most sublime of all moral teachings, the only basis of freedom and the dignity of man, and which by way of him became the core of Hegel and Marx, is for Berman deception and tyranny. He has thought none of this through. Similarly, he takes Rousseau's preference for solitude as a "dropping out," a weakness of old age. But that preference was there from the beginning and is not the result of Rousseau's peculiar psychology. It has to do with the necessary, the intransigent, tension between human nature and society. The real satisfaction of man is in the sentiment of existence which can best be experienced alone, at the end as well as in the savage beginning.

The source of Berman's blindness and his sloppy reading is in his dogmatic presupposition that there is a harmony between the growth of the self and the demands of civil society. It may be so, but surely Rousseau is the most powerful critic of that view. He is, to be sure, the source of radical libertarianism, radical egalitarianism and the demand for a real community as over against the institutions of liberal society. But it is also the case that for him individual desire poses a problem for community. His reflection on the ancient city, which Berman regards as so dangerous, was Rousseau's attempt to see what a participatory community really is. What common good can there be for men of unique, individual selves? Letting go, which is really what Berman is selling, would, according to Rousseau, release not the expanding self but the passions of nature bloated by bourgeois society, engendering monsters of vanity lacking even the bad conscience which acts as a corrective within that society. Rousseau contains both the inspiration for and critique of the New Left. That is why he is so paradoxical to Berman. His commitment makes it impossible for him to study Montesquieu or Rousseau seriously and turns his book into propaganda rather than scholarship.

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Tocqueville. By Hugh Brogan. (London: Fontana Paperbacks, Collins, 1973. Pp. 95. $1.50, paper.)

It is difficult to decide for which public this short book has been written. For those who have studied Tocqueville's work, it does not contain one sentence which could be regarded as original, and for those who wish to be initiated to Tocqueville's thought, one must express a warning. Mr. Brogan measures Tocqueville by his own prejudices, which are at times either arrogant or journalistic.

He gives first some hints of what he terms, "the problem of Tocqueville." Here the worldwide Tocqueville renaissance is reduced to George Pierson's book, *Tocqueville and Beaumont in America*, but in fact the Tocqueville revival begins with Dilthey, the great German philosopher-historian, although this is not the place to review the history of the Tocqueville revival.

I cannot resist the temptation, however, to quote a few lines from Mr. Brogan (p. 10), where he deals with the Tocqueville renaissance:

> Tocqueville was also assisted by the boom in sociology. He was unquestionably a sociologist, and if he had a fault, it was a tendency to take himself too seriously. American sociologists, whom their worst friends have never found it necessary to defend against charges of

> frivolity, took him to their hearts; and as American political science followed along the path of solemn unintelligibility Tocqueville's writings were doubly combed for "insights," which, rendered down into jargon . . . could enhance the reputation and salaries of gentlemen with academic careers to make and academic families to feed . . .

This quotation gives the reader of this review perhaps an idea of Mr. Brogan's attitude towards his American colleagues. One wonders whether he has ever read Riesman or Nisbet, whose works are so deeply imbued with Tocquevillean ideas.

In two further chapters, Mr. Brogan appoints himself a supreme judge of Tocqueville's *Democracy in America.* Here he is not less condescending or insolent. He fails to understand the meaning of the "tyranny of the majority," which so deeply impressed minds like Burckhardt, John Stuart Mill, or later, Dilthey, Acton, and Ortega y Gasset. Mr. Brogan knows so little of sociology that he does not see that the "tyranny of the majority" is only another term for the phenomenon of the mass society, which was Tocqueville's primary concern.

Mr. Brogan writes at another page (p. 45):

> Unhappily his book [*Democracy in America*] was not rewritten, and from that day to this everyone with a grievance, real or fancied, against the American people, has found in Tocqueville a plausible, high-toned justification for complaining of tyranny. And the vanity of authors, who of course always deserve enormous sales, which only a conspiracy can cheat them out of, has been much soothed by his words. He himself was probably suffering from a professional hangover. *An Enlightenment writer, born out of his due time, he was hankering for the irresponsibility of the eighteenth century.*

I have allowed myself to italicize the last sentence in order to illustrate the miscomprehension of Tocqueville's mind. Few political scientists have so profoundly anticipated the future. Mr. Brogan regards Tocqueville as a belated eighteenth-century writer, completely ignorant that his basic approach was that of a jurist; hence his affinity and his rank, equal to Bodin or Montesquieu.

Of Mr. Brogan's appreciation of the second part of *Democracy in America,* two passages should perhaps be quoted:

> Tocqueville, reasoning from the speculative to the unknown, is boringly fanciful, and it becomes hard to forgive him for his solemnly pedagogical air. At other times some flash of insight, information or common sense will redeem him. The trouble is that it can never be predicted which way the worm will turn (p. 50).

How witty and elegant!

Another choice example:

> What is one to say of this mixture of shrewdness and the higher silliness, except that it is typical of *Democratie II?* (p. 51).

I do not know what "higher silliness" means, but I do know that *Democratie II* is regarded, without exception, as a classic of sociology.

After these examples I do not think it is necessary to discuss Mr. Brogan's treatment of the *Souvenirs* or the *Ancien Régime.* Yet even the *Ancien Régime* returns to the theme of the danger of the mass democracy. I quote only one passage, taken from the Preface:

> Since men are no longer bound to each other by links of caste, class, association or family, they tend only too easily to concern themselves with their private interests. They are always only too inclined to consider themselves alone and to withdraw into a narrow individualism where every civic venture is stifled. Far from fighting against this tendency, despotism makes it irresistible, for it takes away from the citizens every common passion, every common need, any necessity to understand each other, any opportunity to work together. It walls them up, as it were, in their private lives. They were already disposed to keep themselves apart; despotism isolates them; they become cold towards each other; despotism freezes them.

If one attempts to understand Tocqueville, his main works must be linked together. Both *De la Démocratie en Amérique* and his *Ancien Régime* are comparative studies in State administration. Tocqueville asks himself what are the consequences of centralized administration for the existence of the human being, and what can we or should we do to safeguard our threatened freedom. He may have been more explicit as an analyst than as a reformer, but his questions concern us today even more than ever, and Mr. Brogan's book contains little trace of them.

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Christian Political Theory and Church Politics in the Mid-Twelfth Century: The Ecclesiology of Gratian's Decretum. By Stanley Chodorow. (Berkeley: University of California Press, 1972. Pp. 300. $15.00.)

The present monograph deals with a subject of considerable interest to historians of medieval political and legal thought as well as to students of ecclesiastical institutions. The importance of Gratian's *Decretum* is granted on all hands, but, surprisingly enough, hardly any attempt has ever been made to understand it on its own terms or determine its immediate political significance. Chodorow's essay fills this gap in a commendable if not altogether satisfactory manner and argues persuasively for the dogmatic and methodological novelty of Gratian's work. Its central thesis is that the *Decretum* is not a mere compilation of canons or a piece of timeless legislative wisdom but rather a theological treatise of sorts, whose guiding principles echo the platform of the reform party which emerged during the early decades of the twelfth century, in the wake of the Investiture Contest, under the leadership of the Chancellor Haimeric, St. Bernard, Peter the Venerable, Gerhoh of Reichersberg, and a host of lesser figures. The ecclesiology propounded by the new curial powers emphasized the spiritual nature of the Church over against the worldliness of the Gregorian reformation while at the same time advocating a more conciliatory attitude toward the Empire in accordance with the compromise worked out in the Concordat of Worms (p. 17 ff.). Among the leaders of his generation, Gratian appears to have been the only one to think through the political implications of the "reform of the reformation" (cf. p. 27). Viewed in the perspective of the 1120s and 1130s, his *Decretum* may be described as an attempt to endow the Church, now free from the control of the secular authority, with a juridical constitution properly its own. Its basic thrust is revealed as much by its omissions as by its express statements. Contrary to earlier practice, secular codes of law (specifically those of Theodosius and Justinian) are never mentioned by name in the *Decretum* (cf. p. 15, 150, 209, 220); nor does Gratian appeal to the authority of Gregory VII whenever such an appeal could be construed as lending support to Gregory's followers (p. 202 ff.).

To the tensions implied in the dual nature of the Church as a mystical body and a juridical entity may be traced most of the ambiguities and inconsistencies that Chodorow claims to have uncovered in the *Decretum*. One wonders at times whether the alleged confusions are those of Gratian or of Chodorow himself. The discussion of the relation between divine and human law would have gained in clarity and sharpness if a more concerted effort had been made to link Gratian's views to the broader tradition of medieval political theory stemming above all from Augustine. The translation of *ius naturale* by "natural law" rather than "natural right" (p. 105) merely adds to the confusion and makes it that much harder for Chodorow to explain in what sense the legislator is, or is not, bound by his own laws (p. 135 ff.).

Students of political thought should be warned that the *Decretum* contains little or nothing by way of a general theory of Church and State. The canons devoted to this subject (cf. p. 211 ff.) deal on a more practical level with the manner in which the two powers were expected to coexist and cooperate within the framework of the Christian society of the Middle Ages. Faint indications here and there (e.g., pp. 85, 186) suggest that Chodorow shares the increasingly popular but highly debatable view that the roots of modern democracy are to be sought in medieval constitutionalism rather than in the egalitarian natural law doctrines of the seventeenth century. However that may be, Gratian himself was no democrat, as is evident from his stand on the place of the Pope not only at the head of the hierarchy but apart from it (p. 179) or on the use of the material sword to coerce heretics (p. 223 ff.). The reader is left with the impression that by providing the Church with a legal code adapted to its needs as a separate and independent juridical community, Gratian helped set the stage for the great confrontations of the following century. In light of this fact, one is inclined to agree that the *Decretum* is "one of the most significant works of political theory written in the mid-twelfth century" (p. 7). The issue on which further debate among scholars may be anticipated is whether or not the cleavage between the Gregorianism of the eleventh century and the reform theology of the early twelfth century is as deep as Chodorow's intriguing and highly plausible hypothesis makes it out to be.

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Size and Democracy. By Robert A. Dahl and Edward R. Tufte. (Stanford, Calif.: Stanford University Press, 1973. Pp. x, 148. $6.95.)

The dilemma of democracy, as Dahl and Tufte see it, is rooted in the ineluctable circumstance that every unit of government is constrained by its size (especially of population, but also of land area, natural resources, gross national product, and so on). If, as they posit, citizen effectiveness and system capacity are equally important criteria for judging the viability of a democratic polity, the intrusion of the size factor makes for democracy's dilemma: all other things being equal, the smaller the polity, the more will citizens act responsibly and competently in controlling public decisions, but the less will be the system's capacity to respond to citizen preferences. Citizen participation in collective decisions and the collectivity's ability to respond to citizen preferences are inversely related. As a result, the determination of a governmental unit's optimal size necessarily involves a tradeoff between the two criteria of democracy.

This theme Dahl and Tufte pursue with relentless logic and a good deal of empirical evidence. They pursue it at varying levels of governmental complexity—in terms of countries, of equivalent units within countries, and of local versus national units. They pursue it in terms of classical propositions about the optimal size of the polity, from Aristotle to John Stuart Mill, and they pursue it in terms of aggregate statistics and survey data. They introduce, where necessary or desirable, considerations of social and political processes, like representation, coalition politics, group conflicts, or party competition, that often confound the assumption of *ceteris paribus* so essential to theoretical clairvoyance. Where, then, do they come out?

When setting out on their investigation, Dahl and Tufte assumed, as had their predecessors, that it is possible to stipulate some "optimal democratic unit." The assumption proved illusory, and they conclude that "no single type or size of unit is optimal for achieving the twin goals of citizen effectiveness and system capacity" (p. 138). This is so because "democratic goals conflict, and no single unit or kind of unit can best serve these goals" (p. 138). If one were to follow the logic of each criterion to its extreme, citizen effectiveness would be maximal in a system of minimal capacity or minimal in a system of maximal capacity. Clearly, to avoid absurdities of logical inference, something has to give.

What has to give, according to Dahl and Tufte, is the theory of democracy, or at least those "sturdy simplicities of popular democratic thought" (p. 138) which lead to the conclusion that practice is wrong—undemocratic because it is not in line with theory. Rather, "democratic theory will have to leap ahead and not jump directly into the past, as 'new' or 'radical' democratic thought invariably seems to do" (p. 139). In a highly complex and interdependent world, "it seems to us doubtful that solutions will be found in theories formulated in and for profoundly different times . . ." (p. 139). So far, so good. But where do Dahl and Tufte take us from here?

Frankly, I do not quite follow their argument. An adequate theory of democracy, they say, must tackle "the neglected problem of political units and their inter-relations" (p. 139). Units of different size are needed—small ones "to provide a place where ordinary people can acquire the sense and the reality of moral responsibility and political effectiveness . . . "; and very large, possibly transnational units whose existence "will increase the capacity of the system to handle critical problems, and thus the collective effectiveness of a body of citizens . . ." (p. 140). But this surely does not solve the initial problem, as Dahl and Tufte admit: very large units "will also increase the ineffectuality and powerlessness of the individual citizen," and "finding space for truly significant activity in the very small unit is obviously going to be difficult" (p. 140).

What Dahl and Tufte seem to have in mind is some kind of transnational, democratic and federal system. But to say that federalism is needed, they argue, "is not so much to provide an answer as to restate the question, or, at best, to suggest a direction in which a part of the answer might be found" (p. 140). Federalism, they continue, "is not so much a theory of inter-unit relations as a set of institutions, different from country to country, conveniently anchored in the nation-state . . ." (p. 140). I am puzzled, for I have always thought of some form of federalism as at least one theoretical answer, *ceteris paribus*, to the problem of solving the relationships among interdependent units of different size.

Whither, then, is democratic theory to leap? One task, Dahl and Tufte suggest, "may be to specify not an optimal unit but an optimal number of units with comparatively fixed boundaries" (p. 141). And, leaping ahead fearlessley, the authors yet warn that "boundaries of each unit would be too small or too large for all functions assigned to it; but the costs of a small number of units with relatively fixed boundaries would be less than the costs of any larger number of units . . ." (p. 141). All of which leaves me quite perplexed, for as of last count I am living in some dozen units of varying size, and though reasonably competent in these matters, I find my effectiveness as a citizen is only matched by the incapacity of these units to respond to my preferences, not the least important of which is some reduction in the costs that stem from their multiplicity. Unfortunately, Dahl and Tufte do not provide any clue about how to fix an optimal number of units that would get me out of my predicament. How few are few, and how many are many units? How does specifying an optimal number of units differ from specifying the optimal size of a single unit? And would not the optimal number of units depend on the optimal size of the subordinate or superordinate units in which a given function is performed with least cost?

Another task of theory, Dahl and Tufte recommend, is "to clarify the powers, obligations, and characteristics of citizens and leaders, including officials, in democratic units of different types" (p. 142). As specialization is related to size, leaders in large units even now specialize; and just as leaders specialize, "citizens must, and to some extent do, also specialize" (p. 142). But no sooner has this gutsy leap forward been made when we are informed: "Yet indefinite specialization, like indefinite proliferation of units, would lead sooner or later—and may already have—to so much fragmentation as to create insuperable problems of coordination" (p. 142).

I am sorry to be quoting so much, but I want to let Dahl and Tufte speak for themselves, and if I have quoted incorrectly or out of context or distorted their meaning, I will stand corrected. But, it seems to me, Dahl and Tufte's conclusions do an injustice to the main part of their book that is infinitely greater than any injustice that I might do to their epilogue. For *Size and Democracy* is an eminently interesting and elegant analysis of some critical issues of modern democratic theory. The book is studded with axioms, theorems, hypotheses, inferences and generalizations which can keep a bevy of political scientists happy in their empirical hunting grounds, and I cannot even begin to list some of the many fruitful leads to research that two fertile minds are able to extract from what is a theoretical analysis in the best sense of the term. I must leave it to the reader to find out for himself or herself.

HEINZ EULAU

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Two Concepts of the Rule of Law. By Gottfried Dietze. (Indianapolis, Ind.: Liberty Fund, 1973. Pp. 108. $5.00.)

In this concise, compact little book, consisting of two essays, Professor Dietze has examined the relationship between constitutionalism and the law of the state—between the Law State and State Law. Much of the book is devoted to aspects of German constitutionalism and the development of ideas in Germany, making it an important study of social movements and the "history of ideas." Moreover, although Dietze does not make it so explicitly, the book is extremely relevant for a United States trying to contend with an administration which, while it came to office emphasizing "law and order," has meant any order and any law, without regard to substantive content. Dietze tells us, for example, that dangers to freedom are greater when gradual, because there is less resistance, with the Law State (or Just State) being restricted by State Law.

One of the author's most important themes is that justice and injustice, State Law and the Law State, are matters of emphasis, not opposition. Just as State Law restricts the Law State, State Law is necessary to protect the Law State. Also, "without injustice, there is no justice" (p. 13), like Edmond Cahn's observation that we will never know justice except through a "sense of injustice."

The theme of the first essay—"The Just State and State Justice"—is that a Law State may be only formal in nature, making it susceptible to takeover by virtually any political ideology, including Hitler's. We must, Dietze suggests, keep in mind the liberal commitment which underlies the formal rules, so that the emphasis in our "rule of law" is not on "rule" or "order" rather than "law." Dietze says that while the German Basic Law provides much protection for freedom, a strong possibility exists that the "social" nature of the German Law State may dominate, because "the Just State has not regained its primacy over the state's own concept of justice" (p. 45), with constitutional government "as much determined today by the law of the Federal Republic as it was previously by the law of the Weimar Republic and Empire, i.e., by temporary beliefs" (pp. 45–46).

In the first essay, Dietze says strengthening of governmental power is to be feared in democracies as in any other form of government. But one also has to fear permissiveness, the theme he sounds in the second essay, "State Justice and the Just State," which is less balanced in tone. Here, except in talking about Germany, Dietze is at his most normative, showing his unhappiness with both democracy and American "permissiveness": "As a result of the New Deal and the New Frontier, there spread, under the Warren Court, a permissiveness . . ." making the 1960s "the most terrible period since the Civil War" (p. 62). He also argues that "there is inherent in each liberal the danger of undue permissiveness" (p. 67) because for the liberal the measure of all things is man.

The valuable point that we may "sentimentalize" away the Law State is damaged by overblown language about democracy as involving "the improper permissiveness that drifts toward anarchy under the pluralistic 'government' of an atomized society in which everybody can behave and misbehave according to the principle, *tel est mon plaisir*" (p. 54). Because of his fear of permissiveness or anarchy, Dietze is willing to tolerate much to preserve constitutionalism. Thus, while "To be defensible, actions in the name of the law must not endanger, but serve, constitutionalism," Dietze immediately defends the "dictatorial power of the Reich President" (p. 77), and justifies some authoritarian acts as necessary to protect constitutionalism.

Dietze is unabashedly a traditional political scientist, who defines our discipline as one which "emphasizes the restriction of power for the sake of justice," and thus is "sympathetic to the Law State" (p. 48), as well as "interested . . . mainly . . . in the freedom of the individual from the power of the state" (p. 56). However, neither this wishful and hopelessly outdated view of the discipline nor the further erroneous assertion that political science, "having become value-free" and having succumbed to formalism "will probably be as unable as politics to withstand the democratic tide" (p. 49) interfere with Dietze's excellent analysis of political ideas and the development of constitutionalism which is well worth reading because of its superior quality.

STEPHEN L. WASBY

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Administration: The Word and the Science. By Andrew Dunsire. (New York: Halsted Press, 1973. Pp. x, 262. $8.75. First published in 1973 by Martin Robertson and Co. Ltd., London.)

Dunsire—who is Senior Lecturer at the University of York in England—is faithful to his book's title. That title promises to dig deep into "administration" as word; and it implies a broad survey of administration as science. Nearly one-fourth of the text is devoted to "administration" as word; and Dunsire also seeks to capture the essence of administrative science and practice in Europe and America, roughly from the 16th century onward.

Given the range of professed coverage and goals, diverse and perhaps kaleidoscopic reactions to the book may be expected. For example, some readers may come away from the book feeling that they have more knowledge about the "word" than they had any prior need for, or can ever hope to put to any future use. Some readers also may conclude that the volume's sweep is too broad and brief to be satisfying to those knowledgeable about the "science" as it developed in the several countries and eras surveyed by Dunsire.

My overall reaction is that the promise in the title is not only basically kept, but is kept well. Despite the awesome dimensions of the literature and experience he addresses, Dunsire deals with them effectively and persuasively, on balance.

To provide detail about the volume, this review will distinguish three clusters of chapters. Dunsire nowhere indicates he would be comfortable with such an arrangement, but the convention here is convenient.

In a first cluster of three chapters based largely on English experience, fourteen meanings of "administration" are distinguished and elaborated through p. 52, and a fifteenth is added on p. 199. This could prove heavy going, but Dunsire eases the burden by his comprehensive grasp of authors and topics. Thus I was variously rewarded for trying to keep the meanings straight. Illustratively, Dunsire highlights Jeremy Bentham's 1816 warning about falling into a politics/administration dichotomy, which nourishes those who know we could learn far more from the past if we paid appropriate attention. And Dunsire provides (esp. pp. 26–33) a review of important historical background for the generalist/specialist debate that is always a central issue, and now perhaps more than ever. Only occasionally does the associational richness seem to detract from or exist independently of the argument, as it did for this reader in Dunsire's care to note (pp. 77) that Lorenz von Stein—"the greatest name in nineteenth-century German Administrative studies"—had "no connection with the Freiher, Carl Vom Stein."

With the "word" in hand, Dunsire devotes a second cluster of five chapters to tracing and analyzing the development of the "administrative sciences" in various countries and eras. The range of coverage of these chapters can be suggested economically:

- the developing administrative sciences in England are contrasted with those in France or Germany, and their substantial differences are traced (p. 53) to the differential impact of "the centralising 'absolute monarchy' phase of European political development";
- the emphasis on bureaucracy, especially in France and Germany, is analyzed and related to the less absolutist nature of those states in the 19th century;
- the "American science of administration" is sketched, from Woodrow Wilson through the Simon and Dahl of the late 1940s;
- the "new administrative science" which developed in the third quarter of the 20th century, especially in America and England, is reviewed;
- the "rise of comparative administrative" is highlighted as a development of the 1950s which Dunsire traces to "the outgoing generosity of American aid and technical assistance programmes" (p. 134).

This second cluster of chapters represents summaries-cum-syntheses and, although one can quibble about what they do not intend to do, they are typically very good at what they intend. But let me risk a quibble or two. I was disappointed by several such self-imposed tethers: "It is not within the practicable scope of the present book to present more than an outline treatment of . . ." (p. 102). The patent competence and comprehensiveness of that "outline treatment," in short, whetted my appetite for more. Similarly, I found that the chapter on the "new administrative sciences" omitted much recent behavioral and rational/technical activity that properly could have been mentioned, given the author's own definition of scope. For example, Dunsire refers to early humanistic approaches to organization analysis, like Maslow's. But he does not deal with the decade or so of subsequent research and experience with the "action research" or "organization development" to which those early approaches led, research and experience that have raised the expectations and also the blood pressures of many students and administrators.

Four essays about central differences in degree that are often seen as polarities constitute the final cluster of chapters. Dunsire directs attention, in turn, to administrative science and popular understanding, to public and "private" administration, to centralization and decentralization, and to theory and practice.

These chapters did not well sustain this reader's interest, for at least two reasons. First, much of the material is tied into the problems and prospects of public administration as a field of inquiry in England, with which I could not sustain a continuing identification, perhaps because its nuances escaped me. Second, the issues raised in those chapters are more intractable and elusive than the narrative sometimes allows. Consider (p. 201): "There is undoubtedly a gap between practitioner and theorist; but it is not expressible by a 'theory/practice dichotomy,' it is a mere communication gap."

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Antonio Gramsci: Life of a Revolutionary. By Giuseppe Fiori. (New York: Schocken Books, 1970, 1973. Pp. 304. $3.45, paper.)

"The only ambition of this book," its author comments in the Foreword, "is to complete the portrait of Gramsci" by drawing the "body and legs" in addition to the well-known "head" of a great intellectual and political leader. The result is an account of Gramsci which gives only minimal attention to the content of his political ideas. One can no more separate the head from the heart than the reverse, however, and this reviewer is left wondering how complete a biography this is of the remarkable man who was one of the founders of the Italian Communist Party and who in his philosophically erudite essays, written in prison under the most trying circumstances, produced the most telling criticism of vulgar and mechanistic Marxism yet to appear within the ranks of Marxism itself.

Antonio Gramsci was born in Sardinia in 1891 and died in 1937 shortly after having been released by the Fascists from a ten-year imprisonment. Because he was always in frail health, it is extraordinary that Gramsci could have survived ten years of incarceration as a political prisoner; this very confinement, however, served as the occasion for his most penetrating writing on such subjects as "theory and practice" in Marxist epistemology, the political and social condition of the Italian South, the role of Benedetto Croce in the Italian intellectual tradition, and a variety of other themes from literary criticism to the role of Henry Ford in American economic history.

Despite its limitations, Fiori's biography has many virtues, and one is grateful that an English translation is now available in a paperback edition. The author, a journalist, historian, and novelist, like Gramsci, is of Sardinian upbringing. Antonio Gramsci was highly conscious of having come from the "underdeveloped" Italian south. Fiori is peculiarly able to empathize with his subject; although he obviously admired Gramsci, he does not romanticize him. This is biography, not hagiography, and the book is written with restraint and taste and a scrupulous regard for the sources, and is never wordy or pretentious.

Still, at the end one is left with many questions unanswered (or perhaps even unasked). For example, why did Gramsci, who delighted so much in humanistic studies and who in his writings displayed a truly exceptional capacity for scholarly detachment, forsake the world of the academy for the political struggle and, having done so, willingly assume the role of a Communist party leader? Fiori provides us with some clues in references to Gramsci's early pamphlets (p. 101 ff.), but one wishes that the author had delved more deeply into the influence of the Italian revolutionary's university years upon his subsequent career. How could Gramsci, who, from the beginning of his association with Marxism and the Marxists, had always been the protagonist of "openness" and "dialogue" and had constantly opposed a coercive social order, bear to remain in the closed world of international communism particularly after the ascendancy of Stalin? In many respects he seems as out of place among Communist writers as do the intellectual dissenters in the Soviet Union today.

Fiori's biography, then, is more comparable to an artist's sketch than to a finished portrait. If, however, one reads it in conjunction with Gramsci's own *Prison Notebooks* and with J. M. Cammett's study of Gramsci's role in the formation of the Italian Communist party, one can readily see why Antonio Gramsci deserves to be regarded as one of the exceptional political minds of our century.

In 1947, Benedetto Croce, the Italian philosopher whom Gramsci regarded as at once an inspiration and his worthy opponent, had occasion to review the first two volumes of Gramsci's posthumously published prison writings. Croce praised his fellow southerner for his "openness toward truth regardless of its source," "scrupulous precision," "equanimity," "kindness," warmth of feeling and lucidity of style (Croce, in *Quaderni della Critica* [July 8, 1947], p. 86–88). The arduous life of Antonio Gramsci demonstrates once more that the human spirit lives on long after dictatorships seeking to imprison and destroy it have vanished.

DANTE GERMINO

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Political Obligation. By Richard E. Flathman. (New York: Atheneum, 1972. Pp. 334. $12.95.)

Professor Flathman's book breaks new and fruitful ground in the study of political obligation, a concept which not so long ago was on one side of the Atlantic virtually discarded as a linguistic anachronism, and on the other treated simply as a behavioralist phenomenon. Professor Flathman himself draws on both of these recent traditions in political thinking. They provide him with tools of analysis and methods of approach which, after re-examination and reshaping, can be used to gain a more positive and penetrating insight into the concept of political obligation than these two traditions offered.

The central theme of Professor Flathman's book is that "the practice of political obligation operates only if men *choose* or *decide* to do or refrain from doing certain *actions* because they believe there are *good reasons* for accepting and obeying or rejecting and disobeying rules that require or forbid these actions" (p. xv, author's italics). Those who disobey the law through habit, training, fear of punishment or expectation of reward are not engaged in the practice of political obligation, since it is the nature of obligation that it has to be assumed by free choice after rational reflection.

Flathman argues that the notion of obligation also requires disinclination on the part of those who assume it to act in the way required. In support of this claim he asserts that there would be no need for a society to have any rule unless there were evidence of "substantial and continuing disinclination to act in the manner prescribed by the rule" (p. 163). Two objections may be raised to this. First, there almost certainly are (and logically and conceptually there certainly could be) rules which no members of a community feel any disinclination to disobey, but which are stated and understood as regulations that the authorities would seek to enforce in the event of noncompliance. Such rules may indeed be laid down not because they are needed to secure compliance on the part of persons disinclined to conform to a required form of behavior, but because that form of behavior itself is held to be so vital to the well-being of the community that it needs to be given the status of a law. Second, Flathman's assertion that no rule would be made by a society in the absence of "substantial and continuing disinclination to act in the manner prescribed by the rule," cannot substantiate the thesis that no *individual* member of that society can be held by another member to have an obligation to act in accordance with the rule in the absence of evidence of disinclination so to act (p. 156). The weakness of Flathman's position can be seen in the way in which he handles the position of adolescents and persistent criminals. He recognizes both the *need* not to leave such persons without any obligation to obey societies' rules, and the *fact* that they cannot be said to be disinclined to obey rules they have never accepted; but instead of attempting to reconcile these two factors through a re-examination of the disinclination generalization, he simply asserts that adolescents and persistent criminals are "exceptions" whom it is "appropriate" to treat as having obligations to obey.

One of the points which Professor Flathman is concerned to stress is that political obligation as a practice cannot be established without reference to the content of particular regulations. He demonstrates that it is not possible to infer from the arguments in support of an obligation to accept the general rule "obey the law," an obligation to comply with any specific law. He further argues that reliance on the general rule to secure compliance with trivial legal rules is liable to weaken the force of the general rule as a conduct-guiding concept. While he accepts the force of Hanna Pitkin's thesis that it is part of the meaning of law that those to whom it is applicable are obligated to obey it, he denies that it is necessarily meaningless to discuss the question "Why do I (ever) have an obligation to obey the law?" This question is not a semantic puzzle which only permits of linguistic unscrambling. As Flathman shows it is logically and conceptually possible to question the fundamental rules and conventions of any or every known society. Further, he points out that a purely semantic interpretative answer to the question "Why do I (ever) have an obligation to obey the law?", can never in itself provide a *reason* for obedience. That is not to say that reasons cannot be adduced in support of the general rule, "obey the law"; reasons which Flathman considers at length in a sympathetic and fruitful analysis of Socrates' *Crito*.

Professor Flathman is much more critical of consent theories of political obligation. He insists that it is not possible, without undermining the very notion of consent, to hold a person to have given his consent, whether express or tacit, to any action or arrangement unless he knows, intends and communicates what he does. These necessary conditions of consent lead to the embarrassing result that few persons in any society can be shown to have ever given their consent, and that in consequence consent theories of obligation render virtually all governments illegitimate. This conclusion seems unwarranted, at least with respect to those societies which permit their adult members to choose their governments from among a range of contenders, including those who stand for change in societies' basic arrangements and principles. It is far from clear that fewer persons could be shown to have a political obligation by consent to the laws of the United States, for example, than the number having an obligation under the requirements laid down by Professor Flathman.

LESLIE J. MACFARLANE
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Jean Bodin and the Rise of Absolutist Theory. By Julian H. Franklin. (London and New York: Cambridge University Press, 1973. Pp. 113. $8.95.)

Julian H. Franklin has established himself as one of the foremost authorities on the complex and notoriously obscure thought of Jean Bodin. In this slim but weighty study, Professor Franklin's great intellectual powers and prodigious scholarship are brought to bear in a careful and detailed analysis of the development and substance of Bodin's doctrine of "*puissance souveraine*."

Most textbook discussions of Bodin's absolutism tend to assume that his notion of sovereignty was continuous between the *Methodus*, published in 1566, and the *République* of 1583. This view obscures the fact that in the latter work Bodin had abandoned the theory of sovereignty presented in the *Methodus*. The notion of limited sovereignty developed in the *Methodus* largely drew from and was supported by certain persistent medieval ideas which, in the climate of the Renaissance, were generally favorable to the development of constitutionalism. Franklin contends that a coherent absolutist doctrine did not predominate in the sixteenth century, but appeared rather late with Bodin's formulation.

This is not to suggest, however, that the idea of absolute authority did not exist prior to the publication of the *République*. It was in fact a universally accepted idea in French law. Yet it was situated in the context of the notion of the commonwealth as a *corpus mysticum*. Franklin argues that the often immoderate rhetoric of some of the commentators was in fact a means of expressing the cohesion of the commonwealth through a glorification of its head. Nevertheless, the theme of their discussions of, for example, the sovereign courts and the Parliament of Paris, was the limitation of the king's power; it was a theme embedded in the French political tradition. The commentators wrote against the background of a centralized administration and the medieval principle by which the king must govern with consent. Thus, despite absolutist imagery and language, the commentators of the early sixteenth century were delicately poised between monarchist and constitutionalist ideas. Though such opinions came to be more prudently and more timidly expressed at the height of royal power, around the middle of the sixteenth century, they did persist and were reasserted in the 1570s. Only against this background, Franklin suggests, can one understand Bodin's early theory of sovereignty and appreciate the significance of his shift to absolutism.

The theory of sovereignty developed in chapter VI of the *Methodus* represents a "mature and reasoned conception of the state" in which "absolutist elements are not only absent but are deliberately repudiated" (p. 23). Here Bodin does not depart significantly from the French political tradition according to which the monarch was undoubtedly conceived as supreme, but within the law. He felt that his later discussion did not differ from the constitutionalist perspective maintained in the *Methodus*, but was merely an elaboration of that position. Franklin shows that the turn to absolutism "was not a direct natural outgrowth" of the theory contained in the earlier work; it was a "sudden and drastic shift." And in a period in which political and religious positions were inextricably entangled, Bodin's concern and focus were distinctively political.

The point on which his later doctrine turned was the question of resistance. It was a direct response to the tumult surrounding the Huguenot party and its allies and the subsequent revolutionary movement occasioned by the massacre of 1572. To the emergence of a doctrine of resistance developed by Huguenot theorists such as Beza, Bodin responded by claiming that a "monarchy . . . is either absolute and immune from resistance pursuant to law, or it is not a monarchy at all" (p. 93). This was, Franklin argues, the core of his notion of absolute authority. It was "not that the ruler must always be obeyed, no matter what he ordered, but only that he could not be legitimately resisted" (p. 96). This meant that those "limitations" upon absolute authority which Bodin acknowledged in the *République*, the law of nature and *leges imperii*, were not enforceable by the community. Even if the king habitually and blatantly transgressed the law of nature, "his subjects were legally and morally enjoined from forceable resistance" (p. 93). In short, all institutional restraints which had been included in the first view and supported by the larger tradition were now without binding status. If the older tradition of discourse had favored a constitutional monarchy, Bodin's formulation clearly opened a "legal path . . . to autocracy."

In a time of intense political debate, Bodin's doctrine was only one of the competing theories of sovereignty. Those offered by the Huguenots and the later Catholic League were more empirically satisfactory; but it was Bodin's view which prevailed. The "sheer intensity of the ideological conflict in this period doomed the older tradition as a means of settling constitutional disputes" (p. 104). Bodin's doctrine was a response to this crisis and became widely influential (e.g., in Germany and England) as the general trend toward absolutism became dominant.

Franklin's latest contribution to the growing body of literature on Bodin is a work of impressive scholarship and reflects a sense of intellectual craftsmanship which seems to be rare these days. His sensitivity to the subtleties of Bodin's thought and the skillful use of materials provide us with a fresh understanding of one of the major figures in the current of culture and politics of the early modern period in France.

RANDAL H. IHARA

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Dialectic of Enlightenment. By Max Horkheimer & Theodor W. Adorno. Translated by John Cumming. (New York: Herder & Herder, 1972. Pp. xvii, 256. $9.50.)

This work, first published in 1947, appears in translation at a moment when the "critical theory" of the Frankfurt Institute for Social Research is beginning to interest some American social scientists. Some of the fruits of the Frankfurt school are already well-known here, especially *The Authoritarian Personality* and the writings of Herbert Marcuse. But Horkheimer and Adorno, the founders and main expositors of critical theory, have written principally in German, and it is only in the past few years that their chief writings have been translated. Political scientists who desire an introduction to their thought will probably find *The Positivism Dispute in German Sociology* (Adorno et al., Heineman, 1973) best suited to their purposes.

The present work is ill-suited as an introduction because it is not a book but "fragments" (p. xi), and opaque ones at that. The authors' theme is the disintegration of reason or the dissolution of enlightenment thought and practice in the present century; its task is to trace the causes and to find a way out of the impasse. But since the authors write as heirs of enlightenment rationalism (pp. xiii–xiv, 208), they are both patients and physicians. The pathology of their predicament is this. They accept the Hegelian premise that theory or knowledge is but consciousness of the praxis of the society in which it occurs; all thought is thus essentially historical and milieu-dependent. On the other hand it need not be acquiescent, for by discovering the contradictions within society, it achieves a critical distance by negating, in thought, the status quo. Such is critical theory, or as Adorno also calls it, "negative dialectic."

For the authors, enlightenment is not the propaganda of an educated elite but the movement of consciousness in history. Enlightenment begins as the effort to overcome myth, animism, and resignation to divine fatality. It is the discovery of human freedom, including the wrestle with nature for mastery. From this germ modern enlightenment matured into the scientific and industrial domination of nature for the relief of man's estate; and yet both practically and theoretically enlightened men are enslaved by the instruments of liberation. Practically they are in servitude to bourgeois economy, as the authors attempt to show in their chapter on "The Culture Industry: Enlightenment as Mass Deception." They are dominated in thought by the hegemony of the scientific instrumentalist conceptuality. Since the latter is by design objective, unreflective, and domineering, enlightenment thought at high tide "has extinguished any trace of its own self-consciousness" (p. 4). Enlightenment, specifically positivism, thus falls prey to myth—the "myth of facts": "Under the title of brute facts, the social injustice from which they proceed is now as assuredly sacred a preserve as the medicine man was sacrosanct by reason of the protection of his gods . . . animism spiritualized the object, whereas industrialism objectifies the spirit of man" (p. 28).

The book's thesis that enlightenment at the stage of advanced capitalism returns to myth seems to have been suggested by experience of the Third Reich. The authors' attempt to reveal the return to mythic-magical modes in modern society, and especially to show the tendency of bourgeois society toward fascism, is not so much argued as elicited by an organized flight of ideas and psychoanalytic associations. Their tendentious interpretation of Homer's Odysseus as the prototype of bourgeois man, and of de Sade as a spokesman for developed bourgeois consciousness, may be cited as yet more proofs of the capriciousness of depth psychology. If hunting for hidden motives were legitimate, it could be said that the authors use depth psychology as a pseudorational ritual to exorcize unpleasant realities with which they cannot otherwise deal. But this sort of thing leads to mutual abuse and to the self-destruction of reason of which the authors complain. In this respect they may be right when they imply that their own medication is part of the disease (p. xiv).

If liberal democracy deserves the harsh judgments levied here, it is important to know the authors' assessment of the results of science and domination in socialist nations. This question is not addressed at all. From other writings we learn that they do not endorse Soviet socialism. Their revolutionary language therefore refers to no political reality. The final irony for these men, who more than any other academics inspired the revival of the German Left, was that in the closing days of their career they became the objects of leftist fury.

These remarks do not imply that the Frankfurt school does not deserve careful study. American social and political thought has been so heavily colonized by German thought—by logical positivism, existentialism, Marxism, depth psychology, and sociology—that it is perhaps necessary now to examine the sources.

The translator has not always succeeded in putting the complex sentence structure into good English, and he has suppressed some unusual expressions and Heideggerian mannerisms of the authors. Otherwise the translation appears to be reliable.

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The Development of the Marxian Dialectic. By Dick Howard. (Carbondale and Edwardsville: Southern Illinois University Press, 1972. Pp. xiii, 205. $7.95.)

Most doctoral dissertations have certain unavoidable characteristics, determined by their purpose, that make them instructive to their respective authors rather than to their readers. *The Development of the Marxian Dialectic*, originally written as a doctoral dissertation, is an instance in point.

The main objective of the book appears to be an analysis of Marx's earliest writings from *The Difference between the Democritean and Epicurean Philosophy of Nature* to some sections of the *Economic and Philosophical Manuscripts of 1844* in order to establish the "groundwork of Marx's future theory" and "the unity . . . [of] Marx's life work" (p. 134). Although this groundwork is not specified, the title of the book suggests that dialectical philosophy is supposed to provide the foundation of Marx's thought. This is little help, however, since the author does not explicate the concept, nor does he define the term "dialectic" anywhere in the book.

The Development of the Marxian Dialectic falls into one of the familiar patterns of Marxian scholarship. It first summarizes the content of Marx's earliest writings, and then tries to show how Marx's own views evolved from the philosophy of Hegel and his followers. Scholars, regardless of whether they do or do not adopt a Hegelizing interpretation, tend to agree that Marx made his first discoveries by the process of encounter with Hegelian philosophy, by criticizing its internal consistency and by contrasting its claims with the state of affairs in Germany and France. Thus, Marx's philosophical, social, and political thought is shown to emerge from an attempt to reformulate or resolve some of the problems which had been stated and discussed in the 1840s. Apart from some happy formulations of uncontroversial matters, *The Development of the Marxian Dialectic* does not seem to contribute much that is particularly original to the existing literature written along these lines. The author, however, is sometimes carried away by Marxian rhetoric; one example is the significance he attaches over and over again to Marx's pronouncement about making philosophy worldly and the world philosophical, which allegedly cancels the opposition between idealism and materialism. The author's language occasionally lacks clarity; he fudges important issues and indulges in the oracular, when he could and should be lucid and explicit. Consequently, some texts of Hegel and Marx are more difficult to understand in his formulation than in the original. For instance, what does it mean to say that according to Marx "when philosophy enters the world it loses its inner unity and must recover it dialectically in interaction with the world" (p. 52)? Feuerbach criticized Hegel for inverting subject and predicate and wished to assert by this metaphor that Hegel confused what is ontologically primary (e.g., a man or a citizen) with what is derived (e.g., consciousness) and secondary (e.g., the state). The author often uses Feuerbach's metaphor literally, writing that Hegel makes the constitution into the subject and treats the legislature as its predicate (p. 68), and so forth. His final analysis fails to bring out Marx's explicit rejection of the Hegelian opposition between a natural reality and a supernatural world of abstract ideas, between the real and the ideal, where "ideal" means an existence independent of the world of our senses. His comment on Hegel's *Science of Logic* (p. 188, note 16) seems to reproduce Lenin's error and to confuse the opposition of idealism and realism with that of idealism and materialism. Hegel's dictum, "Every philosophy is an idealism," applies to the former and not to the latter.

The preceding observations should make it clear that the title of the book does not reflect its content. The term "dialectic," as a noun or an adjective, does not serve any useful purpose unless its various senses are carefully distinguished and its proposed uses clearly defined. "Dialectic" can be and has been differently understood, both in Hegel and Marx. There are those who say that there is a dialectic method or logic (without explaining what they precisely mean by it) and there are those who maintain that there is none that one could adopt even if one wanted to. For others, the dialectic is at most a method of exposition (with its notorious triad of thesis, antithesis, and synthesis), a transformation of concepts from each other, a way of looking at the world that emphasizes change and development through conflict, a tension between opposite tendencies resulting in their supersession, the appearance of unintended consequences, or a regular succession of action and its reversal. There is no way of discovering which of these or other senses the author has in mind when the term "dialectic" occurs in the title or in numerous places in his book.

The author has stated in the Introduction that he relied on primary sources for the most part and referred to secondary literature only in exceptional cases (p. viii). This is not entirely true. His observations on Moses Hess and Arnold Ruge are based on quotations borrowed from Georg Lukács's article on Hess (the writings of Hess are easily available in the edition of A. Cornu and W. Moenke of 1961), and his knowledge of Ruge appears to be derived from the writings of K. Hartmann and A. Cornu. His presentation of Feuerbach's philosophy is embarrassingly inadequate (pp. 49–51), and there is little evidence that he knows Bruno Bauer or Max Stirner first hand. It is false to call Adam Ferguson the teacher of Adam Smith or to say that Hegel took the term "civil society" from the latter (p. 179). *Buergerliche Gesellschaft* ("civil" or "bourgeois society") is derived from *Buergerstand*, the dominant estate of 19th century Germany. Ferguson was unquestionally first to use the term, and *The Wealth of Nations* had a lasting impact on Hegel, but Hegel's use of the concept of *buergerliche Gesellschaft* is his own and irreducible to the ideas of his British predecessors.

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Technology Assessment: Understanding the Social Consequences of Technological Applications. Edited by Raphael G. Kasper. (New York: Praeger Publishers, 1972. Pp. 306. $16.50.)

This volume consists of papers given at a series of seminars conducted by the Program of Policy Studies in Science and Technology at The George Washington University from January 1969 to May 1970, along with a summary of the discussions which followed. While interest in assessment of technology has grown since then, this collection remains a valuable introduction to the literature and problems in this area.

As many of the seminar participants recognize, technology assessment is nothing new. From ancient times human beings and their political agents have been concerned with the perceived or anticipated consequences of technological innovation, and both legal institutions and administrative agencies have passed judgment on technologies and their consequences. What is new about our current concern with technology assessment—as exemplified in the creation of bodies such as the Environmental Protection Agency and the Office of Technology Assessment—is the drive to make such assessment a central function of government, to be carried out in accordance with consciously determined basic social norms. Technology assessment inevitably involves both the philosophical judgments embodied in such norms and the creation of institutional frameworks to operationalize and provide sanctions for agreed-upon standards. Problems are involved both in reaching consensus about the philosophical standards to be embodied in technology assessment, and in deciding what kind of political and administrative arrangements can best serve to effectuate these standards. But above all, as this collection illustrates, it is difficult to synthesize the discussion of broad objectives with the discussion of particular means to achieve them. The literature in this area thus tends to deal either with one aspect or with the other, but rarely relates the two.

This tendency is clearly illustrated in this volume. The contributions of Clarence H. Danhof on "Assessment Information Systems," Franklin P. Huddle on "The Social Function of Technology Assessment," and Leon Green on "Technology Assessment or Technology Harassment" focus essentially on the broad problems of goals and values. Other articles covering "Technology Assessment and the Congress" (Richard A. Carpenter), "The Adversary Process in Technology Assessment" (Harold P. Green), "The Management of Technology Assessment" (Louis H. Mayo), "Technology Assessment and the Food and Drug Administration" (Alan H. Kaplan and Robert H. Becker), "Technology Assessment in the Executive Office of the President" (Charles W. Kidd), "The National Transportation Safety Board" (Ernest Weiss), and "Technology Assessment and Citizen Action" (Ellis H. Mottur), deal almost exclusively with detailed means toward particular limited ends, some describing existing systems of assessment, others speculating about what might be done. What is missing from most of these discussions is any clear focus on the politics of technology assessment in the broadest sense, the question of how a putatively pluralist society, with a political system based on conflict among competing interest groups, can or should arrive at the kinds of value consensus which would necessarily have to underlie any coherent national program of technology assessment. Some participants assume there is such a thing as the "public interest" which can provide a standard for technology assessment; others deny this either implicity or explicitly. If serving the public interest is an intellectually legitimate goal, then the various technical questions about analytical and institutional means for precisely defining and achieving it can be given meaningful answers; if not, discussing them is essentially playing games or describing the game-playing of others.

Symposia such as this can hardly be faulted for failing to solve a problem which seems to continue, necessarily or otherwise, to baffle most political philosophers. Judged on the basis of what this work sets out to do and can reasonably be expected to do—to provide an introduction to the increasingly extensive and diffuse literature on technology assessment—this is an extremely interesting, useful, and thoroughly commendable volume. Ideally, it will spur others to readdress the philosophical problems upon the resolution of which the ultimate relevance of works such as the present volume depends.

Victor Ferkiss

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Sayyid Jamal ad-Din "al-Afghani": A Political Bibliography. By Nikki R. Keddie. (Berkeley: University of California Press, 1972. Pp. xvii, 479. $20.00.)

The seemingly indefatigable Nikki Keddie has done it again. "It," in this case is the production of a definitive study of the immensely influential Sayyid Jamal ad-Din "Al-Afghani" (1838–1897). The author has laboriously examined all the available material on the Sayyid—only the opening of the Russian archives or the discovery of new materials in the Ottoman archives, where only one document on Jamal ad-Din has been found to date, may provide us with a different view of the man or his work.

Here, Professor Keddie gives us rather rudimentary details of the man. She establishes conclusively that he was born not in Afghanistan, hence the misnomer "al-Afghani," but in Asadabad, a village in western Iran. He received a traditional Iranian Islamic education in Qazvin and Tehran and in the Shi'a shrine cities in Iraq, primarily Najaf. At the age of 18 or 19 he began a life of travel which saw him remain in any one place for only a few years—to India, Mecca, Iraq, Tehran, Afghanistan, India, Egypt, Istanbul, Egypt, India, London, Paris, Iran, Russia, Germany, Iran, London, Istanbul.

In all these places, Professor Keddie tells us, he attracted disciples through the power of his written and spoken words as well as the force of his personality. He lived modestly from the contributions of rulers, nobles, and commoners, devoting himself to developing his ideas on the problems of the Islamic world. He curried and received the favor of numerous rulers, although falling from grace with regularity. (He was expelled from Afghanistan, Istanbul, Egypt, and twice from Iran.) As he aged and found himself unable to play a direct role in political life, his writings became more shrill. He began to portray himself as "a messianic savior of the Muslim world" (p. 211). His life ended in Istanbul where the Sultan, having learned the lesson, did not expel the Sayyid but kept him under virtual house arrest. He died painfully from cancer of the jaw, which court physicians attributed to excessive cigarette smoking, his last years having been characterized by "increasing obscurity, restlessness, and some bitterness" (p. 421).

His life seems unfulfilled; even his words raise problems. As Professor Keddie suggests, he was a master of "precautionary dissimulation" (p. 79), making the content and style of his message appropriate to his audience and his then immediate goals. Moreover, for all his travels in Europe, Afghani consistently deprecated the economic, political, and technological bases of the power of the West. He attributed the weakness of the Islamic peoples to the selfishness of individual Muslim rulers and the corruption of Islam by the *ulama*. Finally, only in the Khalifat movement in India, years after his death, did his ideas constitute the basis of a serious political program.

What then justifies the very widespread interest in Jamal ad-Din's thought and, thus, this book? Basically that his thought has been the inspiration for many significant political and religious movements in this century. He has become a virtual "mythic hero" in much of the Middle East (p. 422), where his influence has proved to be significant and lasting. His writings anticipate many of the major themes of contemporary Middle Eastern political concerns.

Professor Keddie shows how Afghani gave priority to the liberation of Islamic lands from European encroachments. He called for an intellectual revival and political and social activism. He supported local and Islamic nationalisms. He sought religious reform as the cultural basis of a unified and strengthened nation. He appreciated the role of public opinion and was instrumental in the establishment of many important political journals and newspapers, especially in Egypt. Afghani laid the blame for the ills of the Middle East to the decadence of rulers and clergymen whose corruption then spread to the people. He was a crucial figure in the arousal of the Muslim peoples, a process which continues at least partly through the power of his words. "The only thing that will help us emerge from our present difficulties," he told his followers, "is zeal. Zeal is possessed by only some who know that their honor is only in their race, their power is only in their community, and their glory is only in their fatherland" (p. 109).

Nikki Keddie discusses the sources and development of Afghani's thought; illuminating both its nuances and major alterations over time and place. She writes of his major disciples—Muhammad Abduh, Rashid Rida, Sa'd Zaghlul, and Mirza Riza Kirmani (the assassin of Nasir ad Din Shah) among them. And throughout, the author gracefully acknowledges her debt to a number of other studies of al-Afghani, especially those by Homa Pakdaman, Elie Kedourie, A. Albert Kudsi-Zadeh, and Niyazi Berkes.

Not that the book is without flaws. The level of detail is, perhaps, too great. The explication of his thought in each of the cities in which he resided seems occasionally to detract from our understanding of al-Afghani. To have organized the book by conceptual areas rather than chronologically may have made the Sayyid's thought easier to grasp. (But it may also have imparted a consistency and unity to that thought which was absent.) But as it is, Keddie's work will long remain the basic reference on that now considerably less enigmatic figure, Sayyid Jamal ad-Din.

MARVIN ZONIS

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Participatory Democracy: Developing Ideals of the Political Left. By Daniel C. Kramer. (Cambridge, Mass.: Schenkman Publishing Co., 1972. Pp. 249. $8.95.)

The idea of participatory democracy is gradually being brought in from the academic wilderness and given serious attention. Kramer's contribution to the reappraisal is an attempt to decide whether participatory democracy—or decentralized socialism or anti-bureaucratic socialism, which he treats as synonymous—would be worth trying to implement in practice. His conclusions are pessimistic—that it is unlikely that a majority of citizens would be self-governing in a participatory democracy, and that community is unlikely to be realized. Therefore, he argues, participatory democracy does not offer a satisfactory alternative to welfare state capitalism.

The general approach of *Participatory Democracy*, and Kramer's willingness to draw substantive conclusions are to be welcomed. Political theorists have tried to force apart ideals and reality for far too long. But the discussion suffers from Kramer's peculiar conception of citizen self-government, and his conclusions are thus far less definitive than he supposes.

The basic question asked in *Participatory Democracy* is how far a system of participatory democracy would in fact realize its major ideals and goals. Attention is concentrated on one ideal, that of the politically self-governing citizen, and the chapter devoted to exploring the meaning of this ideal is central to the whole argument. It takes as its starting point the assertion that direct democracy is an unrealistic notion, so that discussion of self-governing citizenship must be framed in terms of representative democracy alone. According to Kramer, a citizen is self-governing when "his or her views on one or more issues that can reasonably be called 'political' are among the reasons why at least one member or employee of the governments with jurisdiction over him votes or decides in the way specified by those views" (p. 39).

The concentration on "political" self-government is, Kramer says, for "reasons of style alone" (p. 41); it does not imply that other potential areas of self-government are unimportant. But even if these other areas are included—and some political theorists would wish to question whether they are non-political—this definition would remain a most inadequate one. Most obviously, it makes no reference to that central feature of "democracy" for most theorists, the free election of her representatives by the citizen, and makes the notion of citizen political self-government so weak as to be virtually meaningless. What it amounts to, as Kramer's examples illustrate, is a version of what Verba and Nie in *Participation in America* call "particularized contacting," i.e., individual approaches to public officials on matters of individual interest.

Although Kramer has set out to see if the ideals of participatory democracy can be realized, he has misunderstood the nature of the central ideal. This is largely because he has tried to assess the theory as if it were an extension of the individualistic, orthodox liberal democratic theory. He has not considered participatory democracy in terms of its own theoretical perspective. Thus, when possible objections to his definition of citizen self-government are discussed, Kramer counters the objection that the definition makes it difficult to decide how long an individual is self-governing by replying that it can only be "for a reasonable time" (p. 57). But advocates of participatory democracy do not see citizen self-government as something like the Cheshire cat. The political theory of participatory democracy does not start from individual, private interests, but is dialectical and communitarian, emphasizing the interrelations between everyday life and the political sphere—as Kramer's use of "decentralized socialism" interchangeably with "participatory democracy" might have suggested. The ideal is not that some *individuals* are sometimes, temporarily, self-governing, but that all *citizens* are collectively self-governing.

To be sure, this ideal requires the kind of evaluation of *Participatory Democracy*, but differences in theoretical perspective are crucial for the interpretation of relevant empirical evidence. Again, Kramer has missed the point that a participatory definition of citizen self-government is backed by the (empirical) claim that although representatives—democratically elected and strictly accountable representatives—are necessary in complex, large-scale societies, this does not preclude direct democracy also in many areas. Thus in his discussion of Yugoslavia, which in any case is far too cursory, Kramer makes no mention of the "economic units" in the enterprise which open up the opportunity for the ordinary worker directly to participate in everyday decisions at shop-floor level. He refers only to the central workers' council of the enterprise, i.e., to the body that is, in a different sphere, analogous to our existing, familiar elected bodies. Nor is the evidence considered which indicates that widespread lack of interest in matters outside the personal sphere might not be as "natural" as Kramer assumes, but related to the nonparticipatory nature of existing institutional arrangements. And to his charge that neighborhood and other councils would become preoccupied with "little concerns," the advocate of participation might well reply, "Little to whom"?

This is not to imply that all Kramer's criticisms fail to reach their targets. Theorists of participatory democracy have tended to neglect important questions such as the relation of participatory bodies to each other, the possible fostering of parochialism, and the precise relationship between different levels of participation. But in this last area, convincing solutions have hardly been provided by those, who like Kramer in his concluding paragraphs, argue that a better alternative to the present system is liberal democratic but "moderately centralized" socialism. A deeper and theoretically more sophisticated consideration is needed of the empirical and theoretical critique of centralized, representative institutions by the participatory democrats before that solution can be accepted.

As in so many books today, there are some irritating misprints.

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Toothing Stones: Rethinking the Political. Edited by Robert E. Meagher. (Chicago: The Swallow Press, 1972. Pp. xvii, 289. $7.95.)

This book contains fourteen responses to a six-paragraph invitation sent out by the editor. This invitation, reproduced on pp. xi–xv, asks the contributors to address themselves to the "decay of the public realm and the consequent withdrawal from political life into private life." The editor asks the contributors to raise the level of their speculations above the particularities of the 1972 presidential election, although he allows that it may be argued that "the only thing wrong with the political whole precluding hopeful participation is its present leadership" (p. xiv).

The contributions, while arranged in three unnamed and apparently undifferentiated sections, are all thoroughly individual and some of them highly personal. They do not fall into any clear categories, although some of them are much less concerned than others simply to lament present calamities. There is thus a lack of unity in the book, each contributor spinning his own thread, and no attempt being made to draw them together. The variety of the contributions, and especially the distinctive focus on some particular aspect of decay that each of them has, makes it difficult to say what the book as a whole is about. The editor suggests that "the ranging scope of this volume as a whole represents a questioning if not a denial of any restriction of the political to 'politics' and so of political or public life to 'elected public office' " (p. xvi). But even if the import of this were clear, it would also be clear that nothing like the whole of the work is about it.

One wonders whether the broad theme given to the contributors could possibly have been dealt with adequately within the compass of a short essay. The contributions are certainly insufficiently comprehensive, and too concerned with the American polity, for the sweeping subtitle "Rethinking the Political" to satisfy the expectations that it raises. Furthermore, some of the contributions address themselves not to political decay but to the ills of society generally: and inasmuch as they leave unnoticed the links with politics, it is hard to know what to make of them.

There are two questions to be put to the book: What is it about politics that needs rethinking, that is problematic, or that is wrong? And what fresh thoughts are in order?

Paraphrasing one or other of the contributors—and the vagueness of some of these paraphrasings is meant to reflect the vagueness of their originals—what is wrong is: the institutionalization of inhumanity in American policy; the hypocrisy of modern America (John G. Hessler); various kinds of excess (even in individualism) in politics and society (L. John Roos); the dirtiness of politics and political actors; and the prevalence of *hubris* (William Arrowsmith). What is problematic is whether power such as America possesses can be humane (Joseph M. Duffy); whether a large modern government is controllable, especially when the people find it unintelligible and despair over its validity (William Pfaff); whether the political order can be concerned with morality, or only with the adjustment of conflict (Aldo Tassi); how to institutionalize pluralism; what to do about fatigue in American life (Robert C. Neville); and what to do about the dangerously limited powers that exist for transcending culture (D. S. Carne-Ross). What needs rethinking is attitudes to obscenity (George Anastaplo); what to do about a situation in which nothing is either sacred or profane, and in which there is no operative language of repentance and contrition for public men (E. A. Goerner); and the notion that everything is black or white (Elinor B. Bachrach).

None of the contributors will have it that a retreat from politics is legitimate, although some of them assume (uncritically, since they advert only to youthful enthusiasm in the Kennedy heyday) that a retreat is occurring. And the only systematic sets of ideals that are advocated are very familiar—the old vision of America, and Lockean liberalism, not much reinterpreted: see especially David Little's scholarly contribution, pp. 116–143.

As might be expected, many more or less specific proposals are presented to deal with the problems that are mentioned—getting the facts to the people (Allard K. Lowenstein); withdrawing support from society when government involves conscience in morally unacceptable activities; creating structures that are accessible and that make creativity possible; committing oneself to politics as a provisional and incomplete enterprise, and ceasing to look to the state for happiness; dedicating oneself, along with a few others, to making room for a truly common and sharable public life; remedying injustices rather than pursuing national interests; being very careful in translating aims into programs; seeking a *public consciousness* that is world conscious, has a sense of corporate responsibility, accepts dissent as politically functional, and is open-ended and flexible (Douglas E. Sturm, p. 165); doing something about obscenity to improve things; forming groups to exploit the transcending power of literature; establishing universities of the public interest to be a "third force" (p. 255), which would perform some of the functions formerly performed by families and churches; and one of the contributors supplies a maxim to "operate in the exercise of political power: *Do not do that which may result in situations for which one does not possess the present means of remedy*" (p. 95).

Some of these proposals will no doubt seem naive and hopelessly idealistic to hard-headed critics—indeed some of the authors have reservations about their practicality. But some of the proposals are perhaps worthy of further development. They certainly need developing, for in several cases, what they amount to is unclear, their impact on and consequences for a political system are quite uncertain, and the reasons for supposing them to be desirable are not developed in any depth. As they stand—some of them quite sweeping, all of them emerging out of necessarily swift diagnoses of the situation and presented to the accompaniment of necessarily brief elaborations and justifications—it is hardly possible to deal with them seriously.

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The Social Contexts of Research. Edited by Saad Z. Nagi and Ronald G. Corwin. (New York: Wiley-Interscience, 1972. Pp. 409, xiv. $13.50.)

Looking backward from America's tricentennial in 2076, the historian of science may well wish for a source-book giving an overview of the growth and change in science as an activity, in the twenty-five years following World War II. Ideally such a source-book will offer many statistical series, a broad perspective, and an insight into the values, assumptions, problematics and cultural orientations of the period in question. Fortunate indeed will be the historian who stumbles across *The Social Context of Research* in the library stacks (or who tunes it in on the console of a research machine?). In this work Professors Nagi and Corwin have assembled a timely, thoughtful and well-written set of essays which illuminate the immediate past, and the present problems, of research as a social activity at the end of a momentous quarter century for the scientific enterprise.

Adopting a somewhat less Olympian stance, and merely looking forward from the present, one may venture that these essays will provide a starting point for many important investigations. By codifying the available literature on a fundamental subject, they have both highlighted its importance and provided a reference point for subsequent enquiries. The editors' initiative is thus doubly welcome.

Nagi and Corwin provide a concluding chapter on "The Case of Educational Research" (which rightly causes them much unease and seems in part to have motivated their decision to create this volume, in search of better things). They also offer "The Research Enterprise: An Overview," by way of general introduction. The other contributions fall into three sets—(1) those which codify available insights and information (Howard M. Vollmer on "Basic and Applied Research"; Richard A. Tybout on "Economics of Research"; Simon Marcson on "Research Settings"; Robert L. Hall on "Agencies of Research Support"; Norman W. Storer on "Relations among Scientific Disciplines" and James P. McNaul on "Relations between Researchers and Practitioners"); (2) those that speculate on the validity of our present knowledge and perspectives (Roger G. Krohn on "Patterns of the Institutionalization of Research"; Don E. Kash on "Politics and Research" and Irwin Deutscher on "Public and Private Opinions"); and (3) an essay that reacts sharply to the tensions of the late 1960s (William Petersen on "Forbidden Knowledge"). This last entry makes fascinating reading. The first set, which through the very act of codification yields new insights, also provides much food for thought. But it is the second set which seems to offer some of the most intriguing possibilities, and calls for further brief comment.

Irwin Deutscher explores the embarrassing consequences of the fact that "one of the few instances in which an attitude is unlikely to be translated into an opinion or an act in any social context is when it is elicited in a rigorously controlled interview situation by highly trained interviewers employing a technically high quality instrument." In other words, what one says depends as much on one's audience as one's intentions. This disturbing reality carries weighty implications for much social science, but Deutscher is better at documenting the notion than developing its implications. In "Politics and Research" Don E. Kash explores the growing awareness of both scientists and politicians that the support of science necessarily involves political as well as intellectual choices. On the basis of a careful analysis he is bold enough to offer a six-point prognosis for the 'seventies. His whole analysis might usefully be contrasted with that of Yaron Ezrahi ("The Political Resources of American Science," *Science Studies*, 1971).

The most provocative paper is that of Roger G. Krohn. He boldly divides the history of science into three phases. In the first, science was nonprofessional and idiosyncratic. In the second, from roughly the mid-nineteenth century, science was not only academic and professional, but a "denomination struggling for adherents, having devices for exclusion of unbelievers, and granting rights to those qualified to interpret the truth." The third phase, that of "big science," dates from World War II. In it science is again being transformed by a new system of support, and again finding a new place in society. Because of our failure adequately to analyze these three phases "we do not understand science." Furthermore "the traditional belief that there is one known and most favorable situation for science is ill-founded." Krohn argues his case well, and interested readers will find in Stephen Toulmin's recent book *On Human Understanding* (New York: O.U.P., 1972) much further material that analytically develops notions only implicit in Krohn's presentation.

What is apparent in all these essays is how the science-technology-medicine-complex has come to impinge on our social and political worlds through the last twenty-five years, and how much there is now to learn from systematic attention to the social contexts of research.

Arnold Thackray

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The Political Theory of a Compound Republic: A Reconstruction of the Logical Foundations of American Democracy as Presented in The Federalist. By Vincent Ostrom. (Blacksburg, Va.: Center for Study of Public Choice, 1971. Pp. 132. $6.95, cloth; $1.25, paper.)

One of the consequences of the Watergate incident frequently cited by commentators is that Congress has been or will be able to reassert its proper role vis-à-vis the Executive: the monarchical presidency might be dismantled. Madison and Hamilton, Vincent Ostrom would probably suggest, had predicted this would happen; in a compound republic no single part can gain excessive power. For those of us raised on the emergence of presidential government such a view would previously have been dismissed as conservative resistance to an active, aggressive set of government policies. But times change, after all there are now as many liberal filibusters in the U.S. Senate as there are conservative ones. Faith in presidential government as a solution to our problem has evaporated in the face of Vietnam, political trials, and Watergate.

In another sense, prevailing wisdom is challenged by Professor Ostrom in his perceptive little monograph. Most accounts of *The Federalist* emphasize their polemical nature or perform an exegesis of a particular Number or theme—"faction" for example. Ostrom makes a significant departure from these in arguing that *The Federalist* should be viewed as a coherent, consistent body of political theory: theory in a true sense, basing its argument on logically consistent basic assumptions. These are not explicit assumptions according to Ostrom, but, nonetheless, easily discernible. Relying mainly on recent writings in political economy as his paradigmatic basis, Ostrom suggests the following basic principles underlying *The Federalist:*

(1) *The Individualistic Assumption.* "Men as individual persons should be the fundamental unit to be considered in organizing any political association" (p. 18).

(2) *Relative Advantage.* Individuals are guided by self interest and will seek to maximize their interests (pp. 21–23).

(3) *Human Fallibility.* "The condition of human fallibility requires analysis . . . reconsideration and an opportunity to alter, amend or change as new information and new understanding give rise to new possibilities" (p. 25).

(4) *Political Constraint.* "When men pursue their individual advantage to the detriment, disadvantage or injury of others, constraint is necessary if the greater good is to be realized" (pp. 25–26).

Building on these basic assumptions, Ostrom proceeds with a tightly argued interpretation of how *The Federalist* provides principles for construction of a government which incorporates these assumptions, as well as the importance of concurrent regimes and the distribution of authority among national decision makers in assuring their preservation. Space limitations preclude examination of each point; suffice it to say that the reader is provided with discussion of a host of topics—security, due deliberation, expression of self-interest, and taxation among others—as they relate to the organization of government in a compound republic. The author's cross-referencing his citations to three different editions of *The Federalist*—Modern Library, Cooke, and Rossiter—is very useful.

There are two problems with Ostrom's analysis—one trivial, the other major. Of minor importance is the author's general style and tone. Although the book is quite short, it is often tediously repetitious. Points just made are summarized and then repeated at the start of a new section or chapter. Of consequence, however, are the prescriptions of the book. Recent developments such as the antiwar sentiment and Watergate notwithstanding, Congress is hardly in a position to challenge even the diminished presidency of Richard Nixon. The notion that the system of countervailing powers at the national level persists in modern times is questionable at best. In addition, revenue sharing or what-have-you cannot eclipse the inherent inferiority of states in the vertical relationship with the federal government. One has only to thumb through recent works such as M. Reagan's *The New Federalism*, to see the essense of these new relationships between levels of government. To suggest, then, as Ostrom does, that the theory of *The Federalist* is appropriate to meet modern world realities is simply not supported by the author.

These latter points, however, should not obscure the merits of this study. They relate to conclusions, not to the quality of analysis preceding them. Ostrom has provided a novel and pertinent re-examination of the thinking of Madison and Hamilton. The use of concepts from the literature of public choice provide a new departure point for study of some old problems. Ostrom's work deserves the careful attention of students of American government and American political thought. If the prospects of serious intellectual challenge are not enough, the price of a copy alone makes this one of the best bargains on the market. One hopes that future monographs in this series will provide the same combination of scholarship and economy.

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Obligation: A Social Theory. By Ralph Ross. (Ann Arbor: The University of Michigan Press, 1970. Pp. 200. $7.50.)

The great accomplishment of John Dewey's *Human Nature and Conduct* was to locate morals in the context of everyday *individual* life. By showing how our actions involve decisions about "right" and "wrong," Dewey made it obligatory for students of human behavior to take account of such decisions in the same naturalistic terms with which they dealt with attitudes, feelings, thoughts, and perceptions. What Dewey left to be achieved was to locate morals in *group* life, that is, in social and political conduct. That task requires that one show how those spheres are also permeated with moral qualities. Professor Ralph Ross's study of *Obligation* has now achieved that crucial task.

Like so many other important achievements, this one seems very simple once stated. We start with the idea that the patterns we see in conduct depend upon the analytical categories we bring to bear. Thus, the sociological aspects of conduct are those analyzed under such categories as statuses, roles, groups, institutions, and social systems, while the political aspects are those analyzed in terms of categories such as power, authority, influence, decisions, rule, and government. In the same vein, then, the moral aspects refer to conduct considered in such terms as choice, responsibility, evaluations, and obligations.

Each of these sets of categories is *limited* in that it deliberately selects out from the totality of human conduct only certain aspects for analysis. No set of limited categories can embrace the totality of conduct; none is "more basic"; all are equally "natural" features of behavior. Hence, when morality is viewed this way it becomes possible to analyze the conditions, correlatives, and consequences of moral conduct, and various systems of morals can be compared as objectively and profitably as systems of political, economic, or social behavior.

The locus of morality, for Ross, is in the obligations contained in the roles that are attached to statuses such as parent-child, husband-wife, ruler-citizen, teacher-pupil. We all occupy many of these statuses and they constitute the building blocks of all social systems. Effective freedom of choice among alternative sets of such role-obligations is both the defining criterion of moral behavior and the major source of variation in moral systems. Without such freedom, it is meaningless to impose any judgment of right or wrong. If a person is required by physical force or the absence of alternatives to follow a single line of prescribed action, then, in Ross's terms, the behavior is neither moral nor immoral; it is nonmoral.

The variability arising from differences in freedom of choice is conceptualized by Ross in terms of *existent* versus *critical* morality. The former refers to ongoing, unexamined patterns of conduct, formed of deeply habituated patterns of obligations, values, preferences and criteria of judging between right and wrong. All societies, of course, are "equal" in this type of morality.

But critical morality is a different matter, being present only when there are alternative courses of conduct to choose among, and when such decisions are made in terms of how well the chosen paths will serve desired ends and how much they will constrict or expand the human potential. Societies obviously differ greatly in the extent of their critical morality. Moreover, with this specification, the moral domain takes on certain qualities not found in other domains. For in none of those is freedom of choice a defining constituent. No society, however underdeveloped or unfree, is any less political, economic, or social than others.

The great value of this distinction between existent and critical morality lies in the break it makes with "cultural relativism," the doctrine which social scientists have traditionally invoked in insisting that one cannot usefully or meaningfully compare the moral systems of different cultures. Ross declares that not only *can* such comparisons be made but that indeed they *must* be made, if the most important human decisions are to be brought under rational scrutiny. The moral cynic's question, "Why is critical scrutiny of moral standards any better than simple uncritical acceptance of them?" is then seen as the *beginning* rather than the terminal point in moral discourse.

A few steps into that discourse reveals that "cultural relativism" rests on an unexamined belief in the "equal humanity" of all people, regardless of the conditions of their lives. But Ross argues persuasively that "humanity" is a variable and not a constant; that the faculties which define humanity can be either stunted or developed; that the conditions of development of those faculties, including the moral, can be specified; and that while, finally, individuals and societies may "prefer" to remain undeveloped in their aesthetic, intellectual and moral faculties, this cannot be said to be a "choice," unless there has been a prior awareness of alternatives and freedom to choose among them. Only with such awareness and freedom is it possible to realize the full humanity of the species.

It is the task of critical morality, Ross asserts, to inform us about the profits and costs of alternatives, drawing upon the social sciences and the humanities for the information needed. Disdain for this enterprise has made the social sciences seem lifeless, uninspired and unconnected to crucial concerns of the people they study. For Ross, then, the establishment of critical morality as a legitimate discipline is essential to reconnect the study of human conduct to the fullness of that conduct itself.

> Man can be of only dubious consequence to himself or society if he is identified totally with sociological or behavioristic man, for man as stage actor or responder is not man as moral agent. The latter, but not the former, involves reason fully, and must be thought of under the categories of choice, decision, and responsibility (p. 53).

The program Ross advocates does not involve the surrender by the sciences of their insistence on objective inquiry into human conduct. On the contrary, such concern for the facts, gathered objectively under the canons of scientific procedure, must be preserved if we are to get the reliable information we need to choose among competing courses of conduct in far more enlightened ways than now seems possible. Nor may we refrain from such decisions on the grounds of their arbitrariness. For, minus God or other absolute standards, "arbitrariness" can refer only to variations in degree of reasonableness, information, rationality, and constraint. Since moral decisions vary greatly in these regards, they cannot sensibly be said to be all equally arbitrary.

Nor can decisions on moral questions be excluded from serious study on the grounds that one cannot logically go from a statement of the "is" to a statement of the "ought." That rule describes limits on grammar, but it has nothing to say about actual conduct itself, and the ways in which the "is" and the "ought" are closely connected at all times in such actual conduct. The pursuit of this line of argument reveals that the "relativist" finally has to retreat to a solipsistic cynicism, from which posture no clarification of human conduct is possible. The final position of the solipsist is the denial of life. That always stands as a human option, of course, and its implications are clear.

Ross's formulation has powerful liberating implications as well for the study of moral development. With his conceptions of the moral faculty, and the *enabling* role of freedom of choice, it becomes possible to study the processes and stages of moral development without defining those stages in terms of culturally-specific values, such as the democratic ethos. Effective freedom, and, by implication, the democratic society that maximizes such freedom, are now seen as the *conditions* required for, but not the content of, a fully developed moral faculty. People who reach that stage of development are always free to choose anti-democracy and anti-freedom courses of conduct, knowing full well that if that choice is implemented, there will be a decline in future critical morality and in individual moral development. Socrates may have thought that to know the good was to be unavoidably committed to doing the good. But the well-tutored and powerful fascist wise-guy knows better.

To liberate two major lines of consequential intellectual venture, as Ross has done, is itself an extraordinary achievement. But there are other matters of high value in this slim and elegant volume. Limitations of space make it possible only to state them briefly.

Chapters 8 and 9 offer an intriguing distinction between political science and sociology, identifying the former as concerned with overt, conscious and intentional decisions and consequences, while the latter is concerned with the "covert" patterns, those that are less conscious and intentional. In the same chapters, the discussion of "functionalism" is advanced beyond anything now in the literature, while in the chapter on the "Social Functions of the Intellectual," the author expands the horizons of intellectual activity so as to revivify the life of the mind and reinvest it with a much-needed espirt. In his discussion of "The Limits of Consensus," Ross provides a model of how to weigh and choose among moral alternatives, here between stability and flexibility as qualities of social organization. In analyzing the diverse forms and consequences of these two qualities of social life, Ross shows how well a brilliant polymath, such as he is, can enlighten human choices when he brings evidence from science, philosophy and art to bear upon the issues. In short, he provides us with a model of critical morality working at its best. Finally, in his concluding chapter on "Obligation in Our Time," Ross attacks some of the knottiest problems regarding "alienation" in a way that cuts decisively through the morass of sentimental confusion so characteristic of most writings on that subject today.

To sum up: Ross shows that democracy, freedom, and critical reflection, using the insights of philosophy, science, and art, can make for richer and fuller lives. *Should* we prefer richer lives over other forms of existence? *Should* we be "more human" rather than "less human," where being more human means the fuller exercise of the moral and other faculties? Suppose one does not want to? Must one? No, one must not; one need not; but the consequences of doing so or not doing so are eminently clear. That is what critical morality can and does tell us. And that is why education in critical morality is an indispensable feature of any sensible curriculum.

This may all add up to whistling in the wind, as far as any influences on the conduct of education are concerned. After all, Dewey's equally seminal and penetrating discoveries in *Human Nature and Conduct* have gone largely unattended. But one can do no less than to call attention to what is in store for us if we persist in the desiccation of our study of human conduct, and, alternatively, what promises are implied about what we might become if we attend to the wisdom of this fine book.

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The Idea of Culture in the Social Sciences. Edited by Louis Schneider and Charles Bonjean. (Cambridge and New York: Cambridge University Press, 1973. Pp. 149. $9.95, cloth; $2.95, paper.)

The concept of culture is widely used by social scientists, although they often do not agree on its meaning. This book includes essays by scholars representing the disciplines of anthropology, sociology, economics, political science, history, and geography, who have attempted to identify the role of the concept in their discipline. As the editors have stated: "It was hoped that from scrutiny of their various contributions, significant convergences and divergences in usage of the idea might appear and that some important hints might emerge with regard to the unity—or lack of unity—of the social sciences" (p. vi).

Anthropologist James Boon presents, with great erudition, a synthesis of the use of the culture concept in his discipline. One gets the impression that he is passionately groping for a higher-level understanding of the concept. Sociologist Talcott Parsons attempts to summarize his work on the distinction between the cultural and social systems. In his explication of the four parts of the cultural system—employing the categories of cognitive symbolization, moral-evaluative symbolization, expressive symbolization and constitutive symbolization—he provides an elaboration of the culture concept.

Economist Kenneth Boulding advocates the development of a cultural economics which would provide a new dimension to economic analysis. He is distressed by the lack of interest among economists, with the exception of labor economists, in the role of culture. He points out that anthropologists have also neglected the study of economic institutions, as exemplified by Margaret Mead's refusal to consider writing, at Boulding's suggestion, "Coming of Age in the Federal Reserve." He presents an exceptionally lucid analysis of the relationships between phenomena at the macro- and microeconomic levels. His discussion of macroeconomic paradoxes (p. 62–64) is most interesting; he shows, for example, that decisions to save on the part of individuals may not result in the net accumulation of capital but may lead to increased unemployment and a decline in output. Or a rise in money wages may result in smaller real income during periods of inflation.

Political scientist Lucian Pye also examines problems at the macro (political culture) and micro (personality) levels. He argues that the basic problem of behavioral political science is to understand how individual psychology can account for the performance of political entities. He notes the tremendous diversity of subjective experience, only a small fraction of which is registered in behavior, and alleges his preference for going from the performance of the political system to the analysis of its probable psychological antecedents. He supplements the analysis by checking the hypotheses developed from it through survey research.

Historian Robert Berkhofer, Jr. associates the concept of culture with broad theories of history and society which leads him to criticize certain historians who have overstressed the importance of ideas derived from particular views of culture, as determinants of historical events. Geographer David Sopher examines the spatial patterning of culture, which shows that cultural notions have provided geography with important descriptive material and research tasks.

Finally, sociologist Louis Schneider reviews the contributions of the other participants and provides suggestions for developing and elaborating the concept of culture. He examines notions like automatization (cultures evolve in an autonomous fashion), hierarchy (elements of culture can be found at different levels), and alternation (members of different cultures do things that superficially look different when in fact they are equivalent), and suggests that they should be developed even further in order to increase the utility of the concept of culture. He finally expresses a cautious hope for the future of the idea of culture in the social sciences.

The volume is useful for those who wish to rethink their use of the concept of culture and also those who wish to become acquainted with the viewpoints of other disciplines. Yet I myself did not get any significant new insights from this collection nor find any clarification of why some of the simpler definitions of culture might not be preferable.

My own favored definition is that which anthropologist M. Herskovits offers in *Cultural Anthropology* (New York: Knopf, 1955): "Culture is the man-made part of the human environment." This definition is striking in its simplicity and clearly includes Tylor's much quoted definition. Schneider objects to "terminological inflation," which occurs when a concept includes too much. This problem can be dealt with, however, by proper qualification of a term, such as material culture, subjective culture (e.g., Triandis, *The Analysis of Subjective Culture*, New York: Wiley, 1972), political culture, scientific culture, etc. It seems to me that the strategy of a *clear*, although admittedly too inclusive, definition is likely to increase effective communication. After all, the purpose of a scientific construct is to aid in thinking and in communication among scientists. Boon's analysis in this volume does stress the communication aspect of the construct, Parsons emphasizes the symbolic aspect, and the other essays clearly show that the function of a scientific idea is communication.

Herskovits's definition is certainly one of the ideas that is most easy to communicate. With proper qualifications it can be sharpened to be at least as useful as the definitions that are proposed by the essayists of this volume. For example, subjective culture can be defined as the subjective reaction to the man-made part of the environment. This definition immediately suggests that attitudes, beliefs, norms, roles and values are elements of subjective culture, and they can in turn be defined to fit into the broader category. For example, attitude can be defined as an idea, charged with affect, predisposing action toward some entity of the man-made environment. This line of argument was not considered in this volume, although it seems one that could have been fruitfully discussed. Perhaps it is a line of argument that only a psychologist, like myself, would find useful, and the failure of the volume to include a contribution by a psychologist resulted in this omission.

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The Structures of the Life-World. By Alfred Schutz and Thomas Luckmann. Translated from the German by Richard M. Zaner and H. Tristram Engelhardt, Jr. (Evanston, Illinois: Northwestern University Press, 1973. Pp. xxxvi, 335. $10.50.)

This volume is the first installment of the originally unfinished manuscripts of Alfred Schutz completed by his student, Thomas Luckmann—a *magnum opus* which took many years of labor from both of them. The publication of the second installment is expected in another year. Before his death in 1959 at the age of sixty while teaching in the graduate faculty of the New School for Social Research in New York, Schutz had finished only one book—*Der sinnhafte Aufbau der sozialen Welt* (1932), translated in 1967 as *The Phenomenology of the Social World;* in it he confronted Max Weber's sociology with Edmund Husserl's phenomenology in such a way as to lay the foundation of his later works. Though unfinished before his death, *The Structures of the Life-World* (*Die Strukturen der Lebenswelt*) represents the lifelong project of a great thinker whose themes have been scattered throughout his papers and manuscripts which were collected and edited in three volumes of *Collected Papers* (1962–66) and *Reflections on the Problem of Relevance* (1970).

Schutz devoted his whole life to "the problem of social reality" as aptly suggested in the subtitle of the first volume of his *Collected Papers*. As *The Structures of the Life-World* testifies, Schutz's phenomenology is an archaeology of social reality or the everyday life-world as the intersubjective world. The importance of this work of Schutz's (and of his other works as well) for the human or social sciences is indicated by his conceptualization of social reality as "the sum total of objects and occurrences within the social cultural world as experienced by the common-sense thinking of men living their daily lives among their fellow-men, connected with them in manifold relations of interaction" (*Collected Papers*, I, 53). Political reality may be viewed as a specific region or subworld of social reality thus defined. By reality or world is meant "certain unities of meaning" organized by human consciousness in certain fixed, specific ways correlative to external things both human and natural. In reading *The Structures of the Life-World*, those who know Schutz's previous works will recognize the familiar terrains and contours of his recurring themes. In it, however, there are surprises and additions and detailed analyses of ideas that previously were only sketched (see, for example, the fourth and last part on "Knowledge and Society").

Political scientists need to understand the very basic phenomenological framework underlying *The Structures of the Life-World*. First, phenomenology is a philosophy of meaning. Unlike natural objects, men, whose actions are the subject matter of the social sciences, are conscious and embodied beings who are capable of endowing meaning to their actions, that is, of performing their acts with goals in view. The task of phenomenology, therefore, is similar to an archaeological expedition which attempts to excavate the sedimented layers of the common-sense understanding of actors in the social scene: it is to explore the meaning of meaning. Second, following Husserl who discovered the importance of the life-world for theoretical enterprises, it is the task of Schutz and Luckmann in *The Structures of the Life-World* to *show* the relevance for the social sciences of the life-world *as* social reality where men as bodies endowed with active consciousness pursue their practical interests in interaction with other men. Above all, the decisive importance of the life-world (and thus phenomenology) is twofold: the life-world is the practical province of action whose analysis has the central place in phenomenology as in the social sciences, and it is also an intersubjective world. As the life-world is intersubjective from the very beginning, intersubjectivity is the modality of human existence "pregiven" to everyone as long as man is born of woman. The life-worldly experience characteristic of the normal, wide-awake adult in his natural attitude is the "primal type" of reality we experience; and all the other provinces of meaning are construed as modifications of it. By the same token, the stock of knowledge necessary to carry out everyday life-activities is also pre-eminently socially derived. In contrast to theoretical thinking which brackets everything in radical doubt, the life-worldly stock of knowledge is "taken for granted" or accepted as "self-evident" without question ". . . until further notice," although it is in principle questionable in a problematic situation. By means of his stock of knowledge at hand, the actor is interested in orienting himself in routine ways toward practical achievements. In the everyday life-world intersubjectivity or sociality itself is taken for granted in the following way:

> (a) the corporeal existence of other men; (b) that these bodies are endowed with consciousness essentially similar to my own; (c) that the things in the outer world included in my environs and that of my fellow-men are the same for us and have fundamentally the same meaning; (d) that I can enter into interrelations and reciprocal actions with my fellow-men; (e) that I can make myself understood to them (which follows from the preceding assumptions); (f) that a stratified social and cultural world is historically pregiven as a frame of reference for me and my fellow-men, indeed in a manner as taken for granted as the "natural world"; (g) that therefore the situation in which I find myself at any moment is only to a small extent purely created by men (p. 5).

Third, as phenomenology is concerned with the experiential meaning the actor confers to his action, the analysis of action must take into account the structure of internal time-consciousness or temporality as experienced directly by the actor himself. According to the temporal dimensions as experienced by the actor, Schutz formulates his typology of the social world: the worlds of (1) "consociates" (*Umwelt*), (2) "contemporaries" (*Mitwelt*), (3) "predecessors" (*Vorwelt*) and (4) "successors" (*Folgewelt*). Among these four worlds (or more precisely subworlds), the world of contemporaries is most directly relevant to the social-science analysis where the role of man is understood in terms of anonymity and typicality.

The Structures of the Life-World is divided into four parts: (1) "The Everyday Life-World and the Natural Attitude," (2) "The Stratifications of the Life-World," (3) "Knowledge of the Life-World" and (4) "Knowledge and Society." The first part is a short outline of the everyday life-world as man's fundamental and paramount reality or the paramount province of meaning which is taken for granted by the normal, wide-awake adult in interaction with other men through his common-sense knowledge. The everyday life-world is not only the world which modifies our actions but also the world which we in turn modify through our actions. The second part deals with the temporal, spatial, and social stratifications of the everyday life-world as we experience or live it. Schutz and Luckmann start their analysis with the *immediate* experience of the Other in the "thou-orientation" and the "we-relation" where we share simultaneously the flow of each other's lived experience and extend it to the *mediate* experience of the Other in the "they-orientation" of contemporaries (as well as of predecessors and successors) in relation to typification and anonymity whose "functionary type" (e.g., *the* government) is of great importance for political science. The third part is concerned with various phases of the actor's stock of common-sense knowledge. It ranges from the analysis of the situation, types of common-sense knowledge essential to the performance of action and the acquisition of knowledge to the problem of relevance. As life taken as a series of situations is conditioned by relevance, that is, by our "interest" in a given problem or something that "really matters to us," Schutz and Luckmann consider relevance as the most important though most difficult problem in describing the everyday life-world. With great care and detail they analyze relevance in its thematic, interpretational, and motivational structures and their intertwinement. The fourth and last part deals with the social derivation of all knowledge as "intersubjective mirroring," the presuppositions of knowledge as subjectively experienced and how it is objectified and how knowledge is transferred and distributed socially. Thus it is both a claim for a *social* theory of knowledge and a significant contribution to the sociology of knowledge. In short, it is a social archaeology of all knowledge.

In summary, *The Structures of the Life-World* is a work of great magnitude in the epistemology of the social sciences in general and phenomenological social science in particular. By revealing the *social* foundation of social-scientific epistemology, it has shown the relevance of the phenomenology of the life-world to the human or social sciences. It occupies an important niche in the philosophy of the social sciences. Its cardinal merit lies in showing how to *practice* rather than how to *talk about* phenomenology. With Schutz, phenomenology has finally come of age in the social sciences. The inclusion of Schutz's selected writings expertly edited by Helmut R. Wagner in "The Heritage of Sociology" series under the title of *On Phenomenology and Social Relations* in 1970 is a testimonial to this fact. Schutz's influence on the methodology of the social sciences has been growing steadily since his death—notably in the ethnomethodology of Harold Garfinkel, Aaron Cicourel, and others in American sociology today. Schutz's phenomenology makes a revolutionary challenge for the discovery of a paradigm in formulating the general principles of epistemology for both the social and natural sciences. It adds an important impetus to the development of the philosophy of the sciences. As for our profession, Schutz's phenomenological insights offer a genuine alternative to the philosophy of political science couched in the dated epistemology of logical empiricism that has dominated the American scene so long—perhaps too long.

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Mind and Politics. By Ellen Meiksins Wood. (Berkeley: University of California Press, 1972. Pp. 192. $7.50.)

Professor Wood, who teaches political theory at Glendon College, York University, has written a provocative, lucid study of the relation between epistemology and the differing concepts of individualism which mark liberal and socialist political philosophy. To the liberalism of Locke and J. S. Mill she attributes the proverbial *tabula rasa* image of the mind passively receiving experience. To Rousseau and Marx she attributes a Kantian notion that the mind is a positive participant in the construction of experience through "dialectical" interaction between *a priori* categories of thought and the external world. Although she recognizes that both views may arguably fall within the same broad tradition of empiricism, her main thesis purports to be that these theories of knowledge imply quite disparate political results. Despite its apotheosis of individuality, liberalism creates an atomistic society in which individuals become objects or occupants of externally imposed roles, functions, and interests. All genuine sense of community is missing. The alienated individual confronts the state, like Mount Everest, as something that is simply "there." In contrast, Professor Wood holds, the dialectical theory of knowledge implies an inseparable interrelation between true individuality and community—within a socialist future. It promises the "convergence in all men of public and private" (p. 109) and the "transformation of human relations" (p. 169).

All of this involves some problems of interpretation and omission. The author herself concedes in the end that Rousseau must be understood "in moral rather than epistemological" terms (p. 172). As for Marx, she remarks early on that it "can be argued" (p. 7) that he falls within Kantian epistemology. But from then on, the argument becomes largely assumption. Relying a bit too much on the admittedly epigrammatic *Theses on Feuerbach*, she misses other important points of reference, including Marx's own explicit statement of a materialist theory of reflection in his preface to the second edition of *Capital*. Rather than letting Marx speak for himself, she too often reads into him the views of Fromm, Avineri, Sartre, and Marcuse, who happily agree with her but hardly represent the full range or sharpness of debate over Marx's epistemology. The names of Kolakowski and Schaff, for example, are curiously missing. Nor does she satisfy this reviewer on Marx and history. Dismissing the usual deterministic interpretations as "misconceptions" (p. 178), she nevertheless comments that for Marx the French Revolution could have been only bourgeois, since "historical circumstances and the development of productive relations as yet alallowed nothing more" (p. 164). If that implies a dialectic in history, it leaves us uncertain about the exact point in history at which the mind of individual man will triumph in dialectical mastery of the external world. The possibility that that promise—whether from Rousseau or Marx—may somehow be betrayed is hardly touched by Professor Wood.

Yet, in dealing with liberalism, she draws in detail what she considers its betrayal—not only in liberal society but in the pessimism which she finds below the surface of liberal theory. Although she notes Locke's moral norms of natural right and voluntary contract, she considers far more significant his doubt that individuals will in fact behave rationally and his recognition of class inequality in postulating the existence of money prior to the social contract. As for J. S. Mill, she even suggests that his concept of free individuality may have been an "aberration" (p. 41) in his overall philosophy.

But given a twist in the meaning of "dialectical," the case of Mill may instead suggest that it is liberalism itself which in fact meets the demanding standard initially posited by Professor Wood. As she remarks more than once, actually in complaint, both liberal theory and society contain "an eternal tension . . . between individuality and sociality" (pp. 123, 159). In turn, her own futuristic vision of a conflict-free society in which individuality and community converge seems to imply on occasion the end of the dialectic (pp. 120, 170). And so ironically, it may be the latter-day liberals, the heirs of J. S. Mill, who alone live with an unending dialectic as they forever restate their creed in response to the refractory world around them. Or, why not just admit, as have so many commentators on Locke and Mill, that there has all along been a "discrepancy" between epistemology and political prescription in liberalism, rather than arguing either that one leads necessarily to the other or that one may be an "aberration" from the other? It may simply be that many political philosophers in the age of modern science and material progress have found any idealist theory of knowledge impossible to accept, but at the same time have found the dream of ultimate individual freedom irresistible. That may fit Marx as well as the liberals. And of course it badly compromises Professor Wood's basic thesis about the importance of epistemology.

But in the end does she still hold to that? There are hints along the way that it could be an open question whether a political thinker's epistemology or prescription in fact comes first. She concedes as well that our disputes over epistemology *per se* may forever elude resolution. And so in her final lines, Professor Wood suggests as an alternative that we see "the controversy as . . . a choice between different designs for man's life in society" (p. 185). By that she apparently means again liberal society *vs.* a Rousseauistic-Marxist vision. Whichever we choose, we can then presumably work our way back to an appropriate theory of knowledge and human nature. One may understandably be tempted to wonder whether this closing turnabout is quite fair play. But despite these difficulties, Professor Wood has managed to hold the problem of epistemology and political prescription in a sustained focus which is unusual in contemporary commentary. Anyone seriously interested in that general problem, or concerned more particularly with liberal and Marxist thought, indeed stands in her debt.

VINCENT E. STARZINGER

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Television and the Public. By Robert T. Bower. (New York: Holt, Rinehart and Winston, Inc., 1973. Pp. 205. $7.95.)

Many scholars who are increasingly concerned with the pervasive influence of television on political behavior will welcome the publication of this book. Bower, head of the Bureau of Social Science Research in Washington, D.C., deserves credit for a solid examination of mass perceptions of television: who watches what, when, and why (according to a national survey conducted by the Roper Organization in 1970). His research replicated a study by Gary Steiner ten years earlier (*The People Look at Television: A Study of Audience Attitudes*). In addition, Bower analyzed the diaries of a viewing sample of Minneapolis-St. Paul households. It must be noted that the Columbia Broadcasting System sponsored both studies; however, there is no taint of bias on Bower's part.

Tables, charts, and graphs proliferate throughout the book. Unfortunately, readers are forced to apply their own tests of significant differences on some sets of statistics which would benefit by their inclusion. Despite a methodical redundancy in much of the text and Bower's occasional tendency to dwell on rather tedious findings, the data are clearly organized.

Although much salient data on public opinions of television have already been incorporated into conventional wisdom, this work is an impressive documentation of the paramountcy of television as America's most important source of significant political information. It should be noted that Bower's focus on perceptions of presidential election coverage and network news (albeit very brief inquiries related to educational and cable television are included) reinforces an emphasis on national politics that television itself has encouraged. Not even implicitly does the study suggest how television has helped to shape our concepts of community and thereby modify the potential of politics in society.

Little is said about political socialization *per se*, but there is much information that scholars in this field may find useful. Bower found, for example, that race was the best predictor of people's general attitude toward television in 1970. Blacks, regardless of education, took a considerably more sanguine view of both programming and commercials than whites. Inexplicably, whites viewed an average of three more hours per week than blacks.

In dealing with the age, Bower utilized multivariate and cohort analyses to rebut gently the "generational" theories of opinion differences. Strangely, questions about age-race relationships were not even raised.

As of 1970, Americans watched more television and with greater satisfaction than ever before. College-educated people were most likely to be critical of television. They spent slightly more time in front of their sets than less educated viewers and there was no discernible difference in their viewing habits.

Much of the criticism of television alluded to excessive violence; other criticisms mentioned moral problems in general and children viewing adult programs in particular. The most objectionable violence was real violence: crimes, demonstrations, and wars covered by television news cameras. Other such data revealed by Bower (and elsewhere) will undoubtedly be utilized in pressuring news editors to project a more roseate view of current events.

The title may mislead impatient readers concerned with more profound inquiries than what people say they think about television. Americans may complain mildly about commercials, but they are unlikely to divulge much about the subtle effects of electronic hucksterism on themselves or on their political culture, generally. Most commercials are, after all, designed to exert a cumulative and subliminal influence on behavior. Similarly, students of the cumulative impact of explicit and excessive violence as entertainment are not likely to be enlightened by asking people what they think about television and how it affects them. Readers should know they can expect no more—nor less—than Bower promises in his Introduction. The publication of his basically one-dimensional analysis amounts to a small but worthy contribution to the literature on public viewing habits and attitudes about television.

PAUL CONWAY

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Congressional Voting in a Partisan Era: A Study of the McKinley Houses and a Comparison to the Modern House of Representatives. By David W. Brady. (Lawrence: The University Press of Kansas, 1973. Pp. 273. $10.00.)

Students of Congress, including those who concentrate upon contemporary behavior, should find Professor Brady's book a valuable addition to the relatively slim literature on the institution in the 19th century. The study consists of a complementary combination of history and quantitative analysis of roll-call voting in the 55th and 56th Houses (1897–1900). Brady makes generally good use of such tools and techniques as the Riker significance coefficient, indices of party cohesion and party likeness, factor analysis, and multiple linear regression in order to document and explain the extraordinary degree of party voting in these Congresses.

Briefly, the author's major thesis is that the high levels of party conflict and intraparty loyalty were produced by an interactive combination of external forces (chiefly constituency influences based upon the well-known socioeconomic and sectional cleavages of the period) and internal forces (principally the hierarchical and centralized party leadership structures in the House). Not unexpectedly, Brady draws upon the notion of the election of 1896 as a "critical" one and upon Polsby's account of the institutionalization of the House during the 1890–1910 "take-off" period. He outlines the issue agenda of the time (including the bitter clash between agrarian and industrial interests) and shows that these issues were the source of consistent, unrelenting interparty conflict, a conflict which was heightened by the structure of power in the House. The increasing volume and complexity of the congressional workload attendant on the emergence of the new agenda in the industrializing nation created a strong impetus for the centralization of power in the hands of an experienced and knowledgeable leadership.

Specific findings are too numerous to summarize here, but it should be mentioned that Brady finds the McKinley Houses to have the highest levels of party voting in the history of the body. Factor analysis shows that party is the sole factor which can be reasonably inferred to underlie roll-call outcomes. Because Brady is well aware that party sometimes has a certain "emptiness" as an explanatory variable, he goes on to explore the interrelationships between it and several constituency factors. Democrats are most likely to come from predominantly agrarian southern and western districts, while Republicans are more likely to represent industrialized northeastern or midwestern districts. Against individual party support scores the author regresses party affiliation, interparty competition scores, and an "Industrial-Agricultural Scale" score for the district. To control for regional effects he runs separate regressions for each of the four major regions. Unfortunately, data limitations force Brady to rely heavily upon the industrial-agricultural score as the sole measure of the nonpolitical aspect of constituency. Despite this limitation, constituency performs rather well in the regressions, generally following hard on the heels of party competition in terms of independent explanatory power, particularly in the 55th Congress. Methodological pragmatists will be gratified to know that the full and regional regression equations "predict" with considerable success (as such things go) in both Congresses.

Brady deserves commendation for his efforts to relate his findings to evidence about other sessions of the House, especially several 20th-century ones. He examines the works of Lowell, Turner, MacRae, Truman, Froman, and Shannon, finding both points of convergence and divergence with several of these authors.

The author's discussion of institutional factors is informative and represents a thoughtful application of the institutionalization interpretation. He contends that the leadership of the House was well-equipped to exercise control, in view of its clear experience advantage over the rank and file and also its simultaneous control over the leadership of the standing committees. The high rate of membership turnover and large numbers of freshmen and second-termers created an environment in which the taking of cues from the party leadership became natural and expected. Moreover, as Brady observes, the leaders of the respective parties tended to come from districts which were prototypically Democratic or Republican in their constituency characteristics. Thus, the policy preferences of the leaders were readily translated into the official stands of the legislative parties, and were promoted effectively because of the positional advantages enjoyed by the party leaders. Brady finds that the most extreme supporters of the party positions were the leaders themselves. In sum, the influence of party as a voting cue was, as Brady hypothesizes, attributable to a unique combination of clearly defined constituency pressures and skilled, authoritative, and cohesive leadership. The extent to which this contrasts with the modern House is the subject of Brady's brief concluding discussion. Two obvious major differences are the greater heterogeneity of today's constituency interests and the absence of any substantial overlap between party and committee leadership in the modern House.

Because the book is rich in data and because the analysis is both meticulous and refreshingly modest in tone, there is some temptation to minimize flaws in the study. Most noteworthy is Brady's tendency to treat the House as an institution functioning in relative isolation from both the White House and the Senate. Despite the considerable technical skill which he exhibits in this book, Brady does little to answer the nagging question of whether it is possible to strive for generalization based upon statistical analysis of yeas and nays without also doing serious injustice to the rich complexity of the "life-worlds" of the legislators under scrutiny.

CHARLES M. TIDMARCH

Union College

American Criminal Justice: The Defendant's Perspective. By Jonathan D. Casper. (Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1972. Pp. 175. $5.95.)

Crime and Justice in American Society. Edited by Jack D. Douglas. (Indianapolis, Ind.: The Bobbs-Merrill Company, Inc., 1971. Pp. 297. $7.50.)

Crime, Dissent, and the Attorney General: The Justice Department in the 1960's. By John T. Elliff. (Beverly Hills, Calif.: Sage Publications, 1971. Pp. 276. $10.00.)

Research in the field of the administration of criminal justice has hardly struck out in original directions; indeed, the old staples of plea bargaining, civil rights, deterrence, and drugs have continued to form the bulk of the writing. The three books reviewed here, however, attempt in different ways to carve out newer approaches.

Perhaps the most original approach is that of Professor Jonathan D. Casper, who, in *American Criminal Justice: The Defendant's Perspective*, examines criminal justice from the perspective of the defendant, rather than that of the judge, prosecutor, defense lawyer, or neutral observer. His point of view—that of the "consumer" of criminal justice—has largely been ignored.

Professor Casper is appropriately modest about the significance of his research, which is limited to taped interviews with 71 defendants charged with committing felonies in the State of Connecticut. Of these, 49 were inmates of Connecticut correctional institutions, 16 were on probation, and 6 had received dismissals or acquittals. The men in prison were selected on a random basis, whereas those on the outside were self-selected—they responded to a solicitation letter. Although the sample is quite skewed—it is limited to one jurisdiction and by and large to those who were ultimately convicted—the defendants' attitudes and perceptions, according to Professor Casper, suggest lines for further research and dispel some of the common myths about the typical defendant as "crazed killer or mugger or the glassy-eyed junkie" (p. xiii).

If these defendants are typical, what they learned from their jail experience is what they knew before. They see the system "as an extension of their life on the street."

> . . . The criminal justice system like the streets is a game of resource exploitation. The defendant typically has little in the way of resources and doesn't win. He can, though, with luck and skill, lose less than he might. In this way, I want to argue, the system has no moral component in the eyes of the defendant. . . . If the policeman's job is to catch criminals and the prosecutor's job is to convict them, this is about the same as saying that the defendant's job is to break and enter. All are, in the defendant's eyes, about equally immoral (p. 18).

Crime and Justice in American Society, edited by Professor Jack D. Douglas, is an acerbic response "to the alarming and fundamental inadequacies" of the report of the President's Commission on Law Enforcement and the Administration of Justice.

The introduction and the eight essays in the book center on three controversial themes. The first is that "the public outcry over 'crime in the streets' is a form of hysteria which distorts realities and, which, consequently must not become the basis for any national policies pertaining to the fundamental issues of crime and justice in our society."

The second theme is that "laws, law enforcement, crime and justice are largely determined by political decisions" (p. xviii). Mr. Douglas argues that criminal laws are enacted by the middle and upper classes to place the poorer classes under the more direct control of the police, while the middle and upper classes pass only civil laws to control violations within their own ranks.

The final theme is that the assumption on which law enforcement procedures and judicial practices proceed is no longer valid—if it ever was. The basic assumption of these essays is that our laws and legal practices, not the social imbalances of our society, are causing our most severe problems of crime and justice. This assumption forms the basis of their attack on the view of the President's Commission that the challenge of crime in a free society can be met only by a socioeconomic reordering of our society.

The now-controversial office of the attorney general, which until recently has been largely ignored as an area for research, is the subject of Professor John T. Elliff's *Crime, Dissent, and the Attorney General*, the first book in a series under the general editorship of Professor Joel B. Grossman of the University of Wisconsin. This series is designed to explore the policy-making role and functions of courts and other agencies concerned with law in the political system.

Based on extensive interviews with past and present officials in the Department of Justice and access to some records, this book describes the forces that led the Department of Justice to assume a larger role in the criminal justice process than ever before. Professor Elliff compares and contrasts the roles of the attorneys general Robert Kennedy, Nicholas de B. Katzenbach, Ramsey Clark, and John Mitchell, focusing on Clark and Mitchell, much to the disadvantage of the latter.

One chapter devoted to Domestic Intelligence Surveillance anticipates the revelations of Watergate. During the 1960s the Justice Department supplemented the FBI's activities by creating its own computerized Interdivisional Intelligence and Information Unit, originally designed to anticipate civil disorder. The growth of the activities of the domestic intelligence-gathering efforts was largely unknown by political scientists and, as reported in this book, had somehow escaped the attention of most of the members of Congress.

This intelligence gathering occurred without the benefit of clear administrative, legislative, or judicial guidelines. Professor Elliff explores the uses of electronic surveillance and covert infiltration, and their possibly chilling effects on freedom of speech. He concludes that "if political liberty is to be firmly established in the long run, the Justice Department must give up its functions under statutes that proscribe essentially political offenses" (p. 255).

Well written and convincingly documented, this book lives up to its goal of setting the Department of Justice at the center of the legal policy-making process.

Ruth G. Weintraub
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The War Industries Board: Business-Government Relations During World War I. By Robert D. Cuff. (Baltimore: The Johns Hopkins University Press, 1973. Pp. 304. $13.50.)

Robert Cuff's detailed and imaginative history of the War Industries Board uncovers the political anatomy of an important experiment in government economic controls. While the book suffers somewhat from lack of breadth and comparative perspective, it provides many valuable insights for scholars with a general interest in administration, large-scale regulation and planning, and business-government relations.

The early chapters document the dominant role of business interests in seeking government regulation when the marketplace is disrupted. Wartime mobilization had created production bottlenecks, shortages of military supplies and basic commodities, inflation, and an uncoordinated scramble among military purchasing bureaus and private firms. The federal government's initial stabilization efforts were dominated by industry-staffed advisory committees, which pushed a reluctant administration and Congress for legal powers to enforce their plans. As reorganized in 1918 under Bernard Baruch, the War Industries Board (WIB) came to be seen as one of the greatest concentrations of government power in American history: fixing prices, dictating priorities in production and shipment, and generally establishing the prototype for a governmentally planned and integrated economy.

The heart of Cuff's book re-examines the extent and application of WIB's power. The supposed paragon of centralized control, he concludes, was in fact disorganized, weak, and often ineffective. Its authority was frequently circumvented by recalcitrant military bureaus and businesses. The agency was deeply dependent on the voluntary cooperation of industry (and deferential to its interests) in setting prices and administering priorities. This was partly due, Cuff holds, to WIB's structural weaknesses. It lacked legal authority over the military and business; inadequate administrative resources induced dependence on industry for staff, knowledge and implementation; consequently it was fragmented into decentralized committees, oriented to particularistic needs of specific industries. Yet WIB's lack of centralized coercive power, Cuff argues, was entirely in accord with the motives of WIB's business man-administrators, who hoped to demonstrate that the nation's needs could be met by voluntary cooperation among public-spirited industrialists, and who believed that the mobilization effort should preserve the fundamental structure of American business. Therefore WIB leaders were ambivalent in pressing for greater legal authority (although it is not clear they could have gained it), and were content to rely on moral suasion and "regulations" that merely placed a governmental imprimatur on agreements engineered by dominant firms in each industry.

This is revealing scholarship, more balanced than most "revisionist" histories of the Progressive era. But one is not entirely persuaded how far WIB's organizational weaknesses actually rendered it ineffective. Cuff's emphasis on the *process* of regulation rather than its *consequences* leaves the reader with no external indicia of WIB's impact and no systematic discussion of wartime production, changes in business profits, or inflation. (In fact, inflation abated during 1918, especially for basic metals subject to WIB agreements.) Ironically, Cuff's diligence in probing beyond official reports to in-house memoranda and letters of top WIB officials may account for an apparent overemphasis on agency failings and disregard of successes: such high-level communications are likely to stress problems and crises, rather than ongoing regulatory activities handled routinely and effectively by lower-level officials. Moreover, WIB's performance is often implicitly measured against an impossible standard—the highly integrated, powerful, and uncompromising agency pictured in its own press releases. The resulting implication, surely unwarranted, is that when a WIB regulation was bargained-out with industrialists the agency had failed, that what was good for General Motors was *necessarily* bad for the country.

An inherent weakness of the single case study is underlined by Cuff's attribution of WIB's problems to its organizational structure and voluntarist ideology. What has been the fate of comparable regulatory efforts during the Depression, World War II, in post-war Europe, and in the Nixon administration, where the government agency *had* what WIB lacked—clearcut legal powers, greater administrative resources, more balanced leadership? A comparative analysis, one suspects, would reveal that WIB-like decentralization and particularism, bargained-out regulations, concessions to the economic stability of regulated industries, all develop in other settings, and stem from the functional dynamics of modern industrial organization rather than the legal, organizational, or ideological characteristics of the regulatory agencies. Indeed, the most valuable aspect of Cuff's study is its illumination of *generalized* regulatory processes: control by normative appeal and threat of adverse publicity; reliance on businessmen for advice, information and implementation; pursuit of agreement rather than legal coercion. In this regard the story of WIB enhances the literature of institutional studies on which a theory of governmental economic planning and regulation might be based.

Robert A. Kagan

Yale Law School

The Desirability of a Correctional Ombudsman. By Timothy L. Fitzharris. (Berkeley: Institute of Governmental Studies, University of California, 1973. Pp. 114. $3.00.)

The author advocates the establishment of an ombudsman specializing in complaints arising from the state corrections system. The California Legislature had passed a bill to set up such an institution in 1971, but it was vetoed by the governor. While a second attempt is presently being made in California, Minnesota established a correctional ombudsman in July 1972. The Minnesota correctional ombudsman differs from the proposed California version in one important respect: he is responsible to the governor, not the legislature.

Fitzharris examines the adequacy of existing grievance procedures available to inmates of the California state prison system. In his view the courts are over-burdened, slow, and reluctant to intervene in what they consider a responsibility of the executive branch. Most complaints are handled at the administrative level, but the author finds serious weaknesses here: the willingness and capability to resolve problems varies greatly from place to place, and a large proportion of prisoner complaints are made against the institution itself or its staff, and—not surprisingly—prisoners mistrust staff investigating themselves.

California law provides a third type of complaint channel: the right of prisoners to appeal to top executive-branch administrators and to members of the legislature. The author shows convincingly, however, that the governor and his staff, as well as legislators, do not have the time, expertise, and inclination to carry out their own investigation; invariably the agency about which the prisoner complained is asked to suggest an appropriate reply. These political channels of complaining therefore do not add significantly to the existing (and deficient) administrative complaint channels.

The author discusses the most important structural characteristics required for maximizing the benefits to be derived from a correctional ombudsman—such as his independence, his accessibility, the investigatory powers he needs, and the expertise required of an incumbent and his staff, as well as his jurisdiction and his powers to recommend and to make his findings public. The discussion emphasizes all essential characteristics of the institution of ombudsman, and his proposals are sound. The appended version of the California bill can be considered a good model for this type of institution.

The author has high hopes about the benefits that could be derived from the proposed office. He expects the ombudsman to stimulate communication and flow of information in prisons. In a more general way Fitzharris expects the ombudsman will improve the quality of prison life and prison administration. To an extensive list of "advantages of a Correctional Ombudsman" the author adds a small cautionary note: the institution of ombudsman should not be considered a panacea or a kind of structural penal reform (p. 70); yet he immediately adds that he considers his proposal a "giant step along the path of cooperation between the citizenry, penology professionals, and prisoners towards the desirable goals: prisoner rehabilitation, social reintegration and achievement of positive citizenship." Other detailed studies, however, should warn us against exaggerating the probable impact of ombudsmen on bureaucracies (e.g., L. B. Hill on New Zealand).

As an advocate of the correctional ombudsman, the author fails to examine thoroughly the question why one should prefer a correctional ombudsman over a general (state) ombudsman for all citizen grievances. He suggests that his proposal is a reform project in its own right and will more than pay for itself (for example, by eliminating troubles that might lead to riots). He also implies that since the state of California is too large for one ombudsman to handle all areas of complaint, it will require specialization. S. V. Anderson suggests in the Preface that contributions of general ombudsmen to the field of corrections have been worthwhile but modest. Even if all these arguments could be substantiated (and no serious attempt is made in the book) one cannot escape the conclusion that one ought to try a general ombudsman first before specializing. This proposal for specialists is partly the result of the increasing frustration which advocates of this humanizing institution are experiencing in the face of a remarkable skill of American politicians at preventing innovation in this field.

KARL A. FRIEDMANN
The University of Calgary, Canada

The Fight for Competitive Advantage: A Study of the United States Domestic Air Trunk Carriers. By William E. Fruhan. (Boston: Division of Research, Graduate School of Business Administration, Harvard University, 1972. Pp. 183. $9.00.)

Compared with the airlines of other countries or with other regulated industries within the United States, the U.S. airlines have met the needs of consumers fairly well. Yet the industry and its regulator, the Civil Aeronautics Board, have had more than their share of critical analysis. While Professor Fruhan is not violently critical, he still ascribes much of the industry's chronic problem of overcapacity to policies and decisions of the Civil Aeronautics Board.

Using regression techniques, Mr. Fruhan demonstrates that nearly all the factors which determine the relative profitability of an airline are beyond the control of the airline's management. Route structure, competition on specific city pair routes, and fare level and structure are all controlled principally by the Civil Aeronautics Board. Of the two key factors within management's control, Mr. Fruhan through his regression analyses minimizes the importance of aircraft type selection, leaving schedule frequency as the only quantifiable and important factor available to management to gain competitive advantage.

According to Mr. Fruhan, the airline which can gain a strong share of the market will gain a more than proportional share of the profits available in that market. Mr. Fruhan then asserts that the key to an increased share of the market is to increase scheduled flight frequency. Obviously, this results in competitive airlines steadily increasing flight frequency, in reduced number of passengers per flight and in financial crisis for the airlines, including risk of bankruptcy for the weaker carriers.

The Civil Aeronautics Board abets the process by bailing out the industry through fare increases when a financial crisis occurs. This prevents the process from becoming self-limiting as a result of financing problems.

Most observers will agree that overcapacity is the airlines' biggest problem. Many, however, will be less certain of the causation chain which Mr. Fruhan outlines. Mr. Fruhan does not admit to the re-equipment cycle as a cause of overcapacity at all, and includes the business cycle as an indirect cause only through its impact on financial availability. Yet the change of gauge involved in shifting from 30 passenger DC 3's to 100 passenger jets on the same route and then later shifting to 300 passenger wide body aircraft without reducing passenger schedule availability would seem to be an important cause of overcapacity. Unfortunately, Mr. Fruhan does not discuss alternate possibilities. Instead he asserts that his own causation chain is correct without proof other than that timing of the periods of overcapacity fits his theory.

There are some more specific problems with the book as well. Mr. Fruhan's statistical treatments are incompletely documented in the text, but some of his results present uncomfortably high R^2 results for a complex problem in the social sciences. They may be legitimite, but they lead to suspicions of problems with the technique. Further, there is an important inconsistency in that Mr. Fruhan asserts early in the text the "CAB's greater concern with the level of industry profits rather than individual carrier profits", yet makes individual carrier profit problems (the weakest carrier) the principal reason for the CAB's willingness to legitimize overbuys of aircraft through fare increases. This is his major thesis.

Recurrent overcapacity problems are of serious concern in the airline industry, and could easily become the reason for future drastic policy shifts by the government's regulators. Mr. Fruhan suggests an explanation for the problem, and discusses possible attacks on it. Although his explanation of the causation chain for the problem is different from other explanations, his recommendations are not at variance with other proposals.

ROBERT L. THORNTON
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U.S. Congressional Directories, 1798–1840. Edited by Perry M. Goldman and James S. Young. (New York: Columbia University Press, 1973. Pp. 417. $20.00.)

Every scholarly library will want to hold this collection of basic documents on the organization of the U.S. Congress for its first half-century. For some early Congresses, members are reported according to their place of Washington residence, in most instances a boarding house. Co-editor Young has already demonstrated (in his earlier book *The Washington Community 1800–1828*) how significant these groups were for Congressional outcomes.

Much information on early committee structure is now also made available in this compendium. This sort of data is incredibly hard to come by, and for students of early congressional history, and U.S. political history more generally, this volume comes as a godsend.

There is a short, informative introductory essay by the editors on the possible uses of data from the directories, and an account of their provenance.

NELSON W. POLSBY
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Education, Training, and the Urban Ghetto. By Bennett Harrison. (Baltimore and London: The Johns Hopkins University Press, 1972. Pp. 267. $10.00.)

Many political scientists—especially urbanologists and education, welfare, and manpower policy analysts—will want to read this book based on the author's prize-winning economics dissertation (The William Polk Carey Prize in Economics at the University of Pennsylvania). After examining the race, sex, education, training, individual earnings, employment stability, and occupational status of more than 50,000 persons in 16 metropolitan areas, Professor Harrison comes to the conclusion that the only way to improve life in American cities is to increase the income of urban minorities by remaking our economy. He argues that the quality of urban life cannot be enhanced without a reduction in crime, that crime cannot be reduced without drug control, and drug control cannot succeed without "massive job development."

Harrison's major thesis is that the economic conditions of urban minorities in general and the unemployment of nonwhites in particular cannot be improved by such things as more years of schooling, training programs, reverse commuting, or ghetto dispersal. He says, "Attempts to change the worker himself—whether to remedy his personal 'defects,' or to move him to a 'better' environment—have not worked in the past, and there is no evidence that they will work in the future" (p. 171). Instead, he offers two reasons why income levels of blacks and whites may differ: (1) market imperfections, e.g., monopoly or discrimination, and (2) a fundamental segmentation of the labor force according to class, race, and sex.

From his analysis of data collected in 1966 and 1967 by the Department of Labor and the Bureau of the Census, Harrison rejects the proposition that income differences between races can be attributed to real differences in productivity related to their education. He devotes considerable attention to showing that, in the present economic situation, there is little pecuniary incentive for nonwhite ghetto dwellers to increase their human capital by investing in education:

> The present value of the lifetime return to completion of four full years of high school is nearly $19,000 for whites, as compared with about $6,000 for nonwhites. . . . For whites, the risk of unemployment falls with years of school completed. . . . For nonwhites. . . the average effect of education on employment . . . is virtually zero. . . . Education helps members of both races to move into what are considered to be more prestigious occupations. But, once there, the nonwhites are systematically underemployed, receiving earnings which are hardly above the levels enjoyed in the previous position, and facing almost exactly the same expectations of unemployment as before. For ghetto whites . . . the occupational mobility . . . is translated into substantially higher earnings and significantly lower risks of joblessness (p. 68).

Moreover, he points out that the unfounded conventional wisdom which says that a person with more education will earn more money and attain higher status, may raise expectations that cannot be realized and, therefore, could result in serious frustrations.

Using Bluestone's concept of the segmented economy ("The Tripartite Economy: Labor Markets and the Working Poor," *Poverty and Human Resources*, July/August 1970), Harrison concludes that education does not provide a vehicle for mobility between the periphery of the economy (the welfare sector, the "hustle," the training sector, the secondary labor market) and its core. Furthermore, he claims training programs merely "recirculate the working poor among the segments of the periphery economy" (p. 148). His solution to improving inner-city life is not to affect the supply of ghetto labor, but to increase the demand for it.

Harrison refers to *Griggs* v. *Duke Power Co.*, 91 U.S. 849 (1971), as what may well be a landmark decision in reducing the market imperfection of discrimination disguised as over-credentialing, or requiring educational standards unrelated to successful job performance. Harrison endorses the Philadelphia Plan and Affirmative Action, but he makes it clear that even good-faith efforts to hire the hard-core unemployed in private industry will not "make a serious dent in the problem" (p. 178).

Thus he calls for the federal government, by supporting new towns in-town and community development corporations, to expand its delivery of desperately needed urban public services and to create millions of new jobs, pulling workers out of the periphery into the core of the economy. He regrets that only 0.3 per cent of the federal budget was spent on job development in 1969 and sees his proposal increasing wages in the ghetto by a factor of one to three.

More income may be a highly desirable, and even necessary, condition for better life in the inner city, but is it sufficient? Harrison's regression analyses do not show how economic variables affect political, sociological, and psychological ones; on the contrary, his causal path runs in the opposite direction. His assumption that more jobs mean better lives for urban residents may be similar to the assumption held by many, until challenged by the Coleman Report (*Equality of Educational Opportunity*, 1966), that more money spent on schools results in higher pupil achievement. Andrew F. Brimmer and Kenneth B. Clark remind us of the critical importance of other dimensions. Brimmer says, "Although a computer programmer may earn as much as (or more than) a high school principal, he clearly has less weight in the community's affairs" (in Andrew F. Billingsley, *Black Families in White America*, 1968, p. 174). And Clark emphasizes ownership, control, management, and independence as necessary ingredients of self-respect and self-worth (*ibid.*, pp. 168–169). Will public service jobs, even if developed to the extent recommended by Harrison, provide sufficient political clout and the human dignity to improve the lives of those who reside in the urban ghetto? I doubt it. Harrison's critical analysis of the modest economic impact of minority training programs is superior to his prescription for increasing the political power of ghetto residents.

JEDON A. EMENHISER

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Marihuana Users and Drug Subcultures. By Bruce D. Johnson. (New York: John Wiley & Sons, 1973. Pp. 290. $12.95.)

Drug Use in America: Problem in Perspective. Second Report of the National Commission on Marihuana and Drug Abuse (Washington: U.S. Government Printing Office, 1973. Pp. 481. $2.25.).

In America the drugs which are thought to be the most devastating in their effects are the opiates. Opiate dependency is a threat both to the user and to the fabric of society. A rational strategy to deal with this and other drug problems, including marihuana use, should begin with a systematic effort to acquire information. In recognition of the fact that drug use in all its varieties, including the legal, semilegal, and illegal, needs far more investigation than it has received, Congress in 1970 provided for the creation of a Commission on Marihuana and Drug Abuse.

In its recently published report, *Drug Use in America: Problem in Perspective*, the Commission indicates that public discussion of the drug phenomenon should include consideration of the moral, philosophical, historical, and political implications of drug use. Public policies and decisions should derive from these considerations. The Commission goes on to note, however, that not once in almost seven decades of policy making on the subject have such underlying assumptions been systematically evaluated and a broad, coherent foundation for policy making been established. Long-term strategies designed to minimize irresponsible drug use have not been pursued. The Commission has sought to formulate a policy-making process that includes all of the important variables and that disentangles various crucial issues.

The Commission's recommendations can be summarized as follows: American society should continue to discourage any drug use that occurs outside of medical channels or established social patterns. The primary concern of drug policy should be with disruptive or dysfunctional drug use. Drug policy should not be limited to illegal drug taking but should include dysfunctional use of legal substances, including alcohol and barbiturate drugs. Law enforcement can reduce but not eliminate the illegal supply of drugs. Elimination of the illegal market can occur only when a vigorous enforcement of the drug laws is joined with an effective program of reducing demand for drugs. The Commission further recommends that all drug-control functions of the federal government be consolidated in the hands of one agency—a recommendation which has since been partially realized by the creation of the Drug Enforcement Administration. Finally, the Commission cautions that the barbiturate drugs of today have become the functional equivalent of the freely available opiate drugs of the turn of the century. They constitute "America's hidden drug problem."

The final report of the Commission on Marihuana and Drug Abuse is to be praised for several reasons. To begin with, it correctly points out that public policy must reflect the complexity of drug-using behavior, including differences in the drugs themselves. The tendency to regard drug use as a unitary phenomenon has resulted in confusion and ineffective individual and governmental action. There is no one cause of drug use; rather, the phenomenon results from a complex interweaving of sociological and psychological factors.

The Commission report correctly indicates that crop eradication programs will not eliminate the availability of opium, cocaine, or marihuana. The United States by means of such programs may reduce the supply of drugs entering the international illicit market, and, therefore, reduce the supply entering this country. Total elimination of opium, cocaine, or marihuana cultivation, however, is impossible. It should be remembered that there are compelling political reasons for the government's insistence on a crop eradication program. It is difficult to imagine a president launching a serious effort to constrain heroin use while ignoring the foreign cultivation of opium as well as the foreign manufacture and importation of heroin.

The Commission's final report can be criticized for contending that the present system of drug control should be retained, with adjustments, because the present controls result in a net benefit for society; i.e., they result in a reduction in the social cost of drug use that outweighs the cost of control itself. This is debatable. Efforts at the social control of drugs, including reducing the supply, increasing the social or economic cost, and instituting stiffer penalties for use do have an impact on use patterns but they may well result in far more serious secondary social maladies. The Commission itself seems to recognize this possibility by stating in another context that public policy "as presently designed, is premised on incorrect assumptions, is aimed at the wrong targets, and is too often unresponsive to human needs and aspirations" (p. 8).

The Commission recommends that the government continue to prohibit heroin maintenance as a treatment modality. It can be argued, however, that there are sound reasons for trying heroin maintenance on an experimental basis. An argument in its favor is that heroin maintenance might attract two important groups of opiate users: (1) those who do not succeed on methadone maintenance, and (2) those who do not initially volunteer for methadone maintenance. If heroin maintenance were instituted, addiction-related crime would surely decrease because the costs of the heroin habit would be eliminated. Moreover, the lessened chances of infection, overdose, and imprisonment would represent an important benefit to the addict. It is also possible that heroin maintenance would remove the impetus for market expansion caused by the high profits of illegal distribution.

Many commentators on the American drug scene, including the authors of the Commission's final report, are convinced that the laws which govern drug-using behavior in the United States have important consequences for the prevailing patterns of drug use. Bruce Johnson's study, *Marihuana Users and Drug Subcultures*, is based upon 3,500 questionnaires sent to students and friends of students at 21 colleges located predominantly in the New York City area. These questionnaires sought to explore patterns of drug-using behavior through the use of subculture theory. Each chapter of the book employs empirical data to demonstrate what the consequences of the present drug laws are on drug-using behavior.

The theme of Johnson's book is that involvement in cannabis selling, and not the use of marihuana, is the principal factor which leads to increased participation in subcultures of drug use. It is participation in drug subcultures which explains why marihuana is linked with various unconventional activities. Johnson believes that the present drug laws in the United States "play a fundamental role in structuring and organizing subcultures of drug use" (p. 205).

The relationship between cannabis and the use of more potent drugs is another major aspect discussed in the book. Johnson concludes that there is nothing to indicate that the use of marihuana is a causal factor responsible for the use of more dangerous substances. Rather, the social needs of the individual must be examined in order to understand why some individuals do use other drugs. According to Johnson, it is the sale of cannabis which facilitates association with subcultures that are involved with dangerous drugs. Transition to the use of such drugs would be a consequence of this association rather than an inherent effect of cannabis. After the individual has become a seller of cannabis and has become immersed in a drug subculture, the drugs which he chooses to experiment with are determined primarily by socioeconomic and sociocultural factors. The factor of being a seller of cannabis is important in determining the degree of a person's involvement with and commitment to the use of other drugs.

Johnson believes that the present drug laws, which define marihuana use as illegal, are an important element in holding drug subcultures together. Legalizing cannabis would, he believes, undermine both the illicit drug market and the social processes by which students are recruited into, and maintained in, drug subcultures. The first report of the Commission on Marihuana and Drug Abuse called for a decriminalization of marihuana use. This recommendation recognized, as does Johnson, that there are secondary social maladies that can result from the implementation of archaic drug laws.

The final report of the Commission on Marihuana and Drug Abuse, as well as Johnson's book, should be of interest to everyone concerned with finding solutions to America's multifaceted and complex drug problems.

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People of Paradox: An Inquiry Concerning the Origins of American Civilization. By Michael Kammen. (New York: Alfred A. Knopf, 1972. Pp. xvii, 316. $8.95.)

This ambitious volume represents still another quest for the American national character or, as the author prefers, "style." Unlike most earlier similar undertakings, however, it is concerned less with questions of definition than with the problem of origins, and all but two brief concluding chapters are devoted to a discussion of seventeenth- and eighteenth-century developments. Erik Erikson's proposition, in *Childhood and Society*, "that a nation's identity is derived from the ways in which history has, as it were, counterpointed certain opposite potentialities" (p. 97), supplies a ready-made frame of reference, while the central argument, that Americans have from the beginning been "uniquely a people of paradox" (p. 106), repesents an elaboration of Erikson's further suggestion that the United States has subjected "its inhabitants to more extreme contrasts and abrupt changes" than most "other great nations" (p. 97).

Why Americans "have been unusually and profoundly perplexed by ambivalence and contradictory pulls" (p. 107), may be traced, according to the author primarily to several related conditions that, he suggests, distinguished the English colonies from those of other European nations in America. First, the English colonies were "offshoots of a deeply divided society" (p. 139) that was "peculiarly racked by tensions and contradictory tendencies" (p. 113) that were inevitably transported to America. Most of the contradictions had been resolved in the parent state by the early eighteenth century, but two powerful conditions combined to give them continuing force in America. First was the problem of establishing the legitimacy of institutions, patterns of belief, and standards of behavior in all areas of colonial life, a problem that presumably was greater in the English colonies because of looser controls exerted by the metropolis and a more rapid pace of social change. Second, and much the most important stimulant of ambiguity and conflict, was the "bewildering, fluctuating pluralism" (p. 94) that, increasingly during the eighteenth century, resulted from the growing demographic, cultural, and economic diversity of the colonies. By exacerbating longstanding dualisms and spawning a welter of new biformities, polarities, and paradoxes, this diversity, the author argues, had made colonial society profoundly unstable by the eve of the Revolution. Subsequently, the Revolution created as many incongruities as it helped to resolve, and over the next century "antimonies inherited from colonial times endured" while "a new garden of dualisms blossomed," as disengagement from Europe permitted the American "penchant for ambi-tendencies to continue apace" (p. 250).

This series of hypotheses is engagingly set forth and interlaced with a number of valuable and original insights about specific aspects of early American cultural development. The case made for them is not, however, a compelling one. The author leads the reader so hurriedly past so many biformities and paradoxes that one puts the book down without any certain sense of how all of these illustrations, many of them quite cursorily explored, fit together or what they might mean. More important, the author's reading of the tensions and contradictions does not always help to dispell the doubts created by these occasional failures of elaboration and analysis. Thus, his assertion that the colonies "became *less* stable with the passage of decades" (p. 63), so crucial to his argument, may in some ways be true for some colonies but is clearly not true for most; and it rests on no firm data base (indeed, one has yet to be systematically put together) but only on the general assumptions that diversity is usually productive of social tensions which in turn result in social instability—an assumption, of course, that must be tested in every specific situation. For the colonies, an equally strong case could be made for precisely the contrary view: that, in the aggregate, tensions arising out of diversity were usually not dysfunctional and that, to borrow a happy characterization from the author, the "invertebrate" character of society helped to provide a functional release from anxieties and frustrations generated by the constrictions of tradition and social space that continued to exert a powerful influence in the more settled and less open societies of western Europe. By pushing the colonists toward compromise and hybridization, moreover, pluralism may well have helped them to learn, perhaps much more quickly and with less emotional pain than their European contemporaries, to cope with the vast social changes generated by the economic and social expansion of the early modern western world and thus to become a peculiar source not of weakness but of strength.

This is not to suggest that the colonists fashioned a homogeneous utopia out of such diverse materials but only that they did produce a society with a high degree of stability and a remarkable capacity for adapting to change, and that the existence of so much diversity could only have accelerated the process. In his conclusion, the author suggests that over the last century the counterposition of the many contradictory tendencies within American society has given it "the ultimate stability of an arch" (p. 297). But that achievement, like most of the tensions which underlay it, was already vividly manifest during the last years of the colonial period and the era of the American Revolution.

Jack P. Greene

The Johns Hopkins University

Essays on the American Revolution. Edited by Stephen G. Kurtz and James H. Hutson. (Chapel Hill, N.C.: The University of North Carolina Press, 1973. Pp. 320. $12.50.)

Bernard Bailyn, fittingly accorded first place in this excellent collection of essays by eight historians, begins his essay with this true statement: "The American Revolution not only created the American political nation but molded permanent characteristics of the culture that would develop within it" (p. 3). This book demonstrates that those from whom we can learn about this crucial political event are historians not political scientists.

The first and last essays, by Bailyn and Edmund S. Morgan, deal with the broad questions of the causes of the Revolution and its consequences. The other six essays are by Jack P. Greene, Richard M. Brown, John Shy, H. James Henderson, William G. McLoughlin, Rowland Berthoff and John M. Murrin, and deal with more specific aspects of the Revolution. No attempt is made to be comprehensive, nor does a single, consistent interpretation emerge. But the essays do speak to each other, though not overtly or argumentatively, so the book is more than a collection of essays related only by an overarching theme.

To demonstrate how this exchange takes place (and since it is clearly impossible to do justice to the essays in any summary fashion), let me examine one theme announced by Bailyn that runs through several of the essays: the question of the role of ideas in the Revolution, "how ideas and beliefs relate to what men do" (p. 5). Bailyn believes that particular ideas understood in a particular way "may be understood to have lain at the heart of the Revolutionary outbreak and to have shaped its outcome and consequences" (pp. 11–12). These ideas are not so much the "noble ideas of the Enlightenment and the abstracted details of constitutional law" but an "elaborate pattern of middle level beliefs and ideas" that came to have an "instrumental grasp on [the colonists'] minds" (p. 8). In particular, this pattern of ideas derives from Great Britain, from "the voluminous writings of the Exclusion Crisis" and "the literature of the Glorious Revolution." Bailyn is reluctant to point to Locke as the fountainhead of these ideas and instead emphasizes the importance of "the writings of the early eighteenth-century" that opposed the government of Walpole (p. 8). These ideas "would most specifically determine the outbreak and character of the American Revolution . . . " (p. 9). Bailyn does not see ideas as "epiphenomenal, superstructural, not the shapers of events but their rationalizations . . . " (pp. 10–11). The ideas and beliefs of the colonists *did* shape their response to British "acts of power," and under the pressure of events these ideas were refined and made more coherent and consistent. The final result of this process of distillation is the crucial theoretical passage of the Declaration of Independence, which begins with the announcement of the self-evident truth of man's equality.

Jack P. Greene also speaks of "a hard core of unchallenged beliefs . . . [that] formed the central premises for an emerging colonial perceptual system" (p. 53–54). Greene correctly if cautiously suggests that "it is probably somewhat misleading" for Bailyn to argue that there is a sharp distinction between the ideas of the Glorious Revolution and those of the opponents of Walpole: "[the latter are] really only a more pure, intense, and uncompromising version of the same central ideas . . . " (p. 53). The line of descent may at times be unclear, but the Declaration of Independence is nonetheless the offspring of Locke (and the political works of Locke provide the most adequate gloss on the Declaration and on that pattern of ideas and beliefs found in the pamphlets of the pre-Revolutionary period).

Greene, however, finds the key elements of the "basic substructure of [colonial] expectations" not in the colonists' speech but in their deeds. In their "actions" and "behavior," Greene finds a radical individualism which sees political society as protective of the individual and his interests but also as "facilitative and positive in that it encourages . . . the individual to pre-occupy himself with his own goals without forcing him to be much concerned with . . . the community as a whole" (p. 57). According to Greene, the colonists do not speak their views because of the public unacceptability of such a position. But of course, the position had been publicly stated by John Locke, although stated in such a way as to seem acceptable.

Greene locates another "elusive component of this implicit structure of expectations" (p. 59). The good man is the automonous man. "Wilderness conditions . . . had early forced men to rely heavily upon their own resources. Mastery of environment . . . had to an unusual degree required mastery of self . . . " (pp. 59–60). Virtue is therefore defined as mastery of "one's own passions," a means for the mastery of nature. Although Greene does not draw the connection, virtue so understood was not only a necessity of "wilderness conditions" but a part of Calvinist teaching. As William G. McLoughlin shows in his essay, "The Role of Religion in America," even after the Revolution, "Americans were clearly committed to the establishment of a Protestant Christian nation" for "they were convinced—all Americans were convinced—that the safety and prosperity of the nation depended upon inculcating into its citizens the fear of God and the love of one's neighbor" (pp. 209, 210).

These are the central elements of what George Grant has called "the American primal": the new political and moral science expounded by Locke and distilled in the Declaration, the new physical science by which man will master nature, and that Protestantism which embraced both the new political and physical science and provided the needed support for man's mastery over himself (virtue). McLoughlin is aware of "the scientific challenge to the heart of Protestant faith" (p. 254) but just as serious a challenge was posed by the new political science. As he says, "Hosts of people simply refused to believe any longer that the duly constituted authorities in church and state knew best what was right for them . . . " (p. 201). But as Tocqueville knew, it is only one step from the doubt of human authority (because all men are equal) to the doubt of all authority, human or divine. It is no accident that the supreme doubter, Descartes, a founder of modern science, is the first philosopher to proclaim in 1637 the equality of man. A similar pronouncement is made fourteen years later by the founder of modern liberalism, Hobbes.

Edmund S. Morgan concludes the final essay by speaking of America's commitment "to the creed of equality," a commitment "that invited conflicts and still invites them . . . " (pp. 308, 309). Morgan is correct, but the conflicts he envisions are between those seeking "their right to equal treatment: and those blocking such access." The more profound conflicts inhere within "the creed of equality" itself. What are the imperatives of that creed? Berthoff and Murrin in their essay force us to consider one problem inherent in the creed of equality: is the goal only "political parity" or is it also "the social equality of persons" (pp. 282–285)? That problem is still with us and constitutes the most important debate taking place in America today. Some Americans have "face[d] the possibility that their liberty —their freedom to compete—was undermining their equality" (p. 283). To be "truly equalitarian" it may be necessary to be less committed to one of the rights that all men share equally, the right of liberty.

The Revolutionary period is of penultimate importance because the creed of equality which is both America's glory and her fate was formulated then. These essays provide an excellent introduction to our understanding of that glory and fate.

Richard Crosby

Colgate University

The Rise and Fall of the People's Century: Henry Agard Wallace and American Liberalism, 1941–1948. By Norman Markowitz. (New York: The Free Press, 1973. Pp. 416. $8.95.)

This book is neither a comprehensive biography of Henry A. Wallace nor a satisfactory explanation of the rise and fall of an imaginary people's century. With only a brief survey of Wallace's life and career prior to 1941, which synthesizes superficially what others have covered more thoroughly, Professor Markowitz concentrates upon those years the Iowa agrarian served as vice-president, secretary of commerce, editor of the New Republic, and presidential candidate of the Progressive Party. In the context of Henry Wallace's total public career, this period constitutes his less than glorious years.

In the realm of agriculture Henry Wallace was an expert with few peers. Knowledgeable in matters of farm policy, and an adept phrase maker, he could transform his political rhetoric into practical reality. Abstruse terms such as "ever normal granary," "economic democracy," and "social discipline" were given concrete meaning while he was secretary of agriculture. The first was implemented via the storage of food and fiber by the Commodity Credit Corporation; the second was achieved to a degree by the county committee system of the Agricultural Adjustment Administration; and the third succeeded by using the acreage allotment-price support mechanism to demonstrate to farmers the collective advantage of limiting their individual decision making in favor of considerable federal management of agriculture.

It was another story when it came to matters of foreign policy. In this area Henry Wallace could not transform clever words into meaningful programs. His catchy phrase "the century of the common man" was a slogan without substance. Attainment of this abstract ideal was as elusive as Franklin D. Roosevelt's "Four Freedoms"; Wendell Willkie's "one world"; or Woodrow Wilson's world made "safe for democracy." To achieve a modicum of international stability, let alone a century of peace, required a type of tough diplomacy practiced by Henry Kissinger and not the actions of an amateur like Henry Wallace.

For all his good intentions and years of public service, Wallace was not a good politician. His personal Watergate was in allowing those around him in 1948 to get control of the Progressive Party machinery and, in so doing, force him into untenable positions before the American electorate. Wallace so deeply believed in the Sermon on the Mount as a moral guide, both in personal and international relations, it was difficult for him to consider the possibility that men and nations do deceive one another.

The author hails Henry Wallace as the leader of "social liberals" (p. 2) who desired the attainment of world peace and universal abundance. These noble goals were thwarted, according to the author, because of the obstruction of turncoat New Dealers, Cold-War liberals, red-baiting anti-communists, Harry S. Truman, and assorted fascists. On the domestic scene even F.D.R. comes in for censure since "social liberals were in fact prisoners of New Deal broker-state politics and the Roosevelt personality cult" (p. 325). Markowitz reacts uncritically to the problem of credibility confronting Wallace arising from his constant need to apologize for the bellicosity of the Soviet Union or to defend the dogmatic pro-communist stance of the Progressive Party.

Henry Wallace deserted his so-called Gideon's army over the issue of the Korean War, and as a result his armor seemed a bit tarnished by those awaiting the advent of the elusive people's century. Thus the author comments: "Henry Wallace, spending his final years in virtual exile at his South Salem farm, would observe the ensuing foibles and dilemmas of the cold-war liberals with faint interest, preferring instead to return to God, strawberry planting, and a celebration of the eternal verities of rural life" (p. 297). While this description is deprecatory in tone and overlooks his useful work on behalf of undeveloped nations, it does, however, point out why Wallace did not fit in with the urbane and sophisticated clique that constituted the far left of that era.

In this book the author makes use of other studies as well as a host of manuscript sources and interviews. Despite its polemic style, doctrinaire assertions, and unwarranted attacks on liberals who opposed the Progressive Party, this work still has value. It does reveal the mentality of the radical left of the post-World War II period and explains in part the rationale for their ideology. Further investigation might indicate that the "1948 Progressives" were in fact the advance guard of today's New Left.

EDWARD L. SCHAPSMEIER
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Another Look at the Twentieth-Century South. By George E. Mowry. (Baton Rouge: Louisiana State University Press, 1973. Pp. ix, 90. $4.95.)

This brief volume contains three lectures presented as part of the Louisiana State University Walter Lynwood Fleming Lectures in Southern History. Not being a specialist in southern history, Mowry proposes to comment upon "some relatively unexplored corners of southern culture from, perhaps, an angle of vision not ordinarily utilized by the many distinguished historians of the section" (p. vii).

In the first chapter, Mowry challenges the notion that southern history is distinctive—a contention no doubt novel and pehaps unwelcomed at a southern history lecture series. Mowry argues that excluding the presence of blacks, the South was similar to the western Middle West (Iowa, Kans., Minn., Mo., Nebr., N. Dak., and S. Dak.), especially between 1900 and 1940. The similarities noted are agrarian economies, predominance of fundamentalist religious sects, poverty, one-party politics, galleries of demagogues, and common features of folk culture and literature. While a number of likenesses are demonstrated, the presentation is unlikely to convince readers that the politics or history of the South is not unique since it is questionable whether the presence of millions of blacks can be discounted in understanding the region, as Mowry seeks to do. Nor does he consider the impact on the region of losing the Civil War and subsequently experiencing Reconstruction. Perhaps the thesis presented in skeletal form in this book will inspire someone to undertake a thorough investigation of the similarities of southern politics with those of other regions, rather than concentrating on differences. Research along those lines would advance an understanding of the impact of blacks and the Civil War on regional politics.

While the first chapter poses an interesting proposition, the last two chapters are less evocative. In them Mowry explores what he calls the paradox of southern political conservatism. In chapter 2 the political careers of three southern senators are used to illustrate the gradual disaffection of the southern wing from the national Democratic party. Mowry concludes, not surprisingly, that the public stands of these men were conditioned less by ideological or partisan bonds and more by concern about re-election and a desire to maintain the socioeconomic and racial status quo in their region.

The author suggests that southern support of New Freedom proposals and most New Deal measures prior to 1937 represented (1) a trade-off for regional control of racial matters, (2) a trade-off for regional gains from other programs, or (3) a response to constituent demands. While the author seems inclined more toward the first possibility than the others, he presents little documentation for any of them. Nor does he adequately explain why southern support for the New Deal declined after 1937.

By raising the possibility that southern support for reforms altering the nature of American federalism occasionally came in return for northern Democrats' acceptance of southern racial and labor practices at variance with democratic norms, Mowry seems to undercut the logic of his first chapter. What sense does it make to show that the South is similar to seven midwestern states, once blacks are excluded, if the primary independent variable used to explain the political and cultural attributes of the South is the presence of a large black population? Indeed the thesis of the first chapter is at least partially refuted by the material in the second and third chapters.

The chief value of this slim set of lectures will be realized if they prompt political scientists and historians to undertake analyses of the propositions so fleetingly presented.

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The Role of Analysis in Regulatory Decision-making: The Case of Cable Television. Edited by Rolla Edward Park. (Lexington, Mass.: Lexington Books, 1973. Pp. 112. $9.50.)

Cable television transmission began simply as an expeditious technique for the dissemination of an improved television signal to receptive customers. As the infant cable industry took hold, several changes in its scope and operating procedures were to force a reluctant FCC to intercede in its development. The expansion of cable's activities into such areas as program production, brokerage of air time, pay-TV, and the importation of distant TV signals clearly called for government regulation.

Despite its present status as a viable communications medium, cable television functions against a supercharged backdrop of conflicting claims, controversy, and tightening government controls. Industry advisories proclaim that franchises are being awarded in increasing numbers, whereas trade and financial publications report cutbacks in the construction of cable systems (especially in large cities), sluggish subscriptions and mounting opposition to pay-TV by open circuit broadcasters. Adding to the turbulent atmosphere are the prospects of a new and perhaps restrictive copyright law and the imposition of more stringent federal, state, and local regulations.

Rolla E. Park, senior economist at RAND, has attempted an orderly review of the chaotic growth of this communications system, which is trying so hard to live up to its public relations releases. He has asked eight economic experts—from government and research foundations, from the FCC, and from the industry itself—to share their views on the role played by economic analysis in the drafting of regulations governing cable television. The volume is an outgrowth of a panel discussion held at the Western Economic Association in August, 1972.

Among the questions Park has directed to this panel are: What impact has cable had upon the broadcasting industry? How has your own analysis changed over time and because of the influence of other observers? To what extent has FCC policy been shaped by various analyses? How does the analysis of an "interested" party compare to that of an outside expert?

Not all of the questions are answered by each of the eight contributors, but most are touched upon. Park's summary of their conclusions indicates that the 1972 FCC regulations, which were to be crucial to cable's growth and control, were only slightly influenced by the findings of economic analysts. The regulatory agency tended to regard their data with suspicion because many analysts were directly involved in the industry and therefore were incapable of producing objective reports. Their analyses were frequently tailored to favor the company who hired them and to undermine its competitors.

As evidence of the minimal role played by analysis in the decision-making process, Park cites the fact that final formulation of the FCC regulations was based primarily upon compromises between individual commissioners and contending parties. Furthermore, in opting to implement its cable rulings on a short-term basis, subject to frequent revision, the Commission clearly chose to ignore long-range, reasoned analysis.

On the plus side and in summary, Park notes several ancillary contributions made by economic analysts. Their reports provided a framework for FCC deliberations and an empirical foundation for policy formulation. Their meticulous research lent respectability to the industry, broadening awareness of its importance. And their analyses helped to dispel the FCC's concern that cable transmission would seriously harm UHF stations.

This volume may serve to help continue the necessary research and study to bring about stability and orderly growth to an industry that in itself contains unmeasured potentialities.

NATHAN SHOEHALTER

Rutgers University

Implementation: How Great Expectations in Washington are Dashed in Oakland; Or, Why It's Amazing that Federal Programs Work at All, This Being a Saga of the Economic Development Administration as Told by Two Sympathetic Observers Who Seek to Build Morals on a Foundation of Ruined Hopes. By Jeffrey L. Pressman and Aaron Wildavsky. (Berkeley: University of California Press, 1973. Pp. xviii, 182. $7.50.)

When the Free Press of Glencoe, a fledgling publisher of social science, published Edward C. Banfield's *Government Project* in 1951, the blurb declared it "an astonishing fact that this is one of the very first studies to describe the impact of big government on the lives of ordinary citizens." There has been many a government project since then and many a government contract awarded to a consultant to study the results, yet political science still has had astonishingly little to say about government in action, at the point where it finally touches the putative beneficiaries of its work.

When social scientists do get interested in government at work, they seem to be drawn to instances of its not working very well. Professors Pressman and Wildavsky's *Implementation*, like Banfield's *Government Project*, is not a success story. It recounts the failure of the federal Economic Development Administration to create jobs for the ghetto poor with grants and loans for public works in Oakland in the late 1960s. After five years, most construction projects either had not been completed or had not yielded jobs for the hard-core unemployed. In the authors' key term, EDA's program was not "implemented." Starting with a "policy," which points "to a chain of causation between initial conditions and future consequences," EDA was able to translate policy into a "program," the first link in the causal chain, but it lacked "the ability to forge subsequent links in the causal chain so as to obtain the desired results." This ability is implementation (p. xv).

EDA came to Oakland in the spirit of the impatient, agitated liberalism of the 1960s—guilty about the squalor of the ghettos, drawn by the potential for riot, and too eager to claim, when riots did not occur, that federal good works had prevented them. EDA's Oakland project epitomized its time. More than $20 million in federal funds were to be concentrated in a middle-sized city to "demonstrate" results. The poor were to participate. The city government was kept at arm's length to establish federal *bona fides* with local blacks. Social science was enlisted via a $400,000 contract with Floyd Hunter to study the city's power structure. Bureaucracy was to be circumvented. Indeed, it is misleading to describe the project as an undertaking of EDA. It was more the personal commitment of Eugene Foley, an assistant secretary of Commerce; Amory Bradford, a former vice president of the *New York Times*, who was Foley's special representative in Oakland for the first few months of the project; and a small task force they assembled. EDA had been spending its funds mainly in rural areas and small towns, as Congress intended. It was Foley, newly appointed in 1965, who sought to take it into the inner city, and he left office the next year.

All of this raises the question of how much is to be learned from the case. In some measure, *Implementation* is a tale of the times. The moral of the tale, which was widely drawn as the 'sixties gave way to the 'seventies, is that people should not expect too much of government. The authors help the reader to adjust his expectations downward. They say they would consider their effort a success "if more people began with the understanding that implementation, under the best of circumstances, is exceedingly difficult." Were this seen to be so, people would be pleasantly surprised when a few good things happened (p. xii–xiii).

The core of the demonstration of how difficult it is for government to achieve objectives is Chapter 5, "The Complexity of Joint Action." The authors identify thirty decision points from the time Foley decided to take EDA into the cities in the fall of 1965 to the letting of contracts in Oakland for construction of a hangar in June 1971. A "decision point" occurs each time an act of agreement has to be registered for the program to continue. The cumulative total of agreements needed, taking account of the number of participants in the decisions as well as the number of decisions, was seventy, on modest, reasonable assumptions about both participants and decisions. Assuming an 80 per cent probability of agreement at each decision point, the probability of success after seventy clearances is .000000125. Assuming a 99 per cent probability of agreement, the probability of success after seventy clearances is still less than .50.

After this ingenious demonstration, one wonders how anything gets done. Pressman and Wildavsky acknowledge that this is a puzzle, inasmuch as the landscape is full of government programs that have evidently been implemented.

Yet in the end they argue that among federal programs involving localities, "The EDA public works program is a bedrock example: almost any other one we could think of would involve a greater conflict over ultimate ends or more public dispute, less adequate resources in view of the end contemplated, and other features that make it evident why programs fail" (p. 123).

They explain the existence of many apparently successful programs as follows: "The years of trial and error that led to the present state of operation are lost from view. The huge amount of resources that may have been poured into different alternatives before one caught on is conveniently part of past history for which we are not responsible. Programs that started out to accomplish one set of objectives end up accomplishing another for which, long after the fact, we give undue credit for implementation." In short, "A basic reason programs survive is that they adapt themselves to their environment over a long period of time" (p. 116).

More precisely, the formal organizations that administer the programs adapt. I suspect that the distinction is important for interpreting the case. If federal programs involving localities are to flourish, a cooperative link must be forged across the formal division of the federal system between permanent bureaucracies pursuing common goals at different levels of government. The EDA had no local bureaucracy as a counterpart and ally in Oakland. Moreover, the project was an aberration for EDA, requiring a redefinition of constituencies if not of objectives. To the extent that programs depend on politics, EDA's Oakland project was indeed a bedrock example. There was no controversy. Implementation should have been easy. But to the extent that programs depend on the performance of bureaucracies, difficulty was highly predictable and perhaps more likely than usual. The federal administrative organization was not committed to the project, and there was no local administrative organization. The authors acknowledge that comparison with other programs would be desirable, but say they were "hampered in this respect because we have been unable to locate relevant research. None that we know of attempts to deal with the problems posed by long sequences of decisions involving multiple participants, each dependent on what has gone before and unable either to control the others or to predict the probability of successful outcomes at succeeding points . . . " (p. 124).

Two additional analytical chapters follow that on the complexity of joint action. Chapter 6, "Learning from Experience," criticizes "going outside the bureaucracy" and "coordination" as nostrums and closes with a plea for policy makers to take the difficulties of implementation into account. The final chapter criticizes the economic theory underlying EDA's intervention in Oakland and argues the merits of subsidizing labor rather than capital as a way of raising employment in the inner city.

This is a salutary book at a time of rising interest in policy studies. It is a reminder that the distinctive competence of political scientists lies in explaining the processes by which public purposes are, or fail to be, defined and achieved. It focuses in a systematic way on the achievement of policy, which American political scientists have given short shrift by comparison to formulation, in domestic affairs at least. The authors plainly mean to be breaking a path, and if the path is not wholly new (I find it hard to distinguish between "implementation" and "the administrative process"), it surely needs more mapping of the kind that is usefully begun here.

The book is the second in the Oakland project series, following Arnold J. Meltsner's *The Politics of City Revenue*—the Oakland project, that is, of the University of California, under Wildavsky's direction, through which faculty members and students have worked in city government.

The illustrations are by Rube Goldberg.

MARTHA DERTHICK

The Brookings Institution

The Politics of Environmental Concern. By Walter A. Rosenbaum. (New York: Praeger Publishers, 1973. Pp. 298. $9.00.)

This is an important book about an important public issue. Professor Rosenbaum argues persuasively that environmental issues are the most critical concerns facing this nation and indeed every national political system. His analysis of "ecopolitics" follows the traditional Lasswellian formula of "who gets what, when, and how." The author shows great sensitivity to the political dilemmas facing ecologists and what he calls the pikes upon which politicians may impale many of the demands of environmentalists, e.g., the tradition of incrementalism, the role of organized interests in public policy, the diffusion (or fragmentation) of power, and the burgeoning "administrative state" as ultimate interpreter of public policy. The book also provides important insights into the larger public policy-making process of which ecology is but one application, as well as an informative examination of the politics of air and water pollution, timber, surface mining, and solid waste. The analysis provides a fine historical perspective on the scope of federal government activity in these issues.

Despite these laudatory efforts, however, I think the book has two basic limitations. One, notwithstanding the intrinsic importance of the environmental issue and the major public policy advances of the 1960s and early 1970s, this concern is quickly losing ground to America's cultural dependence upon the "unlimited" availability of power and energy. Rosenbaum's book, therefore, although written (or completed) in 1972, has already become dated, in 1973, as the "energy crisis" very rapidly seizes central prominence in American politics—unless one views the current energy crisis as reflecting the same fundamental concern of ecologists (both new and old) for the wasteful depletion of our limited natural resources. (In this way, the semanticist might effectively argue that the energy crisis is merely one manifestation of the environmental decade.)

These two crisis situations (energy and ecology) are on an apparent collision course and policy makers must inevitably choose between policies that will further develop certain sources of energy (e.g., soft coal and shale oil) at the expense of environmental interests and policies that will permit (or even encourage) a decreased utilization of energy and adhere to a strict environmental credo. I think one could have predicted that public policy resulting from such confrontation would proceed in a direction counter to the environmentalists. Although Rosenbaum does acknowledge the possibility of a displacement of the ecology issue as a major public concern, "especially when the costs, direct and indirect, become more tangible" (p. 16), his description of the 1970s as the "environmental decade" characterizes very clearly the book's tenor and theme.

The second limitation stems from the exaggerated importance accorded by Rosenbaum to the concept of "planning." In examining the complex array of organized governmental and nongovernmental interests that have a stake in environmental policy, the author consistently decries the lack of coordination and comprehensive planning, suggesting that "national environmental planning that is comprehensive, coordinated, and sensitive to government's own environmental impact constitutes the framework of the ecologist's most sweeping political objectives" (p. 255). Although he correctly points out that there are massive political obstacles to the implementation of environmental plans, he shows little appreciation of the limited state-of-the-art of planning as a means of ameliorating social and political problems.

In the field of health policy, for example, we have come to realize that planning has certain inherent limitations even under optimal political conditions. Thus despite the enactment of federal legislation in 1966 establishing an elaborate system of comprehensive *health* planning, individuals concerned with the issues of quality, cost, and the availability of health services are still groping for a mechanism to counter the extreme fragmentation in the health care system. Rittel and Webber have succinctly captured the essence of this problem in a recent article on "Dilemmas in a General Theory of Planning," *Policy Sciences*, 4 (June 1973), 155–169. They point out, for example, that there can be no definitive formulation of a "planning-type problem": "The process of formulating the problem and of conceiving a solution (or re-solution) are identical, since every specification of the problem is a specification of the direction in which a treatment is considered" (p. 161). Moreover, there can be no immediate or ultimate test of a solution to a planning-type problem because "any solution, after being implemented, will generate waves of consequences over an extended —virtually an unbounded—period of time" (p. 163).

Rosenbaum's oversimplification of a very complex process is compounded by his recommendation for establishing a cabinet-level federal agency to "coordinate" national environmental policy. Apparently, he bases this on the assumption that federal *departments* are better able to bring about a rational and coordinated national policy on substantive social issues. That bureaucratic pluralism and overlap do not disappear with reorganization and establishment of a new superagency is clearly evident in the Department of Health, Education, and Welfare—where periodic internal reorganization has become a way of life.

Thus, while environmental issues are admittedly grave and pressing, it is shortsighted to expect that their resolution will take place in a manner generically different from that of other political issues, i.e., external to the traditional system of functional checks and balances. To the extent that planning is proffered as a means of addressing environmental issues in light of our national values and goals, it must be recognized as a limited tool essentially unable to delineate the boundaries of the environmental problem. I do not advocate sitting back and waiting for the technology to develop. But certainly it would have been more relevant if the author had demonstrated greater appreciation of the impracticality of expecting major new policy breakthroughs as a result of comprehensive environmental planning.

HARRIS S. COHEN

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S.D.S. By Kirkpatrick Sale. (New York: Random House, 1973. Pp. 752. $15.00.)

Students for a Democratic Society (SDS) was the best-known and the most influential organization in the New Left. The author describes the evolution of SDS from its infancy as a stepchild of the League for Industrial Democracy in 1960 through the tumult of that decade to its disintegration into warring vanguards by 1970. He provides color and dimension by presenting a wealth of new detail about the personalities of SDS leaders and the complexity of a volatile national group of early reformers and latent revolutionaries. The influx into SDS of Communist Progressive Labor Party members and disaffected youth with a pronounced anti-intellectual bent after 1965 is noted in some detail by the author. Moreover, his account of the community organizing and anti-Vietnam war phases of SDS history is factual and comprehensive. The book gives the reader many insights into intraorganizational factions and stratagems as well as new and original material about the extreme variation in administrative competence within SDS, and it explains how successive ranks of members viewed SDS and its mission. Sale's bibliographic references are exhaustive and complete. The appendices include the early history of the League for Industrial Democracy, a chronological roster of SDS officers, and the organization's constitution with appropriate additions and deletions.

Although the author brings to his task the gift of vivid exposition, his analytical ability does not keep pace with his journalistic skill. This work contains minor and major flaws of interpretation and evaluation. Sale says, for example, very little about the philosophical motivation of SDS members—the books they read or the mentors who framed their views of society and helped stimulate a regeneration of American radicalism. Yet the few intellectuals who called attention to political and social issues were widely read by the young men and women who subsequently formed SDS as well as by students who did not join. Three of the most prominent were David Riesman, Erich Fromm, and especially C. Wright Mills. Despite the importance of their publications, Sale mentions them only in passing. This abbreviated treatment is more serious in the case of C. Wright Mills who more than any other single individual contributed to the ideological shaping of SDS. Similarly, Sale gives scant attention to Herbert Marcuse though he was widely read and quoted by SDS members in the mid-1960s and nourished the seeds of elitism in the Movement.

A more serious problem of analysis is the author's imbalanced view of certain complex issues. For instance, Sale stigmatizes the university as an accomplice in the perpetuation of social "evils" (p. 262) and brands S. I. Hayakawa, former President of San Francisco State College, as "megalomaniacal" for his leadership in ending the campus strike of 1968–1969. While quick to condemn Hayakawa for calling off-campus police to quell unruly pickets and their supporters, Sale is curiously insensitive to the rights and welfare of students trying to continue their education unhampered by intimidating strike tactics. The most noteworthy example of Sale's unbalanced assessment of substantive issues is his incomplete explanation as to why SDS shunned reform for revolution and abandoned coalition politics for estranged elitism. According to Sale, the transformation of SDS can be explained by the corruption of American society: its racism and poverty, the complicity between government and business, Southern violence against black people, the assassination of its leaders, the cultural demoralization of its youth, police brutality, and its unresponsiveness to the pleas and programs of the New Left for reform. This is the New Left position and Sale does not step beyond it. Had he done so, he might have entertained the notion that SDS turned to revolution not only because it recoiled from the negative aspects of American society but also because its programs and strategies had failed while it shifted from a deep concern for the victimization of others to outrage at the imagined injustices done to alienated youth: a transformation that led ultimately to political isolation. The alternate case that Sale does not make is that SDS took refuge in ineffective revolutionary vanguards rather than acknowledge its own shortcomings. It was unable to overcome its administrative ineptitude (which Sale discloses) and emulate the perseverance it so admired in SNCC (Student Non-Violent Coordinating Committee). After less than three years as an exponent of reform, SDS condemned government progress toward the solution of social problems as "tokenism," and proceeded to exchange ideological independence and altruism for subservience to Marxism-Leninism.

If the reader is not looking for profound insights into the social and political history of America in the 1960s, but is seeking a well-documented, thorough, and absorbing account of the day-by-day evolution of SDS, this is the book to buy.

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Who's Who in American Politics: 1973–1974. Fourth edition. Edited by Paul A. Theis and Edmund L. Henshaw, Jr. (New York: Jacques Cattell Press, 1973. Pp. 1216. $40.00.)

Here is a reference work which political scientists should certainly know about, though few are likely to purchase. Theis and Henshaw have collected biographical data on 18,000 American politicians currently holding public office or still active in politics. Included at the national level are all elected officials plus major presidential appointees and the major officials and staff of the political parties. But these persons are included in other biographical collections. What makes this volume indispensable are the state and local entries. Included are all governors, lieutenant governors, major state officials, state legislators, mayors and councilmen of major cities, and officials and principal staff of the political parties.

The entries are based on questionnaires and thus there is the inevitable unevenness in material included. Still the volume is a godsend for political scientists interested in career development. The editors have even provided a geographic index, thus easing the task of comparative analysis among states or regions. Unfortunately, however, though they designate party affiliation in the index, they do not provide a simple code for current political position, if any. Perhaps this might be provided in future editions as an aid to the scholar.

Paul Theis is public relations director of the Republican Congressional Campaign Committee; Edmund Henshaw is executive director of the Democratic counterpart. They are assisted by an advisory committee which includes the American Political Science Association executive director, Evron M. Kirkpatrick, as well as Richard M. Scammon, director of the Elections Research Center, Governmental Affairs Institute.

CHARLES O. JONES

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Letters of Louis D. Brandeis: Volume I, 1870–1907, Urban Reformer; Volume II, 1907–1912, People's Attorney; Volume III, 1913–1915, Progressive and Zionist. Edited by Melvin I. Urofsky and David W. Levy. (Albany: State University of New York Press, 1971, 1972, 1973. Pp. xlii, 610; xxiv, 750; xxii, 705. $20.00 per volume.)

Writing shortly before his appointment to the United States Supreme Court in 1916, Louis Dembitz Brandeis argued that "no law, written or unwritten, can be understood without a full knowledge of the facts out of which it arises and to which it is to be applied." The publication of large portions of his correspondence, under the able editorship of Melvin I. Urofsky and David W. Levy, illuminates Brandeis's passion for knowledge and understanding of the political, economic, and legal issues of his time. Alpheus T. Mason, Brandeis's principal biographer, tells us that even though the reform lawyer, later Supreme Court justice, was a factualist and stickler for details, particular "emphasis should be given to the coherent and purposeful social-political philosophy which underlies his profound factual knowledge" (*Brandeis and the Modern State* [Washington: National Home Library, 1936], p. 233). Although there are no startling discoveries, the published correspondence gives added insight into the progressive temperament. And, as the editors point out, the letters help us understand the techniques and strategy of reform movements on all levels of American society.

Assembling the Brandeis correspondence was an arduous task. Mason incorporated much significant correspondence into *Brandeis: A Free Man's Life* (New York: The Viking Press) in 1946; yet the Brandeis papers remained scattered. The editors began at the University of Louisville Law School, the principal depository. Then, through other manuscript depositories and private collections, Urofsky and Levy sought additional material; from 14,000 letters they made publication decisions. Only two known sources remained closed: the personal letters which Brandeis wrote to his fiancee Alice Goldmark—letters now in the possession of Brandeis's daughter, Mrs. Elizabeth Brandeis Raushenbush; and letters in the warehouse of Brandeis's old law firm, Nutter, McClellan, and Fish. Mrs. Raushenbush allowed the editors to quote extracts from some letters. The lack of cooperation from the present partners of Brandeis's firm is especially unfortunate, for there are gaps in the published correspondence dealing with strictly legal matters. For example, one finds little on *Muller* v. *Oregon* (1908), the Supreme Court case in which Brandeis presented his now-famed sociological brief. In addition, Brandeis destroyed much of his private correspondence.

These three volumes yield little insight into private beliefs and personality factors which often aid political scientists and historians alike in making general assessments of career patterns and subsequent judicial behavior. The editors appropriately conclude that "the figure who stands revealed in these letters will seem, very often, the austere and efficient public man" (I, xix). As Mason has said, one should not reach the conclusion that Brandeis was cold and impersonal. He fervently guarded his own privacy, not surprisingly for one who contributed the important right to privacy in contemporary constitutional law. The letters document Brandeis's insatiable quest for facts—facts he could digest and transform into arguments for social action. Other qualities emerge: integrity, courage, and, occasionally, moral indignation against business practices that he felt needed to be changed. Warmth and deep affection for members of his family emanate from letters to his father, his sister Amy, and his brother Alfred. His correspondence with Alfred, in fact, gives rare insight into personal feelings and decisions; it is thus much like the useful correspondence between Chief Justice William Howard Taft and his brother Horace.

The editorial work is first-rate. Students will find particularly helpful the complete, interesting identification of persons addressed or mentioned in the letters. The explanation of issues or problems—such as the "sliding scale" principle in the gas industry, the Pinchot-Ballinger Affair, or the right to privacy—raised in the text of letters is of consistently high quality, a tribute in itself to the scholarly ability of Urofsky and Levy as American social and intellectual historians. The editors based publication decisions upon the historical value of each letter, feeling that "erring on the side of generosity was preferable to erring on the side of brevity" (Vol. I, p. xviii). One's inclination is to agree: some letters, taken alone, appear to have little inherent merit. Yet on second thought one sees that the entire body of published correspondence, including pieces that seem trivial or repetitive, creates a coherent whole, from which broad themes and intellectual contours emerge.

Volume I, *Urban Reformer*, shows Brandeis's evolution as an urban progressive, fighting the abuses of the Boston street railways and gas companies, in quest of social reform. He became an unusually successful local reformer, having achieved a major success in the passage of the Savings Bank Life Insurance Law in Massachusetts. The letters in Volume I show a growth of confidence and an increased willingness to exercise social and political leadership in Boston and Massachusetts. In the years encompassed by Volume II, *People's Attorney*, 1907–1912, Brandeis's attention was drawn more and more to Washington, though he did not fail in his commitment to Massachusetts. He was a leader in numerous national crusades: an opponent of the proposed merger of the New Haven and Boston & Maine railroads, and a spokesman for orderly development of natural resources in the Pinchot-Ballinger Affair. During this period Brandeis and Senator Robert M. La Follette were drawn closely together, serving thereby as important intellectual forces in the progressive movement of the 1910s. Between 1913 and 1915, as the letters in Volume III, *Progressive and Zionist*, indicate, Brandeis was at the zenith of his career as a reform lawyer. An important policy adviser in the Wilson administration, Brandeis had a major effect upon banking and antitrust programs. In rate hearings before the Interstate Commerce Commission, he assumed the unique role of counsel for the public. His efforts against the New England railroad merger—seven years in argument—prevailed.

Of the five proposed volumes, these three provide us with rich insights into the pre-Court years of Mr. Justice Brandeis. They represent a storehouse of important data regarding the possible effects of background factors upon later judicial decision making. For the student of judicial recruitment, the letters contain important implications regarding career development and patterns of political influence.

More than anything else, these three volumes show us Brandeis's love of the law. His basic acceptance of social and political institutions, plus his reverence for the law as a mechanism of change, make Brandeis a Burkean conservative, as Urofsky argues elsewhere (*A Mind of One Piece: Brandeis and American Reform* [New York: Scribners, 1971] p. 17). And, at the same time, because of his zest for reform, Brandeis emerges as a major figure in the new liberalism of the Wilson era. The letters indicate again and again that Brandeis sought change, but not radical change. As Mason says, "he did not hate capitalism; he deplored its abuses" (*Brandeis: A Free Man's Life*, p. 105). Through the medium of law, social change was possible. And the necessity for such change had a resultant effect upon characterization of the law. The work of legislators and judges alike was to bring the law more in accord with life. Out of such ideas came the acceptance of sociological jurisprudence, one of the indirect legacies of Brandeis's career.

ROBERT G. SEDDIG

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Black Political Parties: An Historical and Political Analysis. By Hanes Walton, Jr. (New York: The Free Press, 1972. Pp. xi, 276. $7.95.)

This book covers a great deal of ground, describing not only black political parties, but also other types of black political organizations and movements. The substantive core of the book is a national overview ranging from the first annual Negro convention in 1830 to the Gary convention of 1972, a summary look at state parties from the Black and Tans to the contemporary United Citizens Party of South Carolina, and an intensive treatment of black political activity in contemporary Mississippi and Alabama, with special attention to the Mississippi Freedom Democratic Party, the Lowndes County Freedom Organization, and the National Democratic Party of Alabama. The book is copiously documented with footnotes, tables, figures, and maps. It begins in a context defined by James Madison and ends in one defined by V. O. Key, William Hesseltine, and others.

While there is no lack of information in this book, there is, unless it escapes me, little real analysis. Some useful distinctions are drawn, such as that between independent black political parties, and satellite or parallel parties, which are usually state adjuncts to a major national party. Yet other than being used as a basis for organizing the book, little is done with this distinction.

Comments on the achievements (or more often lack of achievements) and consequences of black parties are scattered throughout the book, but they are usually isolated comments that are not threaded together into a sustained theme. The standards of political "success" are nowhere clearly delineated, and most of the successes noted are in terms of votes mobilized or offices won.

While Professor Walton is keenly aware of the obstacles to black political success, he contributes little to the development of a theory of the limitations on such success and the conditions under which they may be overcome. What can be expected when a minority organizes to advance interests that are opposed by a majority? What patterns of opinion distribution, opinion intensity, or combinations of multiple issues would enhance the influence of black parties? What institutional arrangements help or hurt the most? Is there an identifiable threshold of declining political discrimination beyond which concrete gains can be made? To what extent does an oppressed minority have any control over its fortunes through democratic politics?

Good questions are raised in the beginning of the book, and there are conclusions at the end, but the conclusions are familiar and do not benefit much from any systematic analysis of the empirical materials brought together in the book. The volume's most important contribution is in bringing together a great deal of descriptive information, and giving an historical perspective on black parties in America.

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In Pursuit of Price Stability: The Wage-Price Freeze of 1971. By Arnold R. Weber. (Washington, D.C.: The Brookings Institution, 1973. Pp. 137. $5.95, cloth; $2.50, paper.)

Arnold Weber—the administrator and one of the architects of the 1971 wage-price freeze—summarizes his perception of the background, implementation, and lessons of this policy in this short book. The book will be a valuable manual, rather like deSegur's history of Napoleon's Russian campaign, to those who decide to impose another such freeze. The book does not contribute to an understanding of the effectiveness of a wage-price freeze.

Weber's perception of the background of the freeze is conventional but flawed. He contends that "from the outset of the Nixon administration the suppression of inflation had been a central concern" (p. 1). In fact, the administration pursued a strongly inflationary policy; after 1969, the rate of increase of both federal spending and the money supply increased for three successive years to record peacetime rates. He suggests that the traditional ways to reduce inflation had proved ineffective or unacceptable; in fact, they had not been tried. He concludes that the administration's earlier resistance to controls somehow increased their inevitability and scope. He does not explain how a president who was elected on a platform critical of controls, whose chief economic advisors resisted controls, and who a few months earlier stated that he would not impose controls came to determine that the 1971 freeze would be either good economics or good politics. Weber reports, without comment, that the minimal prior staff work was limited to administrative details.

Weber regards the 1971 freeze as a bureaucratic miracle. He attributes the credibility of the program to "the performance of the leading players and the skill of the stagehands" (p. ix). The administrative policies that most contributed to its success, according to Weber, were the secrecy of prior planning, the drama of the August 15 announcement, calculated uncertainty about the duration and future scope of the controls, a casual disregard for due process, the intimidating effect of using IRS agents as inspectors, the use of extralegal pressure, etc., and he amply documents the use of these policies. One wonders whether these lessons for improving bureaucratic performance are generalizable to a constitutional democracy.

In the final chapters, Weber suggests several criteria for judging the 1971 freeze:

> Did it reduce inflation during the freeze?
> Did it reduce inflationary expectations?
> Did it permit "the development of more substantial economic policies?" (p. 129).

In fact, the rate of inflation of wholesale prices declined from an annual rate of 4.9 per cent in the six months preceding the freeze to a rate of minus .4 per cent during the freeze, and interest rates declined by .5 percentage point. It is difficult to attribute these changes to the freeze, however, because of coincident changes in other economic policies. Most importantly, the rate of increase of the money supply declined from an annual rate of 9.7 per cent during the prior six months to a rate of minus .5 per cent during the freeze, and this fact is not mentioned by Weber. Some crude estimates of my own (published in the *Journal of Money, Credit, and Banking* in May, 1973) suggest that the controls probably reduced interest rates temporarily in 1972 but had no significant effects on average prices, wages, and unemployment in either 1971 or 1972. For all that, the controls may have been good politics.

The longer-term record of the controls developing from the 1971 freeze, of course, has been disastrous. In 1973, the U.S. economy experienced record rates of inflation and interest and widespread shortages, and there is no near-term prospect of "more substantial economic policies." Grayson, the head of the Price Commission in Phase II, has recently urged the immediate termination of the control system. Arnold Weber, apparently, has had no such second thoughts.

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The Politics of the Ocean. By Edward Wenk, Jr. (Seattle: University of Washington Press, 1972. Pp. 590. $14.95.)

This book overwhelms the reader ranging as it does through every facet of U.S. ocean policy over the past twenty years. The book is also a primer on bureaucratic, administrative, and organizational politics and, while it does not ap proach this subject in a systematic way, it represents keen insight from firsthand observation. Dr. Wenk assesses the strengths and weaknesses of every interest group, cognizant agency, congressional committee, White House officer and the general public as they bear on ocean policy.

The book is organized into three broad subjects. It first examines the generation and development of U.S. ocean policy; a middle section covers the principal substantive issues that occupied the attention of the Marine Science Council, of which Wenk was Executive Secretary; and the final part of the book projects implications and postulates future management avenues for marine policy. These include the author's assessment of problems facing the recently created (1970) National Oceanic and Atmospheric Administration and extensive suggestions for U.S. leadership in the international development of ocean resources.

Wenk, like J. Clarence Davis, III, gives much credit to Congressmen for spearheading the creation of environmentally related policy. Highest praise is given to Hubert H. Humphrey who chaired the MSC and lent the prestige of his personality and vice-presidential office to the Council which, in fact, did a unique job of coordinating agency interests and developing policy priorities. Wenk reserves his most weighty brickbats for the Bureau of the Budget (later the OMB), pointing out procedures which preserved narrow policy alternatives and frustrated officials who had already "suffered both the uncertainties of delay and anguish" (p. 124) in making policy. The author makes many tart observations about government operation, and at one point we are told how Vice President Agnew, failing to manage a debate in the MSC, ordered that the next Council meeting be staged with a complete scenario on hand (p. 163).

The substantive focus of the book relates to the problems of identifying and developing the resource potential of the oceans. For example, the U.S. fishing industry is examined to show how antiquated practices reduce the catch and how a disorganized industry cannot make an effective impact on the policy process except to protect the outdated methods of many of its members. He criticizes the divisions and jealousies in the U.S. scientific community—divisions which restrain the national impetus to explore the oceans for both scientific and practical purposes. He relates in detail the attempt to create a Decade for Marine Development and to establish a food protein program from ocean sources. Energy concerns are discussed also, and Wenk warns that the glamorous promises of ocean wealth must be verified by further exploration and study. While he disagrees with the National Petroleum Council about how far out to sea a nation should claim exclusive rights to offshore minerals, he believes that the petroleum industry can tap ocean oil resources successfully and safely.

The book also covers matters of interest to scholars of international affairs. Wenk examines the development of the latest U.S. Law of the Sea proposals; for instance, he reveals how the U.S. trusteeship concept was developed by government consultants—particularly Professor Louis Henkin. In this proposal the U.S.A. has sought to compromise between the international advocacy of Maltese Ambassador, Arvid Pardo, and the protectionist interests of many developing countries and segments of American industry as well.

No review could begin to do justice to the wealth of information contained in this volume. Those interested in ocean policy will pursue this major contribution with great interest. The reader interested in a case study of policy making and implementation will find the work impressive and utterly thorough, but not organized by any obvious models. The book also contains extensive appendices and notes which will be helpful to the research-oriented scholar. Yet through all of the vast material on a complex issue, one still looks for a clear perspective on ocean policy. Because this book is written from a partisan point of view, we want to know more about why ocean policy should receive priority among other national needs which are equally pressing. Wenk gives a partial answer to this in his description of the oceans as a world resource; but perhaps a more precise answer is obscured in the details of policy memos, jurisdictional battles, meetings and continuing efforts to be mentioned in the next presidential message.

This examination of ocean policy development reminds us of the difficulties any policy proponent must face in seeking and holding the attention of all the necessary parties—from the public to the President and back again. In the last part of the book Wenk focuses upon the future of oceans use and proposes the oceans as an area in which the U.S.A., by virtue of its democratic institutions, scientific know-how, and technological capabilities, can do much to encourage sound management for the benefit of the entire world. These suggestions are inherently visionary but completely in line with the author's obvious impatience over the complex and distracting (though admittedly unavoidable) forest of detailed political reality through which his insight has been projected.

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The Guarantee Clause of the U.S. Constitution. By William M. Wiecek. (Ithaca, N.Y.: Cornell University Press, 1972. Pp. 324. $12.50.)

This analysis of one of the most sterile provisions of the Constitution is intended to persuade us that it has an "awesome potential" for social prophylaxis. It is Professor Wiecek's thesis that Congress and the Supreme Court, especially, ought to convert the guarantee of a republican form of government to the states into a requirement that they reform both their governmental and "social institutions" so as to become truly republican (p. 302). He admits the difficulties involved in activating this clause. They arise from the Delphic character of the constitutional language and the desuetude into which the clause has fallen since 1842, when, he argues incorrectly, it was last applied. Nevertheless, he has subjected the development of the clause to an historical analysis covering the period from 1787 to 1962 to determine its origin, purposes, and uses.

In a country where Bourbon is merely the generic name of a whiskey, and not the surname of royal pretenders, it is not surprising that until now the meaning of this clause has been viewed as one of those barren legal questions which, Justice Frankfurter once remarked, leads to political dialectics and conclusions unrelated to reality. But even when one assumes that some sound purposes of scholarship are served by this study, it is very disappointing to find at the end nothing more than the author's hope that the Supreme Court will "evolve substantive standards of republican government and . . . enforce these (sic) on state governments" (p. 289).

In addition, Wiecek's treatment of his findings is highly tendentious. He has found that on those rare occasions when the meaning of the clause was debated seriously, it was given both "conservative" and "reformist" interpretations, to no one's surprise. Unfortunately, he is determined to prove that these scattered debates reveal the clause to be bulging with latent power, for good or evil if it be construed as broadly as both its critics and admirers have feared or wished. This obsession leads him to confuse separate elements of Article IV, Section 4: "The United States" shall guarantee to each state a republican form of government and, in addition, protect it from invasion and respond to proper requests from a state government to quell domestic violence. These are three distinguishable circumstances, although they may not necessarily be unrelated on all occasions. In seeking to define "republican" government, Wiecek sometimes links the guarantee and domestic violence clauses to demonstrate that the clause was intended to be used to "repress" the downtrodden of various sorts. In his search for proof of this contention, he has included even the Whiskey Rebellion as evidence. He separates these two clauses of section 4, however, when he argues that the guarantee was truly defined by the Abolitionists and radical Free Soilers who incorporated into it the Declaration of Independence and various bills of rights. The latter interpretation supports his reformist goals, of course, but it leads him to the irritating practice of drawing unwarrantable inferences from generally sound data.

In tracing the genesis of this clause Wiecek argues correctly that there was no "single and universal denotation" of republican government in 1787 (p. 13). It was even a "revolutionary ideology," but it was not devotion to it which moved the framers to insert the guarantee of a republican form into the Constitution. It was the fear that Shay's Rebellion would be only the first of many armed assaults against men of property. Consequently, this guarantee was defined by the domestic violence clause as twin elements of a "contemporaneous process of delegitimizing the resort to violence by the people as an extra-legal means of defending the community interest" (p. 42). Despite this highly dubious conclusion, Wiecek concludes his study with the assertion that the framers intended this clause to provide "federal supremacy over the states" and to command the latter "to realize the ideals and aspirations they had embodied in the Declaration of Independence, the preamble to the federal constitution, and the libertarian elements of the state constitutions" (p. 291).

Wiecek draws equally unwarrantable conclusions from his examination of the treatment of the guarantee clause during Dorr's Rebellion and Reconstruction. He concludes that President Tyler's view of the extent and limitations on executive discretion to implement section 4 during Dorr's Rebellion "has prevailed to this day because it is basically sound" (p. 105). Wiecek treats Tyler's words and actions as an interpretation of the guarantee clause, but, in fact, Tyler explicitly refused to rule on the lawfulness of the government of Rhode Island and its challengers. He denied that any president has the authority to determine whether or not a state's government is republican in form. Instead, he refused to send federal troops to a state upon mere rumors and threats of violence and he counseled the parties to resort to all peaceable and legal means of changing the state's government. Dorr's supporters in

Congress tried, but failed, to gain agreement from others that the established government in Rhode Island was not republican. They had no more success in the Supreme Court which refused to treat the dispute as justiciable in *Luther* v. *Borden.* From these events Wiecek draws two conclusions: they support Hamilton's "repressive" definition of the clause in *The Federalist* No. 21, and Taney's opinion was mere *dictum,* "absolutistic," but "defensible" (pp. 122, 124). He was too cautious in restricting judges to " 'fixed constitutions and laws, given them by others, and not provided by themselves' " (p. 121).

Wiecek requires eighty pages to demonstrate that Congress rejected this clause as the legal basis for Reconstruction. Nevertheless, he remains warmly sympathetic to Senator Charles Sumner's contention that the clause is a "sleeping giant" which confers upon Congress unlimited power to remake state and local governments. Incredibly he concludes at one point that even in rejection the clause was greatly expanded in "scope and character" (p. 166). Later he laments that by 1877 it had become a "deadletter" because Congress had "abdicated" responsibility for it (p. 225). Yet, he recites the occasions on which Congress considered and rejected claims that various Southern governments violated the clause.

He admits that implementation of his position would destroy federalism, if pushed to the limits. He has some doubts that judicial reconstruction of political institutions is consistent with his equation of republicanism and self-government. But he overcomes these doubts with his conviction that the Court ought to scrap the doctrine of political questions and adopt the guarantee clause to undertake reform. This strategy would extend the reach of the Court's power because it would not be restricted by the concept of state action embodied in the due process and equal protection clauses of the Fourteenth Amendment.

Protagonists of judicial activism may welcome this addition to the literature dealing with the guarantee clause. Others who might seek illumination of the meaning of republicanism in the eighteenth century, a modern reinterpretation of it, a fresh understanding of the Constitution, or even a serious justification for the reform of republican institutions by robed guardians will find little instruction in this book.

ROBERT J. MORGAN

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Rural Politics and Social Change in the Middle East. Edited by Richard Antoun and Iliya Harik. (Bloomington: Indiana University Press, 1972. Pp. 498. $13.50.)

Conference papers make notoriously bad books, but this volume is a happy exception. The coherence of the book is probably due to careful planning by the organizers of the meeting, though it is also correct that this neglected subject has rarely been treated comprehensively, at least among students of the Middle East and North Africa. There are a number of important lessons to be learned by political scientists from this study.

First, we have on the whole been a bit simple-minded in our use of anthropology (and other disciplines) to rescue political science. Reading the theoretical chapters by Bailey, Fernea, de Planthol, and Antoun will relieve any such illusions, and contribute to a more intelligent use of anthropological concepts in political science, especially in the study of local politics. Second, the seven case studies go a long way toward conveying to our discipline the remarkable interest of anthropology in political questions, as noted by Geertz in his summary. To read about parties, administrators, national elites, elections, and rural development as they affect political change in villages and towns is a sobering experience for scholars who may think our discipline actually encompasses politics. Anyone interested in subnational politics can learn a great deal from this book.

The first theoretical paper is Bailey's and closely follows ideas developed in his *Stratagems and Spoils,* illustrated with Barth's work on the Swat Pathans. Because the possible applications of transactional analysis to politics are many, it is unfortunate that Barth's views were not more thoroughly discussed. Some of the problems with the highly relativist position Bailey advocates are raised in the next essay by Rosenfeld. In his general commentary on anthropology he refers to the difficulties of getting a time or problem focus on change using anthropology (p. 53) and the general aversion of anthropologists to dealing with conflict (p. 60). Most important, he notes the "failure to explain the system that creates the lives the peasant lives" (p. 73). Fernea makes a highly suggestive addition to the theoretical section in his paradigm involving tribal, urban, and productive factors as they relate to change in villages. Antoun to some extent contradicts Rosenfeld by presenting a thoughtful outline of the contrasting demands for order and competition at the local level.

The case studies are uniformly of very high quality and distinctly political in their implications. Peters writes of the transfer of local and external influence from traditional to professional elites in Lebanese villages. Khuri concentrates on change in Lebanese suburbs. Rosen presents a well-written account of competition for office among Arabs and Berbers in the Moroccan town, Sefrou. Meeker's essay on the adjustment of the Turkish Aghas of the eastern Black Sea region comes to life as he related how the nationalizing election of 1950 led to local political change. In Abu-Zahra's essay on Sidi Mateur in Tunisia, there is an excellent account of party and administrator intervention in local affairs and its effects. Harik writes of the basic contradiction of Nasser's policy aimed at restoring popular control to communities, while engaging bureaucratic forces at the local level; and Abu-Lughod outlines the effect of migration of social and political questions in Egypt. Though there have been some fine monographs on local-level change in the Arab world, I know no more detailed and suggestive array of cases to illustrate what remains to be done.

Given the rediscovery of policy in political science, many political scientists will be interested in the final section on economic change and reform. Harik writes of the introduction of a domestic market in Egypt in the absence of capitalism. Keddie has a useful summary of Iran's record with land reform. Owen writes of the dual agricultural sector problem which troubles growth in many countries beyond the Arab world, and Simmons has a finely detailed account of the Tunisian farmer in the Sahel region. These are all penetrating studies, and contrast favorably with much of the writing on policy in industrial societies, which often appears so abstract one can only sympathize with anyone trying to use it in making a real decision.

In reviewing the volume, one cannot avoid wondering why the area-studies scholars took so long to begin to demonstrate the relevance of their work to both theory and policy. It may be that the more traditional cast of much work on the Arab world impeded this kind of effort. An equally satisfying explanation may be that the concentration on the pinnacle of power, especially in the fragile states of the Middle East, has been too readily transplanted from more fashionable comparative analysis dominating our research. Whichever view one finds comforting, the volume of Antoun and Harik suggests that the Arabists may have been too timid and the comparativists may have been asking the wrong questions.

DOUGLAS E. ASHFORD

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Anarchism Today. Edited by David E. Apter and James Joll. (New York: Doubleday Anchor Books, 1972. Pp. 274. $1.95, paper.)

Edited collections of essays are invariably difficult to review, and *Anarchism Today* is no exception. Despite the efforts of David Apter and James Joll to tie the contributions together in a short introduction and conclusion, the volume has an amorphous quality which, in the end, is quite frustrating.

For one thing the book's focus is unclear. Both Apter and Joll are primarily concerned with the student movement (or the "New Left") which they see, in part at least, as a revival of anarchism with a difference. But while some of the contributors deal with themes related to this one, others develop either general or detailed studies of aspects of traditional anarchism in particular countries. The problem, of course, is that neither traditional anarchism nor the New Left variant of it are all of one piece. An attempt to study either movement would be difficult enough; a relatively brief volume with essays treating both movements, and sometimes their interrelationships, can easily become a potpourri.

In describing the relationship between traditional anarchism and the New Left, the editors perceive similarities in the attack of both on the whole apparatus of political organization *per se* The key differences seem to lie in part in the New Left's lack of concern with theory or history or both. Traditional anarchism, as compared to socialism, produced very little in the way of theory or good historical analysis, but the New Left (so the authors feel) is likely to produce even less, for the movement is self-consciously "anti-rational." More important, while the New Left can be identified with one wing of anarchism in its espousal of the propaganda of the deed, it nevertheless differs from traditional anarchism both in its "playfulness" and, in some cases, its view of violence as liberating. In fact, Joll feels that the New Left, consciously or unconsciously, is inspired by Sorel more than by any other classic theorist. After all, most traditional anarchists saw violence as a means toward social liberation rather than as a basis for self-fulfillment.

Finally, both editors contend that the New Left version of anarchism borders on "nihilism." Its purpose is less to create of a "free" political order than to end social roles of whatever kind. Needless to say Apter and Joll are less than fully sympathetic to that purpose, and Joll, at least, argues that anarchism in both its traditional and New Left forms can only be understood if one adds to sociological analysis some examination of the psychological makeup of the movement's adherents.

None of the essays in the volume really attempts this task, with the possible exception of Michael Lerner's discussion of the American student movement. His effort, however, is basically directed to giving readers a sense of the *feelings* of upper middle-class students (including his own) during the 1960s. Most of the other contributions are broadly historical or sociological. Some of them, such as Colombo's essay on anarchism in Argentina and Uruguay, are general histories of the anarchist movement in these countries. Others, e.g., Mauria's "The Spanish Case," deal primarily with particular historical periods and problems. In both Spain and Latin America, of course, the anarchist movement has had a relatively long history. In countries where this history has been less significant, the emphasis of the contributors is on the New Left, sometimes in its relation to the anarchist movement, sometimes without even discussing anarchism. While some of this latter group of essays are broadly descriptive (e.g., Tsuzuki on Japan), others like that of Lerner on America or Gombin on France are primarily concerned with *meanings*, with Gombin focusing on new developments in working-class thought and action in relation to the events of 1968.

In general, the broadly historical essays are too brief to add much to our understanding, and the more specialized accounts too narrowly focused for undergraduate or even most graduate students. As for the interpretative essays, while they are well written, they are primarily concerned with repeating the conventional wisdom about postindustrial society, alienation from the self, etc., which has flooded our eyes and ears for the past several years.

For this reason perhaps the essays dealing with the history of traditional anarchist movements are the more satisfying group despite their limitations. At least they treat concrete historical phenomena in terms with which social scientists trained in the 1940s and 1950s can cope, e.g., social class, historical traditions and the like. Those who see the emergence of the student movement as related to the phenomenon of "postindustrial society" may be right, but other variables may also be at work, and they need to be explored in depth. *Anarchism Today* helps us little if at all here, despite Apter's skill as a comparativist. For example, the Japanese student movement has a long history, as do student movements in Latin America and France. On the other hand, for all practical purposes the American student movement is new, and students in the United States are certainly responding to very different kinds of perceived ills than are students in other countries. One would expect a volume like this one to throw some *comparative* light on the differences, and to aid us in understanding why, despite them, the ideologies of these movements resemble each other so closely. However no real effort is made to do so.

Perhaps more importantly, the development of the student movement in the 1960s is probably the first instance in recent history in which American ideas and attitudes have served as a spark for movements in Europe. Why is this so? It is hard, on the face of it, to believe that the issues which theoretically led American students to question their own society (superaffluence, Vietnam and the race issue) have the same salience in France, Germany, or Italy despite the rhetoric of the student leaders. Perhaps the answer is partly that during the 1950s the United States became something of a model for European youth. For better or for worse, young people began to see their own future as previewed by America, and to see themselves as part of a common culture of which America was the leading representative. Thus, they may feel that their own countries stand condemned by the "evil" in America even if they have not actively participated in that evil. This is only a possibility, but it would be worth exploring if only as a corrective to more apocalyptic visions. Worth exploring, too, is the very important role played by Americans of Jewish background in the early years of the American student movement, when they may have accounted for as much as 60 percent of the membership of SDS. (In France and England students of Jewish background also seem to have played a role in the leadership of the student movement far beyond what one would have anticipated from their proportion of the student population.)

These and other questions have yet to be tackled, and one suspects that they will not be dealt with until we gain more perspective. In the meantime the student movement in the United States and England at least seems to be relatively moribund. If Lerner, Keniston, et al. are right, why the sudden collapse? Or is this just the calm before another storm? The collection being reviewed here offers no guidelines.

My remarks so far have been rather critical. Let me note, however, that most of the contributions to *Anarchism Today* are of high quality, and that a number of the essays are quite useful for particular purposes. For example, the essay by Nicolas Walter on "Anarchism in Print: Yesterday and Today" is a very good annotated bibliography on the subject. Further, if the editors of the volume are less than fully sympathetic to anarchism in its traditional or New Left variant (they are certainly not completely unsympathetic), most of the contributors range from mildly to enthusiastically supportive. The combination makes for a very nice balance.

STANLEY ROTHMAN

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Asian Development: Problems and Prognosis. By John Badgley. (New York: Free Press, 1971. Pp. 210. $6.95, cloth; $2.95, paper.)

This is a first-rate work of political analysis which examines the problems that political development in Southeast Asian countries poses to the countries concerned and to the United States. I am sure the author himself would disclaim that the book deals with the problem of development in the whole of Asia. There are some very perceptive comments regarding the political development of India and China, but the material from these two countries is used occasionally by the author in his attempts to explain or establish his central thesis relating to Southeast Asian countries. His central thesis is that most of the states in Southeast Asia (and the principal ones he deals with are Thailand, Malaysia, the Philippines, Indonesia, and Burma) can only be regarded as states by international courtesy. These countries in their precolonial state lacked key institutions like legal codes, secular organizations in metropolitan centers, bureaucracies, and universities. Then Professor Badgley tries to show that the legacies that may be discerned during the postcolonial period are not substantial in terms of legal codes, political parties, interest groups, bureaucracies, and universities. Impressed by western values and practices, political leaders in these countries have tried to build unitary state systems by invoking certain national values and aspirations. These attempts have resulted in alienation of important minorities, political violence, and eventually in the seizure of some of these systems by the military.

Professor Badgley keeps suggesting throughout the book that the central flaw in the analyses of writers like Karl Deutsch, Rupert Emerson, and Lucian Pye was that they tended to regard these developing polities as nation-states when both history and sociology clearly indicated that they were not. Like Professor Badgley, Samuel Huntington in *Political Order in Changing Societies* has also argued that these developing nations lacked some of the basic political institutions and that therefore, in order to overcome problems of rising frustration and social unrest, they should undertake systematic institution building. Huntington extolled Ayub Khan of Pakistan as the new "Solon or Lycurgus or 'Great Legislator' on the Platonic or Rousseauian model" for having built institutions like Basic Democracies, reorganized the political parties, and introduced a new constitution. Professor Badgley, with the hindsight available after the overthrow of Ayub and the formation of Bangladesh, might gleefully point out that his central thesis in the book has been confirmed, namely, that in these heterogeneous or pluralist societies imposition of ambitious nation-building straitjackets result in either political violence or disintegration. In other words, emphasis on nation building defeats or distorts political development. Therefore, Professor Badgley's sovereign remedy is "devolution of decision making, education, business activity, and cultural control away from the primate cities, that is Rangoon, Bangkok, Djakarta, Manila, Pnom Phen, Saigon, and Hanoi" (pp. 181–2). The author, however, does not appreciate that this remedy is a very complex and in some cases an almost impossible undertaking, fraught with possibilities for political disintegration. What political leaders with what perceptions and perspectives will be able to decide what local communities should receive what kind of stimulation or encouragement for purposes of institution building in local communities? How are these disparate or divergent efforts attending to disparate or divergent community needs to be coordinated under some kind of overall central planning and scale of priorities?

Writers of different ideological persuasion would argue that nation building in some of these societies has gone awry because the political leaders adopted without too much modification western strategies of economic development, namely, encouraging massive industrialization either through private enterprise or without attending to the problems of economic and regional redistribution. This brings us to a significant shortcoming in a book otherwise full of invaluable insights. Professor Badgley now and again concedes that the Communist efforts at nation building through their particular methods have been more successful, but he does not explain analytically the reasons for their success. Nor does he seem to come to grips with the question of what should be the appropriate American stance, given that certain nations are willing to embark on such experiments and prepared to pay the costs.

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Revolution in Peru: Mariátegui and the Myth. By John M. Baines. (University: The University of Alabama Press, 1972. Pp. x, 206. $7.50.)

José Carlos Mariátegui is an important figure in the development of Peru's contemporary political ideology. Professor Baines in a well-written, well-researched study has combined a biography of Mariátegui, a review of Peru's political development during the early 1900s, and a thorough analysis of Mariátegui's writings to produce a definitive study which will be of value to all trying to understand contemporary Peru.

Mariátegui was a poor boy who never received much formal education. At 14 years of age he went to work as a copyboy for a Lima newspaper and from that humble beginning he soon became a proofreader, then a journalist, an essayist, and through his contacts at the newspaper he was drawn into the fringes of the group of young intellectuals who became known as the Generation of 1919. This was a period of ferment in Peru as the opening of the Panama Canal brought the country into more direct contact with the world. The Latin American Student Revolution came to

Lima; trade unions became active, and Mariátegui became involved in some of these activities. In 1919, he took a "scholarship" from the dictator Leguía, and went to Europe where he was able to study and formalize his thinking for four years.

Mariátegui was most influenced by the Peruvian, Manuel González Prada, Henri Barbusse, the French intellectual, Miguel Unamuno y Jugo, the Spanish thinker and Georges Sorel, the French Syndicalist. When he returned to Peru in 1923 he combined ideas from these men with his private version of Marxism to create a philosophy of revolution for Peru. By then his ideas were set, as Professor Baines points out, into a "fixed pattern" (p. 36): What Mariátegui produced was a combination of Peruvian nationalism with an idealized socialism. Mariátegui thought he was a Marxist, but his was an attempt to interpret the spirit rather than the letter of what he called "revolutionary Marxism." Mariátegui saw a religious character to Marxism; he thought revolutionary activity was a way of transforming a person into a new man. To him, revolutionary activity was more important than the achievement of the revolution.

Professor Baines tells us that through various periodicals of which the most important was *Amauta* (32 issues, 1926–1930), Mariátegui promoted his idea that Peru needed a revolution to create a type of socialism based on indigenous institutions preserved by the Indian section of the population through the centuries since the conquest. Because Leguía had exiled practically all of the young intellectuals who eventually were to create the Aprista movement, Mariátegui was according to Baines, "the most prominent oppotion leader left in Peru" (p. 51) to propagate the "Revolution." In the late 1920s Mariátegui split away from the bulk of the other young revolutionists who were creating the Aprista Movement and founded a Socialist Party which became affiliated to the 3rd International. Baines points out that Mariátegui was lavishly praised by the Russian and other Communists at this time as a great Marxian theoretician, not so much because of his contributions to Marxian theory, but because his mixture of Peruvian nationalism and idealized socialism fit the party line the Russians were pushing at that time. When the Russians changed their line, Mariátegui was out of step and was censured at the First Latin American International Communist Congress held in Buenos Aires in 1929. Had he lived he probably would have been openly attacked by the Communists but he died soon after at the early age of 35. But as Prof. Baines points out he "has become [for the Communists] an infinitely more valuable figure than he ever was alive" (p. 140).

The backward state of Latin American studies in the United States is reflected by the fact that it took 42 years before a biography of Mariátegui was published in the U.S. Mariátegui remains an important figure in Peru, and his ideas about "National Marxism" have been discussed all over Latin America. His writings are included in the series of books "The Thought of America" published in Mexico, which includes such figures as Alfonso Reyes, Rómulo Gallegos, Sarmiento, Arguedas, and Ingenieros. His book *Seven Interpretive Essays on Peruvian Reality* has been republished many times in all parts of Latin America, and an English translation was published by the University of Texas in 1971.

Mariátegui died just as the effects of the world depression helped to destroy the Leguía dictatorship and the Aprista, Communist and Socialist Parties began to function openly in Peru. All three claimed Mariátegui as their own, and since then his reputation has continued to grow as he became a symbol representing "Revolution in Peru."

Extensive footnotes to sources, a bibliography, which is practically complete for Mariátegui and three appendices increase the value of this book. It is recommended as a very good piece of research.

HARRY KANTOR

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The Collapse of British Power. By Correlli Barnett. (New York: William Morrow & Co., 1972. Pp. 655. $15.00.)

Correlli Barnett, the British military historian, tells us that it took him five years to write this major study of the fall of Britain's power from her position of *primus inter pares* of the European great powers of the pre-1914 system to that of second rank or perhaps third rank today. We can well believe him. His book reaches back to the end of the Napoleonic War, when he thinks the collapse of British power began, and ends with the total British isolation of 1940. It covers almost every aspect of British life, from education to military organization, from literature to government. Its sources are varied: memoirs and biographies, state papers and reports of Royal Commissions, and include Cabinet documents recently made available in the Public Record Office in London. It is a crisply written, witty and rather sardonic book: British Ministers in the mid-1930s are presented as "like elderly Victorian cavalry officers trying to conduct a great tank battle," British business men of that period as "more interested in getting their golf handicaps down than their profitability up."

The book argues that the true sources of the decline of British power were not physical or geopolitical (as perhaps were the sources of its rise) but psychological, or rather intellectual. It was, the thesis runs, the ascendancy among the nineteenth-century British ruling class of a persistent evangelical moralism and idealism which made the managers of British industry flabby, self-satisfied, and unenterprising, and made government ministers more inclined to the fostering of goodwill than the tough assertion of national interest. The rude truculence which gave Britain the mastery of a quarter of the globe degenerated more than a century ago into a soft, acquiescent cult of generosity to the other side which the realists of the Continent from Bismarck to Hitler exploited. The forcing houses of unrealistic evangelism, Barnett writes, were the public schools, the churches and chapels, instilling in the descendants of Palmerston and Canning the smug neo-Machiavellianism of the Sir Edward Greys with their maxim "to do the right thing is always the right thing to do."

Correlli Barnett says that this moralistic humanism, excellent as it may have been for the secure Britain of the 1850s, was unfitted for the challenging world Britain faced from the 1880s on, when first there was the industrial competition from Germany and the United States, later the naval rivalry with Russia and Germany, and then the well-nigh impossible task in the 1920s and 1930s of keeping order in the Far East against ambitious Japan and holding the balance between France and Germany in Europe at the same time. The main part of the book, "Covenants without Swords," a single chapter of 350 pages, recounts once more the dismal tale of British foreign and defense policy between the two wars and lists, with full documentary references, its many tragic blunders: the opposition to France and siding with Germany in the early 1920s, the strange belief that the signing of many declarations by many nations consolidates peace; the alienation of Mussolini in the mid-1930s by a League sanctions policy aimed against his attack on Abyssinia, "a primitive African state which counted slavery among its institutions"; and finally Neville Chamberlain's childish illusion that he had turned Hitler into a respectable man while Hitler was despising him as a senile old fool.

Barnett maintains that the physical effects of the First World War on Britain were much exaggerated and that the war's results were more psychological than physical. It was the spate of memoirs, novels, and plays about the miseries of the Western Front which appeared in the late 1920s, he contends, which filled the British middle classes with loathing for the trenches just at a time when strong national attitudes toward Germany were called for. But the miseries of war, he goes on, were essentially a middle-class experience. If the Siegfried Sassoons and Edmund Blundens had known anything of normal working-class conditions in England at the time they might have been as uncomplaining as the working class (according to Barnett) was during and after the war.

But it is when Barnett proposes alternatives to British policies of that time that those policies become harder to condemn. One such alternative is that Germany in 1919 should have been "divided and permanently weakened in the very springs of power" (p. 318). But could Britain, after so many years of fighting, have provided the troops for a permanent occupation? Again, Britain, he writes, should have made a deal with Japan in the 1920s which would "avert her hostility and if possible concentrate her ambitions on Manchuria" (p. 303). But why should Japan make a bargain with Britain when she could have it all her own way, and but for a fluke might have done? True, it is hard not to agree with Correlli Barnett that a great imperial power like Britain in the interwar years cannot act as though the international system is a mutual aid society. But these notions intensified rather than caused Britain's plight. The fact is that after 1871, certainly after 1890, Britain could no longer organize the European balance of power from her own resources; indeed, European power could not be balanced from its own resources. British "fair play" perhaps helped somewhat to civilize international politics, but those politics were shaped by deeper social forces of which mental illusions were as much the reflection as the source.

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City Politics and the Press: Journalists and the Governing of Merseyside. By Harvey Cox and David Morgan. (New York: Cambridge University Press, 1973. Pp. 159. $8.75.)

Yes, Virginia, there really are local newspapers in England, and not just those behemoths of Fleet Street and quality productions like *The Times*. The Cox and Morgan study concerns a half-dozen such local papers during the 1960s, all of them published within the Liverpool conurbation. (A useful word to recall, conurbation, whenever our British brethren twit us for employing social science jargon.) Perhaps their most salient characteristic is a heterogeneity in intended distribution patterns, and therefore in expected audiences, the Liverpool morning paper having a regional focus, the Liverpool evening paper a somewhat narrower metropolitan emphasis, and the Liverpool weekly a more exclusive concentration on complaints from certain working-class areas of the city, while the other three papers—*Wallasey News*, *Birkenhead News*, and *Bootle Times Herald*—mainly hold forth, once or twice a week, on happenings in their particular communities of origin. Clearly, variability in treating local events and issues is not a mere consequence of ownership, since all but the Liverpool weekly are affiliated in one way or another, but instead reflects the functional (or dysfunctional) work that each paper performs within the overall Merseyside communicative system and that its editorial side sees, or feels, appropriate for it in its circumstances.

As usual, the title of the book is too ambitious and the subtitle a far better guide to its topic. Indeed, in a fairly crucial way, the title may be downright misleading, since most of the press coverage surveyed—in all but the *Liverpool Weekly News*—is about governmental actions, singly or in series, and far less is about expressed demands and supports among the populace at large. (See especially pp. 48–50 and 58–61.) Moreover, as those alert to nondecisions would quickly observe, suppressed demands go virtually unnoticed, exception made perhaps of implications in the "constraining factors" on reporting local affairs and in the suggested development of "a local equivalent of the national lobby-correspondent/political columnist system" (p. 148) on which the concluding chapter touches. It is upon certain chronological indicators of governance (council minutes, committee papers, annual reports, facility inaugurations, and the like) that the local papers and their academic analysts chiefly lean—and not on politics in its richer and more complex senses.

Although Cox and Morgan explicate and utilize a formal, if rather simple, kind of content analysis (chap. 5), the bulk of their factual presentation is discursively verbal. Besides copiously delineating case histories of "What the Papers Said" (chap. 6) in order to "make these dry [statistical] bones live" (p. 72), they also relate in summary fashion what they learned in interviews about how local editors view their role and the surrounding scene and how local politicians in turn evaluate the press (chap. 7 and 8). These four core chapters, whose substance almost wholly derives from the authors' having immersed themselves in nearby, readily available, and relatively inexpensive written and oral information, are (naturally enough) without footnote citations.

By contrast, the encapsulating chapters, which seek to place these data within an enhancing sociological and geo-economic context and to draw from them certain politically ethical consequences, contain precisely 100 secondary references. The explanatory theory drawn upon is that of media sociology. Most of this is unexceptionable enough. For example:

> A series of stages leads up to the central decision-act, and a series of outcomes flows from it. The process involves the interplay of individuals and groups at a variety of levels, and in a variety of structures. It involves, too, their perceptions, dispositions and resources, the constitutional framework and the power relationships within which they operate. Since communication is an essential element in the system, the output of the media may be examined as it reflects the different elements and stages of the process, and as it makes its own independent contribution to the play of persons, pressures and policies (p. 21).

Yet just as the press perhaps "gives local government a duller image than need be" (p. 26), statements like these may make theory a blander farrago than it need be, too.

The conclusions of the study are suitably modest, urging a more interpretively informative press—"not more reportage of specific events but general discussion of their significance in the light of local government trends and community changes" (p. 150)—accompanied by a renewal of local democracy. Indeed, as befits both the subject itself and the analytic methods brought to it, the entire presentation is modest throughout. The only immodest attribute of this slim volume, in fact, is the price which Cambridge University Press, following its recent custom, has chosen: nearly six cents a page for a book without graphs and with footnotes clustered at the back.

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Modern Revolutions: An Introduction to the Analysis of a Political Phenomenon. By John Dunn. (Cambridge: Cambridge University Press, 1972. Pp. xiii, 346. $14.50.)

The explosion of radical and aspirant revolutionary episodes which shook many parts of the world in the late 'sixties has subsided, but we are now experiencing some of the academic fallout in a spate of books analyzing the revolutionary phenomenon by one means or another. The approach adopted by the present writer, of wrapping a series of essays on specific twentieth-century revolutions inside a general introduction and conclusion, has of course been adopted by a number of previous scholars, the most successful being Eric Wolf. It is far from an easy undertaking, demanding the capacity to extract from a great mass of detail those aspects of the revolutions selected which appear to express both common characteristics of modern revolution and the discrete features of each particular upheaval, and then to draw all the strands together within the framework of an overall conceptual schema.

Professor John Dunn has taken eight case studies—Russia, Mexico, China, Yugoslavia, Vietnam, Algeria, Turkey and Cuba—as the corpus of his book, and endeavored to relate them to a broad framework of explanation. He is strongly critical of comparative studies of revolution in general, regarding them as "evasive" in the face of "the actual character of a number of twentieth-century revolutions" (pp. ix–x), and believes that by concentrating on the understanding of causality his book "may make it slightly easier for sociologists or political scientists with an interest in revolutionary phenomena to develop greater delicacy in their grasp of the intricate problems of specifying precisely what aspects of what revolutions they wish to explain and thus of relating theoretical categories to historical social phenomena" (p. x).

One can readily agree with him that the study of revolutions leaves a lot be to desired, especially in its tendency to promote one dimension of a highly complex series of processes as the answer to the theoretical problem of grasping the logic of those processes. Dunn's critical comments on Chalmers Johnson's *Revolutionary Change* are appropriate in this context, even if they have been anticipated by others before him. But it would be little short of amazing if Dunn were to succeed, within the span of a collection of essays none of which runs to as much as thirty pages, in shedding substantial new insight upon revolutionary occurrences over each of which in its own right gallons of ink has been spilled. In fact, in comparison with other works of similar intent, Dunn's analysis of specific revolutions is decidedly disappointing. His essays do not come anywhere near coping with the problem of causality upon which he undertook to concentrate. Where we would expect to be focused upon the dynamics of national and social breakdown under defined stresses, we are all too often given discursive accounts which, without any clear explanation of the rationale, accentuate in one case the significance of ideology, in another the importance of organization, and in yet another the salience of class antagonisms. Where the author does take the socioecononomic background as his focus (as in the case of Russia, Mexico, and China), the result is the more satisfactory, though of necessity the treatment is partial and terse. In other cases and especially those of Vietnam and Cuba, there is nothing like an adequate indication of the social context in which the revolution occurred. There are often interesting sidelights upon local conditions, but they remain sidelights and do not convey a coherent grasp of the revolutionary process in its totality.

It is notable that in no case is the actual revolution itself dealt with in any depth; mostly it is ignored completely, while in other cases only one part of it is enlarged upon, as is the Zapatist facet of the Mexican revolution. This cannot help but buttress determinist explanations of revolutions, since the subjective and voluntarist factors are passed over. This omission is all the more surprising in view of Dunn's stated view of the components of the revolutionary process, namely, that they consist of the quality of the revolutionaries, the communication of the relevance of their project to the lives of substantial numbers of people, and the failure of social control (pp. 234–39). The scant attention given to the first two components, while precious space is wasted on marginalia such as assimilationism in Algeria or slavery in Cuba leaves his project stranded without the legs it should be walking on.

It is perhaps not surprising in these circumstances that the ultimate conclusion that Dunn arrives at is that modern revolutions are all about nation building in modern conditions, and therefore "simply because styles of integration of national economies and societies into the world economy are experienced for the great majority of nations in the world today . . . as essentially inimical to the maintenance of local identities and the preservation of local interests, there are going to go on being revolutions" (p. 253). I find nothing to cavil at in this dictum, but in itself it does not take us very far. If Dunn had combined this generalized conclusion with a real attempt to apply his three components of the revolutionary process to the case studies, he might have come up with a worthwhile contribution to the anatomy of revolution. As it is, we are no nearer to answering the essential question: Why is it, given the widespread conditions which promote revolutions, that some countries which suffer these conditions have experienced or will experience revolutions and others will not? An "introduction" which performs neither the function of providing basic information nor that of elaborating a reasonably plausible theory, and which in addition is written in a difficult stilted style, does not really fulfull its purpose.

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Administration and Development in Malaysia: Institution Building and Reform in a Plural Society. By Milton Esman. (Ithaca, N.Y.: Cornell University Press, 1972. Pp. 341. $12.50.)

This book is solid testimony to the value for scholarship of professors engaging in technical assistance endeavors. Milton J. Esman's introduction to Malaysian governmental practice came when he and Professor John Montgomery were invited to survey public administration there and propose reform, and when he was invited to serve as adviser in the implementing of their report. From this intense first-hand experience came a deeper and more sensitive understanding of the complex ways in which politics and planning for development impinge on each other in a racially divided society. The ultimate payoff is a scholarly study which can be recommended to all who are interested in the politics of ethnicity, political development, and development administration.

In recent years scholars have debated about whether the colonial experience may not have seriously distorted the political modernization process in many African and Asian societies by grossly favoring the strengthening of administrative institutions and suppressing the development of popular participation channels. According to one view, further efforts to strengthen public administration only hampers democratic development. Esman does not equivocate, sensitive to the explosive potentialities in the relationships of Chinese and Malays, he argues that " . . . a society whose main problem is the management of communal conflict and whose secondary problem is the guidance of far-reaching social and economic change is likely to be better served if its authoritative institutions are stronger than its participative institutions and if the former are able to discipline the latter" (p. 6).

In presenting the social and political background to the current Malaysian context for development administration, Esman has written a vivid and truly masterful summary of the issues and problems of that divided but always potentially promising land. His description of the Malaysian administrative system is not as detailed as Tilman's *Bureaucratic Transition in Malaya*, but it focuses on the critical elements of administration and provides an earthy and realistic perspective that goes beyond anything to be found in the previous literature.

I found most interesting Esman's chapter on the value and belief patterns of Malaysian administrators, which is brilliant in its descriptions but faulty in its explanations. Esman's findings agree with those of James C. Scott's *Political Ideology in Malaysia*, and he accepts Scott's explanation that the orientations of Malaysian officials stem from the "realities of scarcity" rather than from any deeper cultural experience. This explanation is not convincing because Esman fails to explain why Malaysian high officials, many from aristocratic families and all with good and relatively expensive educations, should be so hypersensitive to "scarcity" and products of a "culture of poverty." Furthermore, he acknowledges that there are gross differences between Chinese and Malay orientations, which is an admission that cultural considerations are ultimately the critical ones. As everyone knows, different people react quite differently to the same "reality of scarcity"—some by becoming passive and cautious, others by being aggressive and quick-witted.

The bulk of Esman's book is his analysis of administrative development and the management of communal conflict. Starting with an acceptance of the Melson-Wolfe hypothesis that modernization will intensify ethnic conflicts in a plural society, Esman charts the possible strategies for national development in Malaysia. He points out that Malaysia once relied upon an "avoidance model" for conflict management, but after the riots of 1969, the society had to move toward a "mutual deterrence model," which Esman feels must in time give way to a "guidance model" in which governmental polities can insure that the proper balance of well-being and power is maintained between the communities.

Esman's conclusion in support of authoritative institutions raises both a factual and a theoretical issue. Some students of Malaysian society will argue that Esman has overlooked the extent to which the organization of politics rather than the role of administration was, and in the future is likely to be, critical in managing communal tensions. In their view the character of the Alliance party and the informal understanding among many of the top politicians of all communities helped ease racial tensions, and it was a breakdown in politics, not administration, that provided the fuel for the riots of 1969.

This difficulty in determining the relative importance of the political as contrasted to the administrative system suggests that there may be something wrong with a theoretical formulation which establishes "institutionalization" and "participation" as polar opposites. Why has there emerged in political development theory this notion that there must be one or the other, and not both or neither? Indeed, the Asian experience suggests that optimal conditions for domestic conflict management and effective development require a strong administrative framework and a solid core structure for organizing participation, with only marginal room for partisan controversy. From Japan with its "one-and-a-half party system" and its powerful establishment based on an elite bureaucracy capable of working closely with industry, to India with its dominant one-party system and its traditions of I.C.S. administration, the most successful development pattern has been a parallel institutionalization of both participation and administration, with both directed toward encouraging a sense of advancement. Room for some degree of partisan competition is important, but matters vital to domestic development have to be shielded from whimsical currents and passing emotions. In short, the building of effective authority can go hand in hand with expanded participation if each involves the development of more capable institutions.

The Malaysian race riot of 1969 was not so much a case of participation getting ahead of authoritative institutional development as it was a breakdown of the institutions for participation. Undoubtedly, more productive and more farsighted public administration can help create an environment which will reduce the likelihood of such breakdowns—and in my judgment Milton J. Esman has outlined what is called for in this domain—but the institutions for participation also call for independent attention. Too often political scientists, according to their personal preferences, claim too much for the advantages of authoritative institutions or popular participation and thereby fail to appreciate the separate problems and potentialities of each.

In the Malaysian case we now need a study of possible programs for the development of participatory institutions which can reinforce and not compete against this admirable study by Esman of administrative development.

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Urbanization, Planning and National Development. By John Friedmann. (Beverly Hills, Calif.: Sage Publications, Inc., 1973. Pp. 351. $12.50.)

> "We ought to live where all th' good things iv life comes fr'm," says Hogan. "No," says I. "Th' place to live in is where all th' good things iv life goes to." Ivrything that's worth havin' goes to th' city; th' counthry takes what's left . . . th' sunburnt viggytables is consumed be th' hearty farmer boy an' I go down to Callaghan's store an' ate th'sunny half iv a peach. Th' farmer boy sells what he has f'r money an' I get th' money back whin he comes to town in th' winter to see th' exposition. They give us th' products iv th' sile an' we give thim cottage organs an' knock-out dhrops, an' they think they've broke even. Don't have annywan con-vince ye th' counthry's th' place to live, but don't spread th' news yet f'r awhile.

So said Mr. Dooley two-thirds of a century before John Friedmann, though the latter says it differently.

Cities, Friedmann and Dooley say, have a decisive role in national development (Chapter 1). They are centers of innovation (cottage organs and knock-out drops); they are centers of wide opportunity (often at the country's expense); they facilitate the emergence of counterelites and a style of interest-group bargaining and compromise (Mr. Dooley, Callaghan, and the farmer boy). Development is polarized (Chapter 3). "Core regions are territorially organized subsystems of society which have a high capacity for innovative change; peripheral regions are subsystems whose development path is determined chiefly by core region institutions with respect to which they stand in a relation of substantial dependency" (p. 51). Alas, don't spread th' news yet f'r awhile! Thus Friedmann's main policy prescriptions are: promote urbanization and emphasize interlocking core-periphery subsystems for national and regional planning.

John Friedmann is a distinguished regional planner at home in economics, geography, demography, urban studies, and sociology. He joins these literatures here, emphasizing regional spatial planning. In his most theoretical chapter (3), he seeks "to formulate a theory of the development process in its spatial dimension." Consequently, the book stresses core building (urbanization and regional formation and growth) and core-periphery relations. We learn rather less about processes within existing cities and regions. Friedmann is at his best when he emphasizes the often neglected spatial dimensions of change. Friedmann the regional planner and spatial theorist is often interesting and thought-provoking. Friedmann the political analyst is less so. The following partial review addressed to political scientists emphasizes political issues.

Chapter 2 restates what political modernization scholars have come to call the "social mobilization hypothesis," associated with such scholars as Karl Deutsch and Daniel Lerner. But Friedmann places all his eggs in the urbanization basket (without acknowledging cross-disciplinary links). He takes no account of the writings by Nie, Powell, Prewitt, and Inkeles in this *Review* which show that the independent contribution of urbanization toward explaining modernization (including political participation) is, at most, modest. Friedmann has not reflected sufficiently on alternative variables (education, political organization, industrialization) with greater independent explanatory power.

Chapter 4 studies "hyperurbanization" (a prolonged condition of super-heated growth) in Chile. Friedmann's evidence here suggests, however, that Chile was urbanizing at a slower rate than most of the less developed world. Other evidence indicates that Chile's urbanization rate has been slower than that of other Latin American countries for decades. If Chile's urbanization rate is independently responsible for the country's economic middle-development, and if "many countries, including the United States, have experienced considerably shorter (urban) doubling rates over extended periods of time" (p. 91), then no country would be economically developed today. Friedmann's explanation of Chile's recent political difficulties through the hyperurbanization hypothesis, too, takes no account of plausible alternative explanations (rise of political participation and organization). One could argue a precisely opposite case: Chile's rate of urbanization had been moderate enough (comparatively and through time) that governments could handle it. Had other variables and events not intervened, the Chilean rate of urbanization was politically tolerable.

Chapter 9 persuasively argues that statesmen can learn much from regional planning theorists to inform their policy decisions. Friedmann discusses broad policy issues through a dialogue between a hypothetical head of state and his minister of national planning. Poor Machiavelli, had he given such political advice to his Prince! The head of state faces opposition criticism because the capital region has benefited most from development, draining the periphery; the South threatens secession. Friedmann counsels literacy and urban growth promotion in the potentially secessionist South, the stimulation of "centers of gravity" there, in short, "modernization in the periphery" (p. 167). In contrast, political modernization studies emphasize with rare consensus that modernization is likely to exacerbate such already intense conflicts. This was already happening in Friedmann's hypothetical country. There is also no discussion of the Chief Executive's political bases: If he shifted resources, would he gain enough support in the opposition's turf to balance his probable loss of support in his apparent home base (the capital)? Friedmann's policy advice is insufficiently informed by political research.

Thus Friedmann at his best is still too apolitical. The first part of Chapter 10 is an interesting study of central Chile. Its second part proposes new politically relevant policies (strengthen organized labor, weaken industrial managers, strengthen middle-level cities, weaken central government), but without any political analysis. Could a government elite afford Friedmann's advice? In a fascinating comparison of regional development in the Tennessee Valley and in Venezuela's Guayana region (pp. 275–276), politics are assumed, or stated as outcomes (such as low political participation in Guayana), without more ado. We ache to learn about political support, opposition, stability, and decision making.

Chapter 6 hypothesizes that "local communities differ in their abilities to respond to opportunities in their environment in ways that will promote their economic growth, and that these differences vary systematically with certain characteristics of these communities" (p. 127). This is considered to have been proved later on (p. 246). Yet Friedmann's own evidence—acknowledged reluctantly—indicates that centralization of national power is a more decisive variable. It cancels out the independent explanatory power of the local community variable. Nevertheless, the research emphasizes the challenged hypothesis, and considers political centralization less.

Chapters 1, 7, and 13 state that there is no adequate theory of urbanization. Chapter 3 presents a "theory of polarized development," and Chapter 12 states "an adequate body of theory exists" (p. 251). After Part I ("Theory"), the author warns about forthcoming chapters ("Policy" and "Practice") that "the failure of terminology to correspond precisely to the notions set forth above is owing to the general lack of an established consensus on the meanings of these terms and the fact that the chapters were written at different times for different occasions" (p. 135). The book would have been far more satisfying if, instead of a collection of theoretical assertions at a high level of generality (and Mr. Dooley would have enlivened the tone!), in Chapter 3, followed by essays on diverse and loosely connected topics on theory, policy, and practice, the author had explicated theoretical propositions about urbanization and regional development in the light of evidence in a book-length discussion. A touch of more systematic political analysis would have been welcome, too—and necessary.

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The Politics of Urban Development in Singapore. By Robert E. Gamer. (Ithaca, New York: Cornell University Press, 1972. Pp. 263. $15.00.)

Singapore has been a planned city ever since Sir Stamford Raffles arrived in 1819. Many of the central sections are reclaimed from saltwater bogs. Professor Gamer's superb book shows a keen awareness that any organized development or reclamation project operates in a social context. The value of any such enterprise lies in its effect on the human world, and not a bit in its abstract form as a plan.

Gamer's intention is comparative, general, and moral. He found in Singapore's Kallang Basin (as Herbert Gans earlier found on Boston's West End) that redevelopment disrupts economic and social patterns which many inhabitants of slum areas say they like. In some ways, this book is supposed to be a policy study. It criticizes existing procedures and suggests reforms. The most comprehensive recommendation is that Singapore's planning agencies must try much harder to assess the good aspects of what they intend to destroy in the modernization process. Only then can they measure whether specific plans will make for net improvement or not. On p. 65, for example, he asks whether a housing project may actually depress some living conditions, whether it is likely to increase extortion by criminal groups in a neighborhood, whether it will make young people study their homework less well, whether cottage enterprises will be inhibited, and whether the residents of the area are not already "content with their existence."

It is easier to pose such questions than to answer them. With this latter task, Gamer runs into his main difficulties. He tries to plumb the motives of urban ex-peasants who live in reclamable areas. Why don't (or shoudn't) these people want their lives to be modernized? Maybe it is still enough to insist on the importance of this question without going very deeply into the value problems it implies. That is more than most studies do. But the conclusions of the present book suffer from a reticence to talk as if the available ethics in Singapore had structures, which can be compared as surely as can other social facts that our positivist science admits with greater tolerance.

Specifically, it is not always clear just what Gamer is defending against the encroachments of modernity in Singapore. He has a firm sense of the importance of ethnic divisions in that city, and he occasionally refers to local cultural traditions. But this book is not exactly a plea for the moral maxims of Aw Boon Haw (depicted as they are in dioramas at the Tiger Balm Garden, "behind the artificial mountain containing the bloody war between the rats and the rabbits," p. 5n). Nor is the book written on behalf of any Indian guru, nor for the peace of Malay farm life. Cultural insights (e.g., about garden parties and racial mixing on p. 19) are usually hidden away in footnotes when they are offered at all.

Babel is partly to blame for this problem. Gamer relied on English sources for his documentation; so he had largely to trust the products of an ex-colonial education system. His thesis might have been strengthened, and more fully dramatized with examples, if he had delved into the large available files of non-English Singapore newspapers. The effects of this omission were lessened by his own surveys and unusual perceptiveness. Still one can question any rigid distinction between "Chinese-speakers" and "English-speakers." Actual language use may be better correlated with situations than with persons, and these categories anyhow mesh better with a belief in developmental dualism than with Gamer's thesis about the uncertain effects of planning. Most "English-speakers" in Singapore are Chinese or Indian in background, and there is a spectrum (not a two-part division) of the degrees to which they like selected parts of their ancestral cultures now. Singapore groups may object to further modernization on the basis of transformed traditional values, and Gamer's argument would be more complete if the book could show these viewpoints better.

Tradition is only the first line of defense in this book against redevelopment. Gamer's liberalism also comes into play. On p. xxiii he specifies that stable environments should be created "for individual citizens." P. 192 suggests that the political structure should serve "personal needs." Yet in other places (e.g., p. 204), Gamer sounds like a communitarian. He comes out for strengthening small groups within the big city; but this hope is quickly dispelled by his realistic fear that new intermediate powers would then organize these small communities, and the new leaders would stultify individuals' desires just as surely as government bureaucrats now do. Trying to solve this dilemma between social cohesion and efficient leadership, he concludes (p. 207) that, "It is a riddle."

Existential uncertainty may even become Gamer's main cause. He defends Singapore against the smugness of technocrats who manipulate people's futures. He agrees with a UN report that this dense economy and society should be "treated with something more subtle than a steam shovel." But clearly he does not think it should not be treated at all. Social conscience, and a desire to organize more physical and political resources for poor city dwellers, effectively prohibit that. Gamer says, "Bureaucracy is keyed to dealing with only pieces of human life. . . . Industrialization has brought in its wake much economic suffering and social decay" (p. 206). As an antidote to excessively certain, specialized progress, Dr. Gamer's main prescription seems to be an open mind.

And half a teaspoon of revolution: "The world of factories and mines and plantations may be working at cross-purposes with the world where the majority of the earth's people live. As cities like Singapore and Kuala Lumpur progress in modernization, the possibility diminishes that their governments would ever desire to backtrack and take the road of Havana. So the gap widens as industralization progresses" (p. 189). If we do not ask too specifically about actual plans in Havana or the general popularity of backtracking, this statement still organizes some facts. Modernization is a painful, unsettling, and oppressive experience for most members of the societies where it occurs. But it is difficult to prove that "the poor will never achieve the power needed to live in big houses and dine regularly on fatted calves" (p. 192). They may eventually cease to be poor; and power may then not be handled in such a zero-sum way—even if these facts do scant little to help their present plight.

This book is not written at the fury level of Elsie Elliott's *Avarice, Bureaucracy, and Corruption in Hong Kong: Volume One*. It shows more ambivalence about development, about the impetus to plunge on with plans despite uncertainties in the result. Maybe a Carlo Levi novel, or the *Bhagavad-Gita*, or Nair, Hobsbawm, or Lanternari, could be included in the long and useful bibliography. This book should be read by all students of resettlement housing. It will be of interest to political scientists who study large cities where planning takes place. Finally, it is a primary source for anyone looking into post-technological values. There can only be agreement with the report on p. 127 that, "Many questions remain unasked and unanswered. One searches in vain for men with a broader vision. . . ." But some fine beginnings have been made.

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Competition and the Corporate Society: British Conservatives, the State and Industry, 1945–1964. By Nigel Harris. (London: Methuen & Co. Ltd., 1972. Pp. 384. $12.75.)

The title of the book leads one to expect a discussion of the Conservatives' economic policy—in particular their attitude toward managed economy and the welfare state, and the industrial relations, and their informal relationship with the big business. But the book is mainly a discussion of Conservative economic thought in the period between 1945 and 1964.

Several interesting points emerge from Mr. Harris's account: The first is the great diversity of opinions expressed by the Conservatives. Mr. Harris gives the impression that the opinion expressed within the Conservative Party covered almost the entire spectrum of political opinion in Britain. There were neoliberals, the radical corporatists, and the pluralist corporatists. The neoliberals harked back to the classical economy—to nonintervention, competition, and market mechanisms. By contrast, the radical corporatists pressed the case for expert planning and maximum state control of the economy. The pluralists occupied somewhat the middle ground between these two extreme positions, but they were at times indistinguishable from the neoliberals.

Mr. Harris, however, has not systematically spelled out what cluster of attitudes characterized each of his three categories—neoliberalism, pluralism, and corporatism. For example, he rightly points out that corporatism was no single unified doctrine and that there were many versions of this as it derived from various traditions such as those of Saint-Simon and Robert Owen. (He does not, surprisingly, mention the Marxist tradition.) But he does not clearly indicate the limit of this corporate thinking within the Party. On the other hand, he tends to give the impression that the advocates of corporatist thinking in the Party were at times more radical than those in the Labour Party. Surely no sensible conservative spokesman has ever taken the orthodox socialist position. This failure to define precisely the three categories and select representative spokesmen for each category and deal with their thought in detail and depth seems to have made the discussion general and diffuse.

The second interesting point highlighted by the book is that there was very little correspondence between the opinions expressed and the actual policies pursued by the Conservative governments. Even *The Industrial Charter* (1947), which was the Conservatives' response to the Labour Government's corporate and welfare measures, did not play any explicit role in the Conservative electoral campaigns. Nor were the Conservative governments at all influenced by the notion of "property-owing democracy" which Eden, Macmillan and others publicized with much fanfare as an alternative to Labour's so-called state socialism. Mr. Harris points out that the two parties differ more in rhetoric than in the policies they actually pursued when in power. He goes on to say, "an economic history could present a coherent picture of Government policy before and after 1951 without mentioning the change of government in order to explain what happened" (p. 135). And this, indeed, has been done by several authors such as Andrew Shonfield and Samuel Brittan. One therefore wonders whether Mr. Harris should not have set out right at the beginning the consensual nature of British politics and then organized the discussion of the Conservatives' intra-party debate in the context of economic policies pursued by the Labour and Conservative governments.

The third point that emerges out of Mr. Harris's discussion is that the Conservatives did not have a clear and coherent ideological position. They did not spell out what they wanted to preserve. Nor were they explicit about the sort of society that they were looking forward to. Mr. Harris correctly suggests that this lack of a coherent ideological position was partly due to the inherent difficulty in ascertaining which way society was moving at a time of rapid social change; it was also partly due to the coalitional nature of the Party and the reluctance on the part of the leadership to commit the Party to any detailed scheme which could prove embarrassing later.

In addition to being general and diffuse, and at times sweeping and obscure, the book lacks a proper perspective. There is no rigorous and sustained discussion of what set of basic attitudes characterized British Conservatives as distinguished from Liberals and Labourites. And this should not have proved difficult as in recent years several scholarly works have been published on 19th century Conservatism, in particular on Disraeli and tariff reforms controversy in the Party. Because of the failure to take serious account of these works, the author has mistakenly given too much prominence to Disraeli. But the fact is that the Tory Party that Disraeli led was a Peelite party rather than a Disraelian party. (Mr. Harris seems to have realized this shortcoming and has made an effort to overcome it in an appendix.)

Despite these serious limitations, the book, read along with Andrew Shonfield's *British Economic Policy Since the War* (1959) and Samuel Brittan's

Steering the Economy: The Role of the Treasury (1969), could be useful as it also highlights at least indirectly the consensual nature of British politics and raises implicitly the important question of the relevance of party ideologies in understanding the actual performance of British government.

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Popular Government in an African Town: Kita, Mali. By Nicholas S. Hopkins. (Chicago: University of Chicago Press, 1972. Pp. 246. $9.50.)

This is an admirable example of the growing literature on local politics in contemporary Africa. Hopkins gives us a detailed and spirited analysis of political life in a moderate-sized rural center in mid-1960s Mali, a poor West African country whose central government was vigorously pursuing a "revolutionary socialist option." The book concentrates exclusively on the town of Kita and its immediate environs, but Hopkins fully intends that his study should "cast light on other parts of Mali . . . and thus on the whole of the country" (p. 226). Under Hopkins's skilfull inquiry, the town proves an ideal microcosm.

As a social anthropologist Hopkins writes with refreshing disregard of most of the conceptual apparatus political scientists have brought to bear on the politics of underdeveloped countries and thereby teaches us much about the utility (or lack thereof) of our standard formulations. Hopkins begins by taking Kita, an evident arena of political activity in which men try to regulate their public lives; he then looks for the relevant boundaries, institutions and external links, and asks who is active in what ways, with what goals, using what strategies and operating under what constraints. This procedure leads him quickly to eliminate "ethnicity" as an important political variable. Kita is a multi-ethnic town, but the overriding identity is geographic. One is or is not a Kitan; under that level "solidarity based on residence or religion is more relevant for identity than the real or imagined differences between the various ethnic groups" (p. 52). Overall, an individual's site and length of residence, education, job, and personal abilities and connections are what count politically. Of ascriptive characteristics, only sex is of lifelong political importance. "Tradition" in the ideal sense of being opposed to "modernity," is just not a useful explanatory concept. For one thing, as Kita's history has shown, "in the political realm the introduction of new structures tended to imply the abandonment of the old" (pp. 16–17). In Kita, as in Mali as a whole, politics repeatedly involved a confrontation between those espousing "pragmatic" versus "ideological" or revolutionary approaches to political problems, but there is no useful sense in which one or the other of these approaches or the individuals espousing them could be considered more or less "traditional." Rather, these are personal orientations to political life, in kind not distinct from those found in most other countries, which in turn suggest strategies for political action and themselves become more or less attractive as a person's situation, constraints, and opportunities change. As Hopkins concludes, "The explanatory model that works best is one that assumes that each individual in the [Kitan] political field was able to decide what to do on the basis of his own personal interpretation of his advantage, and acted accordingly" (p. 219).

In his title Hopkins adopts a Malian political cliché, *gouvernement populaire*, and shows the basic sense in which the cliché is accurate. Effective and legitimate government in Kita and most of Mali for many, many years, has meant participatory government. Like all surviving political "traditions," participatory government is open to instrumental use at the same time that it sets limits on people's power. As Hopkins notes, "The legitimate Malian technique for obliging someone to do something is to make sure that that person participates in the decision to act" (p. 223). Hence, the frequent reliance on the ultimate organizational weapon, the mass meeting. Everyone attending one of these could be held responsible for the final decision, even if his role had been to question its wisdom; the cautious and calculating would thus contrive excuses for absenting themselves from meetings whose outcomes they feared. Hence also the flowering of political and parapolitical organizations: the various levels of the single political party organization; the civil service; the government-sponsored cooperatives; the youth movement with its middle-aged officers; the Vigilante Brigades; the athletic and theatrical groups, etc. Over all of these the Party held titular leadership, but it could ignore the strong desires of other affected organizations only at the peril of its own long-term effectiveness.

Political competition occurred as much within as between organizations and when left to develop inevitably took the form of two opposing factions each built around a powerful individual. This factional structure is characteristic of the Western Sudanic area and underlies the politics of regimes of widely varying ideology and purpose. Kitans would react to agents of the central government in part with a view to how their presence and policies would affect the ongoing local structure of political competition. Kita and Kitan politics thus posed a sharp challenge to the central government and its policies of rapid socialist transformation. While the local tradition of popular government fitted well with the central government's rhetoric and its sincere desire to involve the population in the execution of its plans, it also meant that the Kitans simply were not prepared to accept direction thrust upon them from the center. Political abstention, individual passive resistance to governmental policies, and subtle encouragement of the short-term payoffs from shifting one's factional allegiance greeted increased central government pressure to conform. When, in 1968 a military coup overthrew the government at its center, the government had long since lost legitimacy in most of Mali's Kitas.

Micropolitical studies, particularly when they are done with such skill and sensitivity as this one, enrich our understanding of African politics enormously. The monographs on the new African politics which appeared in the mid-1960s and concentrated on the center and the national level of government, can now be appropriately revised, thanks to our greater understanding of what in fact was going on out in the boondocks. Obviously, in Mali as elsewhere the analysis of national politics can never be reduced to amalgamating the micropolitics of all the country's Kitas. One finds, for example, no trace of the military in Hopkins's Kita. Perhaps the least explored and most difficult subject is the complex interaction between local and national politics. Bamako was a major actor in Kitan politics, but in its own way Kita was a major actor in the politics of the capital. Analyzing the links and conflicts between the two and the political uses people made of them at each level is a task that remains to be done. Anyone attempting it, for Mali or any other political system, can profit from reading this exemplary study of local politics.

William J. Foltz

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The Arabian Peninsula: Society and Politics. Edited by Derek Hopwood. (Totowa, N.J.: Rowman and Littlefield, 1972. Pp. 320. $11.50.)

The Changing Balance of Power in the Persian Gulf: An International Seminar Report. By Elizabeth Monroe. (New York: American Universities Field Staff, 1972. Pp. 79. $4.95.)

The Monroe book, with its assessment of increasing industrial world dependence on oil, considers the implications of The Report of an International Seminar at the Center for Mediterranean Studies (Rome) on which it is based as critical in the extreme. It is not difficult to see why as the Arab oil boycott, prompted by the 1973 October War, precipitated the "energy crisis," a now familiar label for a phenomenon that was virtually upon us at the time this book was compiled and that will be with us into the next century.

"A nation that is not dependent on oil is not a great power," according to one commentator (Monroe, p. 4). Perhaps this may not be true fifty years from now, but its truth for present-day Western Europe, Japan, and the United States is knowledgably summarized and interpreted by Elizabeth Monroe, Fellow of St. Antony's College, Oxford and author of *Britain's Moment in the Middle East* (1963). Western oil consumption (unfortunately) and Persian Gulf oil production (fortunately) have been badly underestimated, says Monroe. Americans, whom former Interior Secretary Udall once labelled as "energy pigs," now require for example what were the 1980 estimates. In the past the United States relied on its own and Western oil production, but today's figure of only 10 per cent oil imported from the Eastern hemisphere will grow to 50 per cent by 1980, notwithstanding my reading of Nixon administration energy policy.

There are implications in these few pages for the American balance of payments, overseas investment, overseas military commitments, our historic friendship for Israel, and even tourism. Alternative energy sources—nuclear, solar, geothermal, hydroelectric, coal gasification—cannot be substantially relied upon until about the year 2000 if then. Until adequate substitutes are found and developed, says Monroe, the flow of oil must be maintained, principally from the Middle East with Saudi Arabia as the swing producer. Americans, consuming one-third of the world's energy, will by 1980 face an annual bill of $70 billion for external oil. Demands by O.P.E.C. (Organization of Petroleum Exporting Countries) particularly Saudi Arabia, with the world's largest proven reserves, but also Iran and Libya, are for participation all the way to the pump. These demands pose political as well as economic questions, for Western Europe and especially Japan have few alternative sources or hydrocarbon reserves. The United States (Alaska included) even by estimates more optimistic than those reported by Monroe, has only ten years worth of oil reserves if it continues to consume at an exponential rate.

What is the political, social and economic impact of increasingly massive oil purchases on the producer countries? A background for Monroe's analysis is found in Derek Hopwood's collection on the Arabian Peninsula, which is increasingly important as an energy supplier. Hopwood suitably organizes papers presented by British, Arab, and American scholars at the Centre of Middle Eastern Studies at the School of Oriental and African Studies, University of London, and the Middle East Centre of St. Antony's College, Oxford during 1968–1969. These papers show how past circumstances condition much contemporary political behavior in the Middle East. In identifying forces for social change in this book's four sections, one can trace their integrative and disintegrative potentials.

History: Ahmad Mustafa Abu-Hakima limits himself to certain aspects of development while stressing Kuwait. Much of the internal policy of the oil rich shaykhdom of Kuwait, says Professor Abu-Hakima, has been dictated by the state of affairs in neighboring countries. The two-thirds of the population who are non-Kuwaitis, often Palestinians, create internal pressure. An hereditary amirate, Kuwait legitimizes its existence for its radical Arab socialist neighbors by Arab League membership and aid through the Kuwait Fund for Arab Economic Development (Hopwood, p. 39). In the Trucial Shaykhdoms, oil-rich Abu Dhabi and Dubai confront the regional powers of Iran, Iraq and Saudi Arabia in the wake of the 1971 British departure. The Union of Arab Amirates marks the Trucial Shaykhdoms' political effort to resolve historical differences and provide a defensive alliance.

George Rentz provides excellent background on Saudi Arabia, a development-oriented state operating in a largely traditional, partyless, religio-political framework. Saudi Arabia, as Rentz indicates, owes its unity to fundamentalist Wahhabis who to this day have provided a religiously based nationalism (Hopwood, p. 66). The author does not address himself to some important questions, however. King Faisal has ably continued the cautious modernization begun by his father, it is true, but can an Arab monarch, particularly in an age of radical Arab nationalism, be the vehicle of modernization? Can expenditure of the enormous oil royalties for a modern infrastructure within a staunchly Muslim framework with only token regard for Arab radical goals obtain national commitment?

Writing on Oman in the southeastern part of the strategically significant Arabian Peninsula, Wilkenson, Bathurst, and Kelly are keenly aware of social change and international intrusions antedating oil discoveries by centuries. The context, they generally conclude, may remain largely traditional, but contending elites in recent times have sought outside help in their struggles from such diverse sources as Arab socialists, the United Nations, London and Peking.

Political Developments and International Relations: A. B. Kelidar identifies three pressure zones challenging political stability. First is the familiar U.S./U.S.S.R. rivalry which as Monroe stresses may be compounded by Chinese influence. Second is the Arab-Israeli conflict whose resolution would entail unacceptable concessions by both sides. Third is the less well understood intra-Arab conflict whose Palestinian component Kelidar ignores.

Though Iran is a non-Arab regional power, says R. M. Burrell, it cannot escape great power rivalry or regional conflict. Iran has in the past occasionally exhibited expansionist tendencies. Its sizable but untried military machine may as the struggle for oil hegemony intensifies in the future engage in combat with Iraq, Kuwait or Saudi Arabia, this reviewer speculates.

Frank Stoakes applies structural-functional analysis to the oil-producing Gulf principalities. His most pessimistic position is that existing governmental structures inadequately cope with the stress induced by rapid change: "Social security passes with the old structures . . . it may take time for patron-client groups, political parties, social clubs and perhaps religious brotherhoods to fill the gap . . . " (Hopwood, p. 191). An optimistic position, to which he rightfully leans, is that ample oil revenues may make possible public acceptance of the political community and the regime itself. Material conciliation, he goes on to say, abounds for the general public and, of course, the well-cared-for security forces. Also, many traditional rulers are learning to play the populist game at home and to meet sensitive ideological foreign policy requirements.

Sociology and Culture: P. A. Lienhardt describes the inferior status of traditional women in Oman, while Thomas W. Shea usefully documents the rise of a Saudi, Aramco-trained middle class. He should be read along with William Rugh's "Emergence of a New Middle Class in Saudi Arabia" (*Middle East Journal*, Winter, 1973). Ali Muhammad Luqman, studying Southern Yemen, finds little commitment even in recent times to education and a vigorous press yet is optimistic about their role in liberating women and introducing democratic government.

Economics: Edith Penrose maintains that oil revenues have given area governments a dominant role leaving little room for private entrepreneurship. Since the oil industry is not labor-intensive, and since foreign corporations (instead of the host governments) collect most of the revenues, it is up to the government to develop distributive skills. Professor Penrose asserts that tough negotiations, resulting in what is perhaps the world's fastest growing store of capital, emphasize the need for extensive reorganization in state structures. (An estimate that is making the rounds now is that if the Arabs spend at least one-half of their oil revenues they will still have over $150 billion in gold and foreign exchange by 1985. The fact that the present *world* figure is $150 billion gives the picture. See, among others, *The Wall Street Journal*, November 1, 1973, p. 16.)

Yusif A. Sayigh highlights the disparities in oil and non-oil resources, human skills, population and land area. He proposes a broad federation of all ten political units on the Peninsula and the "rational" distribution of the oil wealth.

Industrial world policy makers preoccupied with maintaining the oil flow will benefit from these books. Many of the conclusions, of the Monroe book in particular, are shared by James E. Akins in "The Oil Crisis: This Time the Wolf is Here" (*Foreign Affairs*, April, 1973). Political scientists interested in comparative political systems about which there is little information will benefit from the Hopwood work. For instance, the contributors agree that religious authority has its uses in this supposedly secular age, especially since Islam and the state do not lend themselves to European style dichotomy.

There is a broader reason for our interest: expectation of dramatic change in Africa south of the Sahara has not been fully justified because of the comparatively modest capital inflow. In the core area of the Middle East, however, systemic transformation fueled by oil revenues may take place before our eyes on a scale heretofore unrealized in the so-called Third World. After reading these two worthwhile books with their picture of increasing abundance impacting on the oil producing nations this reviewer is prompted to ask: What will the effect of scarcity be on the democratic industrial users—at least until the arrival of workable fusion reactors in the next century? The question is: Can we in the West make systemic adjustments to a shrinking pie?

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Political Elite in Bihar. By Shashishekhar Jha. (Bombay: Vora & Co., Publishers Pvt. Ltd., 1972. Pp. 332. Rs. 25.)

Why is it that most of the empirical studies of Indian politics by Indians have proved disappointing, especially if they have been written and published in India? While a case can be made for the paucity of research support and the absence or publishing pressure in most Indian institutions. I think the reason lies deeper than this. With some few notable exceptions, Indian empirical political research has a *pro forma* quality about it. It is as if a methodologist, trained in the United States, told all his colleagues that they must have samples and interview schedules or questionnaires, and they must have frequency tables if they want to be respected in their field.

Methodologically, Professor Jha's empirical study of Bihari state legislators has a mechanical quality. The "sample" was intended to cover all legislators, but as should be expected, the author was unable to interview about a third of them, and the sample became an arbitrary one. Professor Jha writes about a "questionnaire" but does not include it, and one is tempted to conclude from other things he said that he simply had wide-ranging unstructured interviews. His treatment of these data are perfunctory and his use of descriptive statistics and cross tabulations is repetitive, confusing, and achieves negligible results. It may be that Indian society is too complex for such manipulations. However, computers are virtually inaccessible to most Indian scholars. Therefore, this may be an effective demonstration of why the simple transfer of American survey research methods to the Indian academic context often proves inappropriate.

On the other hand, in the historical portion of his book, Professor Jha has amassed a wealth of material about group, community, and factional politics in the state. His description of factional dynamics, though confusing, is perhaps the most insightful portion of the book. He demonstrates the incredible complexity of factional structures in India in which caste cleavage is no more important than personal ties, intracaste conflicts and intercaste alliances. He also indicates why factional groups are so transitory. "Two leaders of the same caste in the same rank inside one factional group (is) . . . impossible" (p. 93).

The book recounts the historical background of factional politics in Bihar and describes the role that caste groups, Muslims and tribal peoples have played in the state's shifting factional alignments. The political elite which Professor Jha describes statistically is limited to members of the state legislature, however, and excludes other political elites such as the higher civil service and Congress party organization leaders. Data are presented on the social origins and demographic characteristics of the legislators and some insight into their political opinions is also offered. To link his findings about legislators to political achievements in the state, Professor Jha attempts an analysis of the politics of agrarian reform in Bihar. The result is a case history only remotely related to the activity of Bihari state legislators. That might be Professor Jha's most telling point, although it appears to be unintended.

While Professor Jha's Indian English syntax will raise the hackles of most American readers, it is his handling of empirical data and his general lack of organization that makes this book difficult to recommend. A good source of material on the poor but important state of Bihar is long overdue, since it has a greater population than most countries (more than 47 million people). Unfortunately, this book only partially fills that gap. Furthermore, although recently published, the research for it was completed in 1964, before all the great upheavals attendant upon Nehru's death.

DOUGLAS C. SMYTH

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The Caciques: Oligarchical Politics and the System of Caciquismo in the Luso-Hispanic World. Edited by Robert Kern. (Albuquerque: The University of New Mexico Press, 1973. Pp. 202. $10.00.)

This is a stimulating series of generally first-rate essays devoted to exploring the meaning of a concept used by many political observers of the Hispanic world. *Cacique* is an indigenous word originally meaning "chief." If there is a consistent contemporary meaning, it is "local political boss," with a shading that emphasizes his monopoly of control and willingness to use violence.

The variety of leaders to which it has been applied is clear from the essays in the book. Karl Schwerin and Charles Gibson discuss the pre-Conquest and Colonial Indian leaders who evolved from autonomous chiefs to brokers totally dependent on Spanish authority. Robert Kern contributes a study of the stressful politics of late 19th-century Spain in which prominent figures condemned the cynical manipulations by leaders through local bosses called *caciques*. Eul-Soo Pang describes the role of local bosses in backlands Brazil called "colonels" (*coroneis*), plausibly included as a Brazilian version of *caciques*. Howard Karno narrates the history of a freebooting frontier entrepreneur and political boss in Amazonian Peru who became involved in international as well as national political deals. Antonio Ugalde applies the term *cacique* to the strong personalist leaders who emerged in a Mexican village after the Revolution and whose followings were gradually institutionalized into party factions. Wayne Cornelius describes some powerful individuals who control patronage and distribution of resources in some squatter settlements in contemporary Mexico City. Enhancing the book's exploration of meanings is the long, three-part essay by Marshall Nason, analyzing the manner in which the term appears in the literature of Spain and Latin America.

The reader will come away with a lively sense of the evolution of the concept and its various manifestations, and, I think, a sense of an underlying, constant phenomenon. He will not, however, get much help in defining that phenomenon nor in learning how to use it theoretically. In his brief and inadequate introduction (there is no conclusion), the editor urges the highly questionable proposition that *caciquismo* is distinctive to the "Luso-Hispanic world" and defines it in such a way that it refers to a large-scale system of politics (such as he is describing in Spain) rather than the local phenomenon which most authors in the volume describe. Nason does try systematically to analyze the use of the term in literature, but no one tries systematically to order its analytical content. Ugalde and Cornelius offer overlapping definitions, but there is no exploitation of the diverse essays in a way that would facilitate comparison.

There are parallels in the essays however, which might be the basis for comparative analysis. The most common explanations for the occurrence of *caciquismo* in the essays is the lack of a government presence in the boss's domain. The intervening factors emphasized vary from personal insecurity in the absence of legal guarantees, or lack of countervailing controls on the (naturally?) rapacious local boss. The weakness of the government may stem from the newness of the political unit in question (as in squatter settlements or frontier areas), the distance from national authorities, or the lack of determination on the part of authorities in policing local leaders.

A second sort of explanation often complements the first. The people or the local leaders or both are somehow felt to be especially prone to *caciquismo* either because of their Hispanic culture or backwardness. Earlier writers were much given to formulations about the ignorance of the people and the barbarousness of local leaders—especially those not from the traditional elites. More recent writings, including some in this book, are more apt to emphasize lack of socialization in handling modern political processes. This line of argument tends to assume that the central government as the repository of either aristocratic virtue or modern values is the logical agency to discipline the barbarous or backward *cacique*. Kern's notion that *caciquismo* had its heyday in the Liberal period of the end of the 19th century fits in with this view, making it appear that *caciques* were particularly prominent in the period of transition when mass politics was emerging and elite, aristocratic controls decaying, but modern political institutions not yet elaborated. But how then to account for the continued occurrences of *caciquismo*? They might be thought of as taking place in exceptional pockets where governmental services and laws are not effective. William Brisk, in a final article, tries to make a broader case for a "new *caciquismo*" by reference to the work on Latin American politics by Charles W. Anderson and others, suggesting that many Latin American nations do not have a strong, legitimate authority. Brisk suggests that various interest groups and institutions come to act as *caciques* in these circumstances, but this is stretching the concept out of recognition.

A search among the essays for help in assessing the significant consequences or role of this type of political structure is less rewarding. By and large, despite the diversity of situations and manifestations, most of the authors appear to find *caciquismo* simply terrible. Although Ugalde and Cornelius have more detachment, most of the authors seem to reflect the polemical and pejorative use of the term which was very strong around the turn of the century. With *caciques* come corruption, instability, brutality, and exploitation. Perhaps the term is too loaded to have a serious theoretical function. That it is considered "typically Latin" and retains such a loading is doubly unfortunate for a serious treatment of Hispanic politics. But that is not an uncommon situation. Perhaps *The Caciques* will stimulate others to articulate a more precise and flexible view of this type of politics. There is much in these essays to show the need and give an initial push.

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Politics in India. By Rajni Kothari. (Boston: Little, Brown and Company, 1970. Pp. 461. $4.50, paper.)

If there is any one Indian political scientist who has been closely associated with American social scientists in the 1960s it is Rajni Kothari, the author of the book reviewed here and the director of the Center for the Study of Developing Societies in Delhi. Kothari is the co-author (with this reviewer) of a book on Indian voting behavior; he collaborated with Sidney Verba in an international comparative study of political participation; he was associated with Philip Jacob's project on the cross-cultural study of values; he collaborated with Samuel Eldersfeld and the Michigan Survey Research Center on an international comparative study of voting behavior; he spent a year at Stanford in the Institute of Political Studies and at the Center for Advanced Study in the Behavioral Sciences; and finally, the book reviewed here is part of an important series in comparative politics edited by Gabriel Almond, James Coleman, and Lucian Pye.

This book reflects these multiple influences. Its central concepts are those currently employed in the study of political development and comparative politics: a focus on modernization, the growth of political and governmental institutions, the role of the party system, the social infrastructures of political life, the impact of political culture and political socialization, and the problems of state and nation building in a multiethnic environment. These concepts are all brought to bear upon the central proposition of this book—that there exists a distinctive Indian model of politics. This model is characterized by Kothari as noncoercive, open and competitive, with high and expanding levels of political participation and a well-developed administration into which the political structures and values of the country have penetrated. Within this broad framework Kothari examines how the political system works, then considers some of its limitations and "costs." He differentiates the Indian case from that of 19th-century Europe where "during the phase of rapid industrialization and social change, political participation was confined to the upper classes of society" (p. 9).

Kothari evidently takes pride in the accomplishments of the Indian political system even while he laments its problems and failures. Although he emphasizes the distinctive qualities of the system and its successes—at times defensively—he maintains a critical stance as a social scientist examining the workings of his own political system. Indeed, in some respects the book is organized around the contrasts between its successes and failures.

Kothari demonstrates that India has been successful in coping with a variety of political crises since 1947. The system has absorbed new elites from lower and middle castes; it has incorporated elements that had hitherto not taken part in village, state or national affairs; its national government has coped with linguistic demands for states' reorganization and a national dispute over language policy that threatened the country's unity; it dissipated secessionist tendencies in South India and in the Northeast; it has sustained a national party and with it a degree of central authority lacking in many developing countries; it successfully coped with two "succession crises" with the deaths of Nehru and Shastri; and Mrs. Gandhi, in a political tour de force, rescued the Congress party from internal factionalism and a loss of voter support that threatened not only the party but also the maintenance of stable central authority.

If the measure of system performance is the management of political conflict, then surely the Indian system ranks high, especially when one takes into account the enormous potential for both conflict and demand making in such a large, complex, participant system.

Why, then, is an elite that is so skillful in political accommodation and so responsive to conflicting interests and demands so unsuccessful in the management of the economy? Kothari, in his review of the government's economic policies, quite properly points to its accomplishments, even as he is highly critical of its failures. But a theme throughout is India's lost opportunities. Most of the agreed-upon economic problems, he writes—the low utilization of industrial capacity, the problem of unemployment, the high rate of population growth, the inadequacy of the educational system, and the misuse of the country's educated youth as a resource—are all inadequately handled by the Indian government.

Curiously, Kothari is unable to find an adequate explanation for the paradox of political accomplishments and policy failures. He suggests that "the requisite energy and drive that are needed are lacking" (p. 381), and after reviewing what needs to be done to stir the inert administrative structure he laments that "there seems to be a frightening lack of imagination" (*ibid.*). Earlier, he suggests that there are tensions in the system between "the objectives of growth and justice, of production and accumulation on the one hand and distribution and equity on the other" (p. 340). Neither of these two explanations is satisfactory, for neither rapid economic growth nor greater distribution and equity have been achieved; and while it is plausible that the simultaneous pursuit of both objectives prevents either from being maximized, it is also evident that neither objective has been pursued very vigorously. As for the lack of "energy and drive," the leadership has demonstrated that when it wishes to act "decisively" as it has on numerous occasions in both international and domestic political affairs, it can do so.

Perhaps Kothari could have found a more satisfactory answer if he had asked why the government has pursued economic policies that are so evidently wrong. Surely, it is not because the country lacks able economists or the present leadership has not received good advice. An examination of the noneconomic objectives of economic policies, the ideologies and beliefs that underlie these objectives, and the interests which they serve might provide more satisfactory answers. The argument that the government is committed to greater equity will simply not wash, not when government is so thoroughly engaged in policies that expand the size, salaries and functions of the bureaucracy to the detriment of both equity and growth.

What I find disappointing in this otherwise excellent work is that it does not do what an Indian social scientist is in the best position to do. It tells us a great deal about how politics works but very little about how government works; it tells us a great deal about how conflict is managed but little about how policies are formulated; it tells us much about what programs and policies have been adopted but little about why they do not succeed. Even if American political scientists were free to conduct research in India, as they are not, these are not questions to which foreigners could easily address themselves. If there is to emerge a creative and independent Indian political science it is with such questions, however delicate, that Indian scholars should be concerned.

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Social Groups in Polish Society. Edited by David Lane and George Kolankiewicz. (New York: Columbia University Press, 1973. Pp. xvi, 396. $11.00.)

This is the best scholarly book on contemporary Poland that has been published in recent years. It is composed of a number of sociological portraits of what the editors consider to be social groups in Polish society today. In each of these essays the historical development of the group is discussed first. The attention is focused on the group's involvement in various intra- and inter-group conflicts in the general political structure of the society.

The book is a pioneering effort in two ways: first because it attempts to present a new view on Poland, a country split by internal divisions and conflicts in spite of the rigid control of the authoritarian Communist regime; and second, because it is based on an extensive review of the vast Polish literature on the subject. Most of the data and some analytical perspectives are derived from it by the authors of the chapters in the book. In the final essay, David Lane comments on what the book has covered stating that the modern Polish society is not only divided into a number of diverse large groups but each of these developed a consciousness of its own group identity and separate interests. By making claims on the political systems these social groups play a political role in the system. He admits that the writers of the essays while showing that groups articulate demands, have failed to demonstrate how demands are aggregated in the political order of the regime (pp. 317–318).

David Lane develops the theoretical and linking framework for the other studies in two essays: the introduction ("Structural and Social Change in Poland") and the conclusion ("The Role of Social Groups"). These essays, though interesting, are not as innovative or revealing as the others. As in the literature published in Poland, Lane is evasive about whether or not the groups he outlines are in fact units of social stratification. The attempt to tear away the "blinders" of the "totalitarian" model is so strong in parts of these two essays that one gets the impression that the author is discussing a kind of Western democracy rather than a structure within an authoritarian regime which suppresses all forms of institutionalization of group interests not approved by the Communist Party.

The shortcomings of Lane's general theory have no bearing on the other chapters in the book. Paul Lewis's illuminating essay on the Polish peasantry is an important new link in the chain of works on that topic, studied in the past by such writers as Thomas, Znaniecki, Chalasinski and in the post-war literature by B. Galeski. Lewis analyzes changes in the Polish village. He is especially interested in contemporary conflicts between peasants and the Party. He shows how the State, unable to force the peasants to join the collective farms and unwilling to allow them to develop private enterprises, is attempting to "freeze" the situation for the time being. Polish agriculture, claims

Lewis, is still based on a peasant economy, which is neither capitalist nor socialist.

In "The Polish Industrial Manual Working Class" George Kolankiewicz considers two topics: the relations in industry between workers' councils and other organizations such as unions, the party, the administration and management of industries; and the living standard of workers as compared to that of nonmanual employees. He tells us that though the Party claims to represent the working class, this class has practically the lowest status and standard of living in the society.

Today as in the past, writers in Poland are seen as spiritual leaders and the embodiment of the national conscience. Nowadays, however, the Party claims to perform these roles. In George Gömöri's "The Cultural Intelligentsia: The Writers," we learn how writers today are still struggling to implement their inherited functions. In another essay, George Kolankiewicz undertakes a study of the technical intelligentsia. Again, the study focuses on changing conflict in society. In the past, the technical intelligentsia was polarized into technical experts and political activists. Today the conflict between a bureaucratic administration and innovative engineers and managers is becoming more important. Kolankiewicz also considers differences in the status of, and rewards for, nonproductive and productive activities.

In "The Local Political Elites," Ray Taras discusses the institutional structure of local authority and what may be regarded as rudimental institions of participatory democracy in the Polish Communist state. He describes the institutional vehicles of democracy at the local level which operate in Polish society. Even at this level, however, the Party imposes stringent controls, either directly or through various administrative branches of the local government. In addition, the same people who hold important positions in the Party or governmental institutions are often elected or nominated to positions in local councils. Still, the writer tells us, some semblance of democratic activities exists on the local level of societal organization.

The book does not give a general overview of Polish society and its current problems. Some very important and influential social groups—such as the military, the police, faculty and students, artists, the Catholic clergy, and the political apparatus of the Party—are not considered. This is understandable, however, since these groups are not often covered in Polish sociological literature either. The book also does not analyze such an important factor as the "Sovietization" of society, which is undoubtedly affecting the structure of social groups in Poland. In any case, it is a very important book in the field of East European studies, both as an introduction to the accomplishments of Polish sociologists, and as a fresh, innovative approach to Polish Social groups. It presents a good understanding of social and political mechanisms and of cleavages in Polish society rarely considered in Western literature.

Szymon Chodak

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The Middle East: A Political and Economic Survey, 4th Edition. Edited by Peter Mansfield. (London: Oxford University Press, 1973. Pp. 566. $19.95.)

Studded with ethnic, religious and linguistic minorities and seared by a large number of interstate and intrastate conflicts, the Middle East is perhaps the most difficult region on earth for political scientists to analyze. Sunni and Shii Moslems, Catholics and Protestants, Jews, Kurds, Copts, Maronites and Armenians coexist uneasily in a region characterized by unstable and frequently changing governments and periodically convulsed by fighting between Arabs and Israelis, Iraqis and Kurds, Lebanese Christians and Moslems, Sudanese Arabs and Blacks, Jordanians and Palestinians, and North and South Yemenis. In a well-organized and well-written volume of 566 pages, replete with charts and statistics, Peter Mansfield seeks to make the Middle East's complexities comprehensible, and while one may not always agree with the analyses presented by Mansfield, the primary author of the book, or by the other contributors, the book is nonetheless of high quality and clearly superior to the other surveys of the Middle East which have appeared since 1970.

Mansfield's book is divided into two sections: a series of thematic studies dealing with regional issues and problems, and a group of individual country studies covering all the Arab states (except the North African ones), Iran, Turkey and Israel. The thematic studies, of uneven quality and different authorship, deal with: Soviet and American policy toward the Middle East (Geoffrey Wheeler; Robert Hunter): the development of the oil industry (Edith Primrose); Arab political movements (Peter Mansfield); the origin of the Palestine problem (Elizabeth Monroe); and cultural trends in the Arab world (Jabra I. Jabra; Louis Awad). Except for the two very interesting studies of Arab culture, the authors of the thematic studies present a clear British perspective on political developments in the Middle East, one which tends to be both sympathetic to England's colonial policies in the region and highly critical of U.S. policy toward the Middle East, particularly its support of Israel. Indeed, criticism of American policy runs like a thread through the entire book, and while some of the criticism is valid, a number of the British writers' assertions are difficult to accept. Thus, in describing Syrian attitudes in 1957, Mansfield asserts (p. 470) "Above all, the American attitude toward Israel, which remained *unchanged* (emphasis mine) determined the attitude of the average Syrian toward the West, just as it was the main cause of any pro-Soviet feelings he might have." In making such an assertion, Mansfield seems to overlook the fact that American pressure in 1956–57 had forced Israel to withdraw from territories it captured in the Sinai campaign of 1956 and that the Syrians, still unhappy at the Turkish annexation of Alexandretta less than twenty years before, were very concerned about a possible Turkish invasion. Equally difficult to accept is Robert Hunter's criticism (p. 97) of the active American support for King Hussein's regime in Jordan in September 1970 when it was faced both by an uprising by the Palestinian commando organizations and an invasion of armored units from Syria. Indeed, the firm display of U.S. support for Hussein, when coupled with the Soviet Union's lack of support for its client, Syria, may be considered a turning point for American fortunes in the region, which had been declining since the June 1967 war. Following Hussein's victory, the pro-Soviet Syrian regime of Sallah Jedid was overthrown and replaced by the more independent regime of Hafiz Al-Assad, while Egypt, led by its new president, Anwar Sadat, (Nasser died during the Jordanian civil war) began to move away from the USSR and toward the United States.

Of the thematic studies, by far the best is the one by Elizabeth Monroe on the origins of the Palestine problem. Monroe does a real service to American political scientists and historians studying this knotty problem by making available the results of recent unpublished Oxford Ph.D. dissertations as well as materials from the private papers of individuals involved in the English policy-making process on Palestine. Interestingly enough, however, while downplaying the impact of "Zionist pressure" on English policy, Monroe sees that factor as the major influence on American policy toward Palestine, although she does not go so far as Mansfield (p. 349) who calls U.S. support for the Zionist cause in the 1945–48 period "ill-informed and sometimes irresponsible."

The country studies, all written by Mansfield, comprise eighty per cent of the book and are very good summaries of the main political and economic developments in each country with emphasis primarily on the post-World War II period. While little in any of the studies will be unfamiliar to Middle Eastern specialists, Mansfield makes available a number of very useful statistical tables on each of the major countries, including budget allocations, main trading partners, and major imports and exports by commodity group. The best two country studies are the ones on Lebanon and the Sudan, although the latter is somewhat marred by Mansfield's apparent bias against the Catholic church, which he blames for many of the problems in the southern part of the Sudan (p. 449).

One rather disturbing aspect of the book is Mansfield's lack of sensitivity to the plight of minorities in the Middle East. He treats the Turkish massacre of the Bulgarians in 1876 almost in passing, and gives little emphasis other than in the cursory introductory section to the massacre of Armenians by the Turks in 1915, the slaughter of Assyrians by the Iraqis in 1933, and the continuing persecution of the vestigial Jewish communities in a number of Arab countries. In addition, although the book tends to be pro-Arab in its treatment of the Arab-Israeli dispute, the Palestinian Arabs, themselves a minority in all the Arab countries except Jordan, do not receive a particularly fair or full treatment. In Elizabeth Monroe's study, no attention is given to the split in the Palestinian Arab community between the extremist Husseini faction and the more moderate Nashashibi faction over the White Paper of 1939; nor is mention made of the Husseini faction's active cooperation with the Axis powers in World War II—cooperation that cost the Palestinian Arabs as a whole dearly in the international deliberations on Palestine after the war. Mansfield, in his chapter on Jordan, fails to mention the role of the Palestinian Arabs in the battles for Jerusalem in the Arab-Israeli war of 1948, and gives all the credit for the capture of the Old City to the Jordanian Army.

Nonetheless, despite these areas of weakness, this is a very useful and worthwhile (albeit expensive) book, and it will serve both as a high quality introduction to the Middle East for those unfamiliar with the region and as a very handy reference book for Middle Eastern specialists.

ROBERT OWEN FREEDMAN

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The French Colonial Myth and Constitution-Making in the Fourth Republic. By D. Bruce Marshall. (New Haven: Yale University Press, 1973. Pp. 363. $12.50.)

Bruce Marshall has written a useful book. Of all the things which differentiate France from her European neighbors, none has been so strikingly visible as the impact of decolonization upon the domestic political system. Marshall's particular contribution to our understanding of this phenomenon is a history of French ideas about the proper relationship between the metropole and her colonies.

By "colonial myth" Marshall means "the

vision held by virtually all of the French elite of an indissoluble link between France and the colonies" (p. 2). Certainly he is right in seeing as the distinctive quality of French colonialism

> the seriousness with which French leaders embraced the ideal of an ever closer union between France and the colonial territories . . . (and their) conception of a worldwide community of peoples bound together into a single nation by common ties of economic and political interest, embodied in republican governmental institutions, and sharing a common cultural base (pp. 12–13).

Surely Marshall is also right in attributing some importance to this myth in explaining why the French had such a hard time coping with the development of divergent consciousnesses inside the Empire and the desire of peoples everywhere to shape their own destinies. France is after all the only European country whose regime collapsed because of this problem.

The book traces the origins of the myth back to pre-Revolutionary France, where we find both considerations of state and the civilizing mission. The Revolution added the concept of citizens endowed with political rights; the socialists and communists extended it to economic and social citizenship as well. Marshall's scholarship in documenting the interplay of these doctrines is impressive (and the bibliography is very helpful). After two chapters defining different conceptions of the myth, Marshall follows their development through key moments in the formation of French colonial policy: the conflict over the Blum-Violette bill in the thirties; the struggle between de Gaulle and Vichy for control of the colonies; the Brazzaville Conference; and, the focal point of the book, the writing of the Constitution after World War II. Chapter Eight is especially good, covering the years 1944–47, a fluid period when the possibilities for change were greater than usual. Marshall succeeds in juggling the extraordinary and multiple complexities of the French party system, the different Constitutional drafts, and the variety of local colonial situations.

What Marshall does less well is evaluate the importance of the colonial myth relative to other factors in explaining significant outcomes in French colonial history. Economics, domestic cleavages, the party system, international rivalries, *raison d'état*, greed, lobbies—these are not by any means absent from the book, but is it not clear what relationship the colonial myth bears to them. Is it a cause of outcomes? Is it a vehicle for description of outcomes? Is it a symptom of a rationalization, an obfuscation? Marshall does an excellent job of describing differences among liberal, radical, socialist, communist, conservative, fascist, and Gaullist attitudes toward the colonies. Precisely because there were different versions of the colonial myth, how are we to understand which one shaped policy making at a given moment? How does the colonial myth help explain the differing treatments by the same country of Indochina, Tunisia and Algeria, or Guinea and Chad? Why does the colonial myth decline (Chapter 9)?

The causes of colonialism and the causes of its decline, at least in its classic form, are very old issues in social science, with a lengthy history of debate behind them. I miss in this book a more explicit attempt to locate the argument in relation to other schools of interpretation. And I miss a comparative dimension, which would have aided in the ranking of causes. Is the difference between the French and, say, British conceptions of the link between metropole and colonies the most powerful factor in explaining the differences between French and British decolonization? Such is the implication of Marshall's book, though I am not sure he would accept it so baldly put. More self-consciousness about cause and effect would have helped us understand the lessons to be drawn from the vast amount of work and thought that went into this book.

PETER ALEXIS GOUREVITCH
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The Emergence of the Indian National Congress. By S. R. Mehrotra. (New York: Barnes & Noble, Inc., 1972. Pp. vi, 461. $22.50.)

Political development still remains fashionable as a concept or—to be more accurate—as a topic. Scholars such as Rupert Emerson and Samuel Huntington now have popularized the phenomenon of political decay in regard to the newly developing nations. The political track record indeed is dismal. Why, in this retrogression of development to decay, is India such a notable exception? Both internally and externally she continues to be plagued by serious problems, both natural and man made.

Dr. S. R. Mehrotra, a specialist in South Asian politics at London University, indirectly provides at least a partial answer. In the laboriously constructed *The Emergence of the Indian National Congress* he provides a detailed account of the coming together in different organizations of Indians in the eighty-year period preceding the establishment of the organization. Thus, in the mid-nineteenth century the reader almost physically can see emergent elites learn the art of political association. Finally, the apex of this early period of development, and the concluding section of the book, is the institutionalization of a national organization which in the following century becomes the major vehicle of the national movement for independence. And the Congress Party continues to provide leadership and cohesion to India today.

It is easy for a political scientist to criticize this volume because the 400-odd-page book ends just as the Indian National Congress is founded. In fact, it originally was intended as a preface to a work on the pre-independence Congress Party. Moreover, a mass of detail and numerous lengthy quotations reinforce the essentially descriptive approach. Thus, it easily can be marked with the pejorative labels of traditional, descriptive, nontheoretical, and historicist. On the other hand, its redeeming features are notable. Essentially, the book is valuable for two reasons centering around the subject matter and its utilization of newspapers.

A superficial reading initially suggests that this is yet another book describing the antecedents of the Congress Party. Mehrotra sets forth the old familiar names of people, provinces, and organizations. But, as the reader grinds away, the picture emerges of the birth of the Indian National Congress not as a skyrocket in the political sky but as an inevitable consequence of steady, unspectacular growth. The political process gradually subsumes compromise, negotiation, accommodation, and a wide range of leadership. Thus I see the subject matter as focused on the foundations not only of the Indian National Congress, but of what becomes the "Congress system" or political process which has so distinguished India from most other newly developing countries.

An important element in the socialization process included British nonofficial residents who were nicknamed the "interlopers" by the British bureaucracy. British lawyers, journalists, teachers, missionaries, planters and traders acted as they did at home. "Their precept and example taught their Indian fellow-subjects the art of constitutional agitation" (p. 4). Indian pupils were sufficiently advanced so that the lessons could be applied in the controversy over the widow-burning issue in 1829–30. The rapid spread of English, particularly among the elite in Bengal, enabled the British interlopers to reach increasingly large audiences. George Thompson came to India in 1843 to argue passionately for the development of voluntary organizations and constitutional agitation. A contemporary newspaper, commenting on the crowded public meetings he addressed, noted that in the recent past only a handful of Indians would have been able to understand the eloquent Britisher. Now, in 1843, "we find hundreds of educated natives listening with admiration . . . " (p. 26). The author agrees that Thompson is appropriately described as the "father of political education in India."

Most famous of all the interlopers in the nineteenth century is A. O. Hume. Students of the period are familiar with his career in the Indian Civil Service, his retirement and subsequent efforts leading to the convening of the first meeting of the Indian National Congress. Mehrotra's contribution does not end for all time the argument over whether or not Hume properly is glorified as the "father of the Indian National Congress." Other individuals, virtually all Indian, already were working in this direction and in time would have served the midwife's function which Hume in fact performed. What is notable in Hume's endless traveling and letter writing is the series of India-wide communication links (see p. 359) which were so well formed by the time the first Congress met in 1885. A rich organizational and human growth preceded the inauguration of the Congress.

Indigenous organizational growth and the development of a civic culture are more the theme of this volume than is the role of "interlopers." Mehrotra provides detailed descriptions of a train of organizations from the Bengal Landholders' Society in 1838 to the culmination in 1885 of the political coalition which originally was to be called "the Indian National Union" (p. 400). Failure rather than success often characterizes the political efforts of the period. Organizations rise and quickly wither and die. But amidst an ever widening elite and increasing politicization, substantial organizations grow in the major cities of India and even begin to reach out into some hinterland towns.

Unfortunately, a rudimentary model developed in the middle part of the book is not followed consistently. Mehrotra states that two different models of national organization were current in the 1870s. One was a notion of centralization with control over affiliated associations. A decentralized alternative proved to be more practical for organizations like the Indian Association which initially desired centralization (p. 295). One hopes that the author will probe the significance of the adoption of this model by the early Indian National Congress in his subsequent volumes.

The voluminous documentation and quotations from contemporary newspapers constitutes the book's greatest asset but also presents a serious problem. A participant's context is accurately created, and an over-reliance on British official records is avoided. But unnecessary repetition results in tedium.

The book's rich descriptions of organizational growth and faithful reproduction of the period recommends it to South Asia specialists. It also can serve other third-world specialists for needed comparative studies on the early development of associational life. Unfortunately, it is priced at an inflationary figure.

Paul Wallace
University of Missouri, Columbia

Working Papers on Canadian Politics, Enlarged Edition. By John Meisel. (Montreal: McGill-Queen's University Press, 1973. Pp. 257. $6.00, paper.)

This is not the typical academic book in which an author systematically examines a subject either continuously or in essays. Rather, it is a collection of five apparently unconnected and lengthy articles on recent Canadian electoral and ethnic politics.

John Meisel, an accomplished and well-known student of Canadian elections, claims that he was prompted to present these pieces in this fashion for several reasons: Three of the articles make available some material derived from a national survey of 2767 eligible voters he carried out after the 1968 federal election before his more elaborate projected study on that election is published. They also contain data that might not otherwise find their way into print. Another paper on French and English Canada is a more complete version of an article in the collection *One Country or Two?* (Montreal, 1972). Finally, this edition has been enlarged over a previous one which appeared early in 1972 and contains a new article on the 1972 election.

Meisel does himself an injustice in not attempting to link the articles with an introduction outlining a common theme, for the five essays all relate to each other rather directly in dealing with factors that make Canada the cleavage-ridden society it is today.

Students of voting behavior will probably find most interesting the three papers on the 1968 election—especially the first one. It covers the bases of party support in 1968 in global terms dealing with such topics as the usual social variables, general issues and voters' perceptions of the parties. A considerable amount of data and descriptive commentary is presented, some of the findings showing changes from past behavior, others revealing patterns of Canadian political affiliation that have continued over time. Unfortunately, Meisel does not differentiate between the new and the old either by footnotes or direct textual reference. So unless one were fully knowledgeable about past work in Canadian voting, one might assume, for example, that before 1968 no one had observed the impact of leadership on partisan choice, the marked strength of the Liberals in the major metropolitan areas, the significant working-class support for the New Democratic Party, or the pronounced dislike of the prospect of minority government by large numbers of voters.

At the same time, the author presents some new material and much that is insightful. One especially interesting point is that the heterogeneity of Conservative compared to Liberal support places considerable strain on the Conservative party, particularly on its leaders. Another is that while the Liberals are often seen as the party of French Canada, in 1968 they were far more the party of the English and people of minority ethnic origin in Quebec.

Yet another fascinating observation is that since underprivileged groups tend to vote for the out-parties, the NDP and the Creditistes, the political system cannot be as responsive to them as it might because Liberal and Conservative party and governmental elites are thereby automatically less exposed to pressures from the less-privileged than they might be were there more such groups among the ranks of their parties' voters.

The paper on party images analyzes the parties in terms of semantic differentials and compares public responses to items on each of the four parties as well as an "ideal" party. It also reviews the differences between these results and those obtained from similar techniques in a national survey conducted after the 1965 election. After systematically dredging the data covering eight socioeconomic groups, the author concludes by suggesting that aside from showing that many groups appear to have low expectations of the political system, more work needs to be done with such an instrument if it is to have much explanatory value.

In the last paper of the three directly emerging from his 1968 survey, Meisel relates values, language, and political alignments. He resists describing Canada simplistically as if it were divided into only two linguistic groups. He feels there are five, which he labels "pure English," "partial English," "mixed," "partial French" and "pure French." He then proceeds to analyze these groups in terms of what he calls private values: religiosity, moral values, (for example, reactions to homosexuality and divorce), law and order, and ethnic and religious toleration. He then considers public reactions to some leading political figures, foreign policy, the monarchy and constitutional change, and the place of Quebec in the federation.

The data illustrate the formidable barriers to a countrywide consensus: first of all, they suggest that the ethnic-religious cleavage in Canada continues to dominate no matter what the momentary issues; and, second, that the issues themselves are cut so deeply across ethnic-religious and regional lies that political elites have little leeway in using them to mobilize national majorities.

The essay on French and English Canada and the options confronting the country as it tries to manage the conflict between these two groups is very much a normative piece. Meisel comes out strongly for compromise, but this prescription does not keep him from a penetrating elaboration of the positions of both French and English.

I consider the final article entitled "Howe, Hubris and '72: An Essay on Political Elitism" the best of the lot, despite its impressionistic quality. It was written during the week following the 1972 federal election, and in 35 pages Meisel manages to capture the essence not only of the just-completed campaign and four years of the Trudeau administration but also of the Liberal party's governmental style since World War II. He concludes that what was largely responsible for the Liberals' close call in 1972 (they obtained 109 seats to the Conservatives' 107, while the NDP won 31 and the Creditistes 15) was not the English backlash against their policies on bilingualism, as Trudeau and many others have claimed, but rather their failure even to appear to be interested in dealing with issues that polls showed Canadians to be concerned about: unemployment, inflation, high taxes, and welfare. Such lack of concern he terms *hubris*—an overweening pride and insolence leading inexorably to downfall—which afflicted the Liberal government in its final years under St. Laurent in the middle 1950s and has continued to characterize the Liberals under Lester Pearson and then Pierre Trudeau. He illustrates his charges of arrogance against the Liberal party by referring to the way it runs the Federal government, the manner in which it recruits to high office, the social background of Trudeau himself, and the general Canadian political culture which reinforces the now ingrained pattern. The article is a *tour de force*.

If there is a general criticism to be offered—and perhaps this is unfair since Meisel claims that much of what appears here is preliminary—it is that the reader is in danger of being overwhelmed with detail. The articles are each extremely valuable but they are marred by the author's reluctance to engage in either generalization or theory testing. Nevertheless, until the projected more comprehensive volume appears, this collection can still serve as a useful up-dating of the literature on partisanship in Canada. And, at the very least, Meisel's insights will undoubtedly spur further research by some of his readers.

PETER REGENSTREIF

University of Rochester

The Soviet Armed Forces, Books in English, 1950–1967. By Michael Parrish. Hoover Institution Bibliographical Series: XLVIII. (Stanford, Calif.: Hoover Institution Press, 1970. Pp. viii, 128. $7.50.)

The development of the Soviet Union into a nuclear "superpower" has been accompanied by unusually frank discussions among Soviet military spokesmen on the implications of this "revolution" for Soviet military doctrine, strategy, and force composition, as well as on the general questions of security and war in the nuclear age. Mr. Parrish's bibliography provides an invaluable reference source and guide for the student and professional wishing to reconstruct this important period of U.S.-Soviet relations or to study the evolution of Soviet views on war and strategy. The 2146 entries, organized under twenty subject headings, cover almost all of the significant publications on military subjects which appeared in the Soviet Union between 1950–1967 and the interpretations put on them by Western scholars and analysts. Because of their limited circulation at the time of their publication, some of these translations and studies, heretofore not generally known, can now be located with the help of this bibliography in any of the U.S. government depositional libraries.

LEON GOURE

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Elections in South Vietnam. By Howard Penniman. (Washington, D.C.: American Enterprise Institute for Public Policy Research and Hoover Institution on War, Revolution and Peace, 1972. Pp. 246. $3.50.)

This is a book to inform, not inflame. As suggested at the outset, "Almost everyone has an opinion about the electoral institutions of South Vietnam—and almost no one has bothered to study them" (p. 1). Professor Penniman's study is the most comprehensive account of the seven national elections held in South Vietnam from 1966–1971 yet to appear.

I have rarely seen an author be so fair or modest about an event directly observed. As a consultant to the team appointed by President Johnson to observe the 1967 elections in Vietnam, Professor Penniman in no way assumes the role of an instant expert on Vietnamese politics. The descriptions are straightforward and the period under study thoroughly researched. The myriad of rules and practices affecting the electoral process are systematically presented, campaign periods fully described, and criticisms and evaluations succinctly discussed. The volume also contains the best account of Vietnamese journalism I have ever read. The appendices contain a brief description of the elections held in North Vietnam since 1945, a fascinating account—the fullest I have yet to see in print—of a campaign appearance by Thieu and Ky and the ten other slates in the 1967 presidential election, and three highly detailed tables of election returns. Overall, the volume should make an important addition to the bookshelves of those interested in comparing electoral processes. It invites and facilitates further analysis.

Substantively, the volume deals with how the nature of the electorate, the level of competition fostered in the elections, and the institutions closely related to the electoral process affected the quality of the elections. In a certain sense, the analysis can be seen in the context of testing Richard Scammon's observation which Penniman quotes, namely, that the elections were "reasonably efficient, reasonably honest, and reasonably free" (pp. 75–76). Professor Penniman concludes that they were. The definition of the electorate maximized universal suffrage, the registration process was inclusive and fair, ballot secrecy was protected, and there were adequate safeguards against voting fraud. Was the count honest? This, of course, has been a persistent criticism of Vietnamese elections and Professor Penniman persuasively argues that it was. Moreover, he says, even if returns were inflated to the full level charged by all groups, the outcome of the particular elections criticized would not have been different. In addition, the elections were covered by an open and critical press and were characterized by competitiveness for all positions.

The institutions that emerged from the elections studied here reflected the complexity of the society at large as well as the weakness of its political party system. The former emerged in spite of the regime; the latter, because of it. In sum, this study illustrates that elections can be democratic (i.e., competitive, open and fair) where political systems are not.

Allan E. Goodman

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Technology for Developing Nations: New Directions for U.S. Technical Assistance. By Rutherford M. Poats. (Washington, D.C.: The Brookings Institution, 1972. Pp. ix, 255. $7.50.)

Rutherford M. Poats, an experienced official of the United States foreign assistance program, has written a thoughtful and careful book on the export of technology to and the harnessing of technological skills and strengths in developing countries. He chooses his examples from six key areas in which technology is essential to development: agricultural research, industrial growth, health, population, nutrition, and education.

He has selected from these experiences evidence of opportunities for success. He makes a strong case for applied research and for concentrating U.S. technical assistance on activities "where the requests and needs of developing countries match actually available and superior U.S. capacity to help" (p. 14). At a time when the prospects for major U.S. public investment in foreign technical assistance are dwarfed by the needs for such development, Poats's book offers modest and practical counsel. Throughout the book are useful recommendations, wise advice, and a sensitive understanding of the paternalism which has so often wrecked foreign aid. He encourages programs by such organizations as the National Academy of Sciences, universities, and professional societies on that modest and carefully tailored scale which U.S. political and fiscal realities dictate. One wise counsel he offers, which the United States could well have followed earlier is: "Not until it is clear that the climate and institutional network for technological innovation in the requesting country are being systematically improved should U.S. assistance for public research centers be considered" (p. 80).

Mr. Poats's sense of priority for the most promising targets for U.S. technical assistance deserves attention. He sees, for example, in higher education the need to shift from generalized support to "particular institutional changes aimed at bringing professional and subprofessional training into closer and more efficient association with research and operations in each field" (p. 233). To promote industrial growth he urges technical assistance designed to improve "indigenous capabilities, both in private enterprise and in public institutions, to acquire, adapt, and use industrial and natural resource technologies" (pp. 84–85). Unfortunately, that formulation, while certainly correct, may understate the difficulties of doing it, which Mr. Poats, of course, understands.

Throughout the book are references to underlying political questions, both as to the capacity of the United States to organize and sustain an intelligent and sensitive commitment of resources to technical assistance on a permanent basis, and the political capacity of various developing countries effectively to use and channel modern technology to the benefit of their citizens. The modest-scale effort which Mr. Poats proposes requires greater sensitivity, greater understanding of political forces, far more competent institutions and mechanisms of collaboration and implementation than have existed to date and, above all, wise leadership by advisor and advisee alike. He sees the need for "a coherent national development strategy that brings to bear fiscal, financial, and political tools as well as technical instruments" (p. 8). He readily acknowledges that AID has "tended to downgrade technical and social issues and the staffs representing these interests" (p. 9). He acknowledges in the context of health programs that "AID did put its primary trust in the ultimate health giving powers of general economic development" (p. 109) and notes that "the politics of many developing countries tend to alienate both local and American experts" (p. 238).

But the book doesn't really address these fundamental political questions. AID has been run according to the notion that economic development is central and comes first, and that political and social development are secondary, separate, and come later. This is simply not true. Politics comes first and probably second, too. The setting of a political system—the capacity of a society to organize itself to do things, the spirit, the drive, the purpose, the leadership—that is what makes things happen. One needs only to look at the successes of Puerto Rico and Israel, where politics made economics. But developing nation governments, where corruption is often a way of life and political and bureaucratic institutions are less developed than a primitive economy even, are not institutions in which fine-tuned technological and economic assistance of a sophisticated sort can be effective. Poats has written a useful book which all interested in these important questions should read. But the political dimensions he clearly recognizes still require systematic treatment.

BREWSTER C. DENNY
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Tito, Mihailović and the Allies, 1941–1945. By Walter R. Roberts. (New Brunswick, N. J.: Rutgers University Press, 1973. Pp. xv, 406. $15.00.)

After so much bad political history over the past twenty-five years on a complex and controversial subject, this book is as welcome as the morning sunshine following a prolonged storm. It depicts a Yugoslavia overwhelmed by Nazi military might, partitioned, occupied, and soon the scene of a bitter civil war between two resistance movements, forcing the major allies to make difficult policy choices. The author, an associate director of the United States Information Service, does a superb job in summing up the major developments in Yugoslavia (1941–1945), and the way in which Great Britain, the United States, and the Soviet Union influenced those developments.

Ironically enough, as Roberts demonstrates, the Big Three acted in support of what they conceived to be their short-run interests (winning the war), and essentially against their long-term goals (an ideologically preferred political system). For Churchill and Roosevelt (largely at the former's urging), Tito's Communist-led Partisans were killing the most Germans and therefore should be supported. Stalin, on the other hand, believed that Yugoslavia's greatest contribution to the war effort was in avoiding civil war. Consequently, he urged Tito to assist General Mihailović and his Chetniks, who were the first to raise the flag of resistance. Only when it was too late, did Churchill begin to worry about the implications of his actions, as he saw Tito turning against Britain and the U.S. when he no longer needed them.

Perhaps the book's greatest service is in throwing light on the leaders of the two resistance movements. Neither Tito nor Mihailović desired an Axis victory. Both wanted the Allies to win and assumed they would. That being the case, Tito's main concern was to insure that he was in power when it was all over. Mihailović, on the other hand, wanted to prevent that from happening. He would have preferred to avoid the terrible Nazi massacre of hostages (usually 100 to 1) following the killing of German soldiers, and to follow Western Allied instructions to European resistance movements to lie low, to husband their resources, and to await the signal of Allied landings on the continent. But Tito's actions forced him to fight against an imposed political solution, preferring that Yugoslavia's future be determined by the politicians and the people after the war. It is not surprising that the one who fought for a particular political solution, especially when aided by the Allies, should win in such a contest.

Roberts explodes the myth that Mihailović was somehow a traitor because he fought against the Communist-led Partisans and because some of his men made accommodations with the enemy. Moreover, on the basis of German and Partisan documents, the author shows that Tito's men were also negotiating with the enemy, and at a higher level. Three of Tito's top associates (including Milovan Djilas), in talks with Nazi representatives, asserted in writing that the Partisans regarded the Chetniks as their main enemy and if left alone would fight only them. From the point of view of the Nazi leaders in Berlin, however, both Tito and Mihailović were enemies of the Third Reich, and the Nazis offered the same reward for their capture dead or alive.

In the end, Tito won the struggle because (1) he never lost sight of the long-term goal and placed great emphasis on organization to achieve it, (2) he operated in territories not actually occupied by the Germans (the opposite was true of Mihailović), (3) he successfully sold himself and his movement as doing the greatest damage to the Axis in Yugoslavia and thereby built up his military potential with Western aid, and (4) other events and problems progressively made the Balkans seem less important to the West, making it easier for the Soviet Union to assist Tito in consolidating his gains in the fall of 1944 and the spring of 1945.

Specialists may wish that the author had covered some topics more fully, and they may find some issues to argue about, but they cannot deny the importance of this much-needed book.

ALEX N. DRAGNICH

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East and West Germany: A Comparative Economic Analysis. By Martin Schnitzer. (New York: Praeger Publishing, 1972. Pp. 446. $22.50.)

While recognizing that East and West Germany started with different capabilities and have "different sets of priorities," Martin Schnitzer, Professor of Finance at VPI, feels that it is possible to make "penetrating judgments" about "which one has performed better over time in terms of economic development and providing for the welfare of its citizens" (p. v). The author compares the modified market-directed West German economy with the plan-directed East Germany economy in terms of institutional structures, levels and rates of economic development, and patterns of income distribution. Emphasis is placed on the 1960s, with separate chapters surveying the political system, the organization of industry and agriculture, the fiscal system, the monetary system, income distribution, and the role of labor unions in each country. Only the introductory chapter and two concluding chapters are truly comparative.

Among the most useful materials covered in these chapters are a succinct summary of the impact of taxation and social welfare programs on income distribution in West Germany, a description of the East German wage system, and a discussion of the role of the Bundesbank in the efforts to combat inflation in West Germany in the late 1960s and early 1970s—this last a concise, readable account of the difficulties encountered in attempting to use monetary policy to control inflation in the existing international economic climate.

For political scientists the greatest value of Schnitzer's work is that it brings together a vast amount of comparable economic data for two important political systems. The author gives us a mixed picture of economic development and social welfare, with both countries enjoying substantial success, while the gap between them "has not narrowed to any significant degree" (p. 386).

Two major weaknesses limit the utility of this book for the political scientist. First, it contains almost no discussion of the politics of economic policies. For example, there is little mention of the multiple issues and personalities involved in the 1963 New Economic System (NES) reforms in East Germany, despite the fact that Schnitzer deliberately stresses the postreform period in his analysis of performance. Similarly, there is no discussion of the issues, personalities, and decisions involved in the late collectivization of agriculture in the GDR, which took place throughout the 1950s and increased in intensity after 1959. As far as the West is concerned, the fundamental differences in the fiscal and monetary policies of Ludwig Erhard, architect of the "Economic Miracle," and Karl Schiller, Willy Brandt's Minister of Economics, are reduced to three sentences (see p. 163). Thus, one looks in vain for anything resembling the in-depth policy analysis found in George Feiwel's *The Soviet Quest for Economic Efficiency: Issues, Controversies and Reforms* (New York: Praeger Special Studies, 1972), or Abraham Katz's *The Politics of Economic Reform in the Soviet Union* (New York: Praeger Special Studies, 1972), other recent publications in this series.

Second, the author has limited the value of his study by defining system performance in narrowly economic terms. For example, development is measured in terms of output levels, growth rates and efficiency in resource utilization, while public welfare is measured by real per-capita consumption and the distribution of income. While these are legitimate evaluative criteria, values other than growth influence resource allocation—particularly in East Germany, where the maintenance of party supremacy and a socialist system of production and distribution are important values. It is widely recognized, for example, that progress in economic liberalization and rationalization in East Germany, not to mention the Soviet Union, were affected by the events in Czechoslovakia in 1968. Indeed, one wonders how valid is the author's contention that one can make "penetrating judgments" about system performance when priorities differ. By choosing to adopt external performance criteria, rather than to deal directly with the multiple objectives of elites, Schnitzer has sacrificed some measure of scope and political relevance for manageability. In spite of these limitations, the effective integration of economic data in a comparative framework suggests that the volume will serve as a basic reference source for scholars interested in the two German states. It may also be of value to scholars interested in the methodology of comparative analysis.

GERALD G. WATSON

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Latin American Modernization Problems. Edited by Robert E. Scott. (Urbana: University of Illinois Press, 1973. Pp. vi, 365. $12.50.)

Eight contributors representing six disciplines writing about four countries—never more than two essays on the same country or problem—may make a collection; they do not make a book. While the unifying theme purports to be "modernization," this concept remains so amorphous that thematic unity is more apparent than real. As editor and senior contributor, Robert Scott does have some interesting things to say about the region, but these observations (discussed below) neither underlie his colleagues' research nor grow out of their findings. Indeed, as will be noted, in some instances the case studies contradict Scott's generalizations.

Given such diversity, it is impossible to do justice to the contributors—all of whom are associated with the University of Illinois' Center for Latin American Studies. Suffice it to say that the book contains something of interest for those who pursue modern Brazil, Colombia, Mexico and Peru; race, political culture, external financing, agrarian reform, military government, urban migration and marginal communities. My comments will be limited to Scott's contributions, totaling nearly a third of the volume, which are presumably of greatest interest to readers of this journal.

Those familiar with Scott's writing will not encounter much that is new, except perhaps greater pessimism regarding the prospects for political integration within these nations. Scott continues to assume that Latin America is in a "transition period," "mov[ing] . . . down the road to modernity;" that modern technology operates inexorably to erase regional and national differences; and that the "convergent" institutional responses to this technological imperative constitute "development," "successful modernization," "westernization"—without its mattering much in Scott's eyes which term is employed (pp. 2–5, 21). This view is qualified, however, by such statements as "there is no unilinear line of development," "such observations are at too high a level of generality to provide theoretical proof," the substitution of "may be" for "is," etc. (pp. 5, 21, passim). While some readers may appreciate Scott's caution, I find this "no risk" style of discourse—a style endemic to academic social science—troubling in its elusiveness. It permits the author to convey a point of view without taking a clear and arguable position.

"Most persons in the emerging areas," comments Scott, "view the development process in a rather simplistic manner, based on a misunderstanding of the western models" (p. 10). To the contrary, I find Latin American writers and leaders often more sophisticated than their U.S. counterparts in realizing that "development" need not mean "westernization" or even "modernization." Peru, Cuba, and Brazil are more technologically modern than they were fifteen years ago. They are also more *varied* institutionally (less like each other in important ways), in part because each has opted for a different mode of incorporating that technology. Even in these studies edited by Scott there is evidence of culturally specific, pre-industrial traits surviving the supposedly leaching effects of modern technology and its "required" social relations. Cartegena's race relations continue to resemble nothing seen in most archetypal "modern" nations; Zé Maria's "traditional" values find reinforcement in a modern, urban factory; strong central governments under military auspices suggest a resurgence of traditionally Hispanic forms of authority.

In some matters Latin America will become like us whether or not it chooses to; in others it has a choice; in still others it probably can't resemble us, want to or not. Delineating the scope and content of these categories would seem close to Scott's central concerns, yet it is a question never confronted openly and systematically. When discoursing on "modernization," Scott usually emphasizes the first category: inescapable, standardizing influences emanating from the centers of world production and communication. While describing recent political trends in the region, Scott injects a large dose of category three, leaving the reader with the impression that these Latins will never overcome their parochialism and adopt the modern, the universalistic, the functional way of doing things. Both perceptions may be true, but if so, we need to be shown *how* they intermesh and *which* things fall into which category. Finally, I don't believe Scott takes category two, the area of conscious choice, very seriously. Brief references to a "socialistic approach" as being "economically oriented" "much in the style of the Soviet Union" (p. 11), suggest that Scott has closed his mind to the ways in which Cuban leaders, for one, have modified some of the "immutable" laws of modernization (reversing urbanization, for instance, or narrowing inequalities during the phase of capital accumulation).

In the final chapter Scott provides an overview of recent political trends, revealing his deep familiarity with a region he has studied for a quarter century. As in previous writing, he sees integration as the central problem of countries beset by "contending traditional and emerging interests," each possessed of "particularistic" values and prone to use extralegal methods (pp. 312–3, 355). Popular parties have failed to provide integration; now the military are trying their hand. Scott doubts they will succeed, concluding that integration will have to emerge slowly and painfully from "the disorder of a more open system" (pp. 355–6).

This is a somewhat Perucentric view, that being a country Scott knows particularly well. Brazil hardly conforms to his generalization that military governments "recognize a need to mobilize the popular majorities" and "seek to expand the proportion of lower-class citizens sharing in the benefits of the national economy" (pp. 350, 335). Likewise the "declining influence" of popular parties, linked to their inability to "cut across regional, social and class lines" (pp. 322, 325), scarcely fits Argentina, where the Peronists have triumphed over the military's efforts to disperse or co-opt them. But any attempt to discern trends in a region as diverse as Latin America must be procrustean, and Scott's is actually less so than most.

This book, then, contains detailed case studies alongside broad generalizations. Both are interesting in their own right, although one suspects they appeal to different audiences. Unfortunately, the promise held out by juxtaposing the two in an expensive book is not fulfilled.

ELDON KENWORTHY

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Revolutionary Education in China: Documents and Commentary. By Peter J. Seybolt. (White Plains, N.Y.: International Arts and Sciences Press, Inc., 1973. Pp. 408. $15.00.)

Peter J. Seybolt, assistant professor of history at the University of Vermont and editor of *Chinese Education: A Journal of Translations*, has collected thirty-two transtions of articles written primarily for Chinese consumption and focusing upon recent developments in China's quest for educational innovation.

While the title of the book possibly suggests a broad historical scope, only six of the translations pertain to the pre-Cultural Revolution period. The remaining 26 translations, comprising the focus of this compilation, were written between 1967 and 1972, and pertain to the general educational goals adopted or renewed during the Cultural Revolution, problems of administration, the roles to be played by the teachers, special educational problems created by immediate economic needs or the past behavior of certain of the students, and the directions to be followed in elementary, secondary, and higher education.

Seybolt, in his 51-page introductory commentary, seeks to provide background information about the political, economic, and philosophical contexts from which the documents emerged. For example, he discusses the Marxist goal of ending all forms of exploitation, and the presumed role education can play in coping with the problems produced by China's immense population, the backwardness of its agrarian society, and the lack of suitable communication structures. The author provides a walking acquaintanceship with the red vs. expert, the Mao vs. Liu, or the mass-oriented vs. elite-oriented forms of analysis. The commentary also discusses in terms of the Mao vs. elitest faction struggle, the events of the Cultural Revolution, especially as they affected the administration and direction of the educational system. Seybolt also sketches the changes wrought by Mao in the Chinese educational infrastructure and in the philosophy of education to be followed.

The appendix contains paragraph-length biographies of fifty-odd personalities alluded to in the documents and a useful glossary of basic Chinese Communist jargon.

Professional China-watchers will find this a useful compendium of educational documents, and others in comparative education who are Chinese specialists will find both the introductory commentary and the documents helpful in understanding "Revolutionary Education in China."

ANTHONY W. FERGUSON

Brigham Young University

Vietnam and the West. By Ralph Smith. (Ithaca, N.Y.: Cornell University Press, 1971. Pp. ix, 206. $6.50, cloth; $1.95, paper.)

At this writing, the problems of the United States in Vietnam are far from over, as evidenced by Henry Kissinger's recent meeting in Paris with Le Duc Tho over the breakdown of the Paris Peace Accords and a recent appeal of the American Ambassador to Saigon, Graham A. Martin, for an increased aid package of about one-billion dollars which will include modern sophisticated weapons. After fighting the longest war in U.S. history, at a cost of fifty-thousand lives and billions in national treasure, we find that the question of "what went wrong" in Vietnam continues to elude official Washington and to confound the American public in general. Fortunately, this slender volume provides a partial answer to why the United States has met with so much frustration and failure in Vietnam.

Professor Smith's thesis is that the West in its interaction with Vietnam never fully understood or appreciated the complexity of Vietnamese society and culture. This lack of understanding led the French, and later the Americans, into disastrous policy decisions in Vietnam. Had Western policy makers had a better grasp of society and culture in Indochina, he feels, it is at least possible that the tragic events of post-1945 Vietnamese history might have been avoided.

Smith outlines how the impingement of the French upset the balance between contending forces in Vietnamese society, leading to a breakdown in the Confucian order and a gradual attempt to restructure Vietnam into a modern nation. The dynamics of this process are presented within the context of the major forces shaping Vietnamese society and culture during the last century. The author emphasizes the need to understand the Confucian moral and political structure in Vietnam; the central place of the clan, village, and ancestors in Vietnamese life;

the role of religions, sects, and secret societies in Vietnamese politics; and the struggle for modernization between the Communists and the Western-educated elite in Vietnam. Attention is also given to the contrasts in the philosophical and religious traditions of Vietnam and the West.

Demonstrating an inadequate appreciation for Vietnamese culture, the French established a pattern of colonial governance that alienated large segments of the population. The policies of "assimilation" and "association" had the effect of driving the Vietnamese into secret societies and religious sects, which were traditional organizations of resistance to ineffective central government, while at the same time these policies contributed to the inflexibility of the French response to nationalist demands. In failing to support gradual constitutional reform, the French inadvertently added further to the growth of these subversive organizations in becoming focal points of anti-Western and nationalist sentiment. Unfortunately, the secret societies and religious sects, as well as other political organizations in Vietnam, had a history of division, lacking more than regional unity, and they were unable to coalesce into a nationalist organization strong enough to unite the entire country. This fragmentation of the nationalist movement has, in the author's mind, been one of the most significant factors in the tragedy of modern Vietnam.

That the Americans were probably more ignorant about Vietnamese society and culture than the French is underscored by their steadfast support of Nho Dinh Diem, an ascetic Catholic and a fanatical anti-Communist. Diem's ineffectiveness as a reformer and his ruthlessness toward political opposition clearly contributed to the Communist cause in the South. Rather than hailing Diem's victory over the *Cao-Dai* and the *Hoa-Hao* religious sects, the United States should have been concerned about the Premier's inability to strike a political compromise with his nationalist and anti-Communist opponents. These mistakes in political judgment during and after the Diem regime were the result, according to Smith, of American failure to judge correctly the significance of current facts within an unfamiliar cultural context.

The author concludes with an admonition that the United States must commit a greater share of its resources to the understanding of Asian societies and cultures. Without such an effort, American policy makers will continue to find themselves ill-equipped to deal with problems in Asia, as events in Vietnam have so unequivocably demonstrated. With Washington considering additional technical and military aid to Saigon at a time when government support of academic programs has been curtailed, *Vietnam and the West* deserves careful attention by all who are concerned with American foreign policy in Southeast Asia.

ROBERT L. YOUNGBLOOD
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The Churches and Politics in Germany. By Frederic Spotts. (Middletown, Conn.: Wesleyan University Press, 1973. Pp. 419. $15.00.)

This is the first full-scale study of the political role of the Christian churches in post-World War II Germany (West). The author, a foreign-service officer, has had access to many hitherto unavailable sources—unpublished church papers and governmental reports as well as interviews with key figures in the events and situations described. The result is a book rich in new information that will prove rewarding to anyone concerned with German politics since 1945.

Two background chapters trace the character and organization of German Protestantism and Catholicism. The author shows how the course of the Protestant Reformation led to some of the central features of modern German history: deep religious divisions, the confessional-geographic pattern of society, and the intermingling of religion and politics. The political consequences of the Reformation reinforced the inherent conservatism and patriarchalism of Lutheranism. On the Catholic side, as a result of the small-German solution to the problem of unification, which left German Catholics north of the Alps a permanent minority, there emerged a ghetto mentality and an often obsessive nationalism.

The collapse of the Nazi regime and the aftermath of a disastrous war put organized Christianity in a position of unique authority. Though both churches had largely failed to provide an effective counterforce to Nazism, at war's end they were among the few German institutions that had not been "coordinated" into the Nazi state. Amidst the boundless moral and material rubble of Germany in 1945 they stood as elements of stability and as an important source of values. The major part of Mr. Spotts's book describes how this great authority and influence of the churches in 1945 was gradually undermined and weakened by new forces and attitudes in Germany—the ascendancy of liberal politics and economics, secularism, and the pragmatic politics of a progressively industrialized, technocratic and prosperous consumer society.

As the author shows, the process of change was inaugurated by the mass influx of Germans from the Eastern territories occupied by Poland which put an end to the dominance of a single church in the local community as well as to the de facto "apartheid" of Catholics and Protestants. The way was now open to full social reconciliation between the confessions, and the laity became freer to ignore or reject ecclesiastical guidance. This development was especially significant in Catholic areas; most of the Catholic hierarchy's activities in the political and social sphere during the past twenty-five years can be seen as a vain attempt to maintain its authority over the individual and the community. The outcome of the bitter dispute over the continuation of confessional schools more than anything else demonstrated that the church could no longer control her members; following this defeat the bishops have been unwilling to risk another showdown over any important issue of social policy.

The disparity between the weakening of the churches' political influence and the continuing substantial legal privileges of the churches is striking. To this day they enjoy a favored position in West Germany that is probably unique in the world. The state provides for the education of the clergy in university theology faculties, the state makes annual cash grants, and state and/or local authorities collect a church tax which at times reaches 26 per cent. In addition to what is explicitly provided by law, the churches benefit from certain unwritten rules of German politics which provide for confessional parity in appointments to the civil service and the various bodies dealing with education and social welfare. "Confessionalism," writes Spotts, "is one of the skeletons in the closet of German politics" (p. 205).

In a thoughtful, if brief, conclusion Spotts evaluates the political record of the Christian churches since the end of World War II. The Protestants, he finds, through their synodal meetings, academies, church assemblies, and countless informal meetings at all levels helped achieve and maintain lay interest in public affairs, and these activities became virtual training institutes for self-government. The Evangelical church thus was a major contributor to the success of liberal democracy in Germany. The Catholic church, on the other hand, tried to mobilize the laity into a political army and generally has sought to transform practical politics into a struggle over ultimate, ideological values, an approach incompatible with the democratic requirements of tolerance and compromise. The church aligned herself unqualifiedly with one political party, the CDU, but in the end the tactics employed undermined the strategy. By 1963 the CDU had come largely under the dominance of Protestants; three years later the CDU entered into a coalition with the Social Democrats whom the church had wanted to keep out of power forever. The CDU today, despite the confessional adjective in its title, is the prototype of an Anglo-American mass party. The Catholic bishops, for their part, have largely lost the political loyalty of their flocks. They still refuse to acknowledge that the interests of a secular society may legitimately differ from the morality of the church, but their threats of electoral vengeance can no longer be carried out.

There is much else in this valuable study that cannot be discussed in limited space: The churches' role under the occupation and their attitude toward denazification, the fate of the Reichskonkordat of 1933, the churches' handling of the problems of reunification, rearmament, conscription and nuclear weapons. The author writes well and lucidly. In all, this is an eminently readable book, which for a long time to come will remain the indispensable guide to the role of religion in contemporary German politics.

Guenter Lewy
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Structure and Process in Latin America: Patronage, Clientage, and Power Systems. Edited by Arnold Strickon and Sidney M. Greenfield. (Albuquerque: The University of New Mexico Press, 1972. Pp. 256. $10.00.)

The study of patron-client relations is maturing and developing increasingly useful theoretical models for the analysis of societies. Professors Strickon and Greenfield considerably enlarge the pool of descriptive studies available in this area with their nine-chapter book, the outgrowth of two conferences on the subject held in 1969. The editors open with a brief review of the background to the study of patron-client relationships which is more historiographic than a definition of what patron-client relations are. We should, therefore, be aware that the conceptual framework of patron-client relations addresses itself to exchange relations (1) between two people (dyadic) or networks of dyads, (2) between unequals, (3) between individuals who gain from the relationship (reciprocity), (4) between people over a relatively long period of time, covering a relatively large number of situations (i.e., multiplex, not simplex relations), and (5) between people who are able to interact frequently and personally (i.e., proximity). The introduction explains what variables the authors of the remaining chapters are to examine: "(1) The Ibero-American cultural tradition . . . , (2) the system of formal positions within the institutional system of the nationally organized, complex cultures of Latin America; and (3) the resources available to the specific actors that may be employed by them in the transactional process" (p. 15). The editors conclude that these three variables are to form the basis for analyzing *situations* in which actors relate to each other in pursuit of goals. The book is and was planned to be largely descriptive.

Despite this aim the first substantive chapter, by Professor William T. Stuart, is a discussion of the application of cultural ecology and general systems theory to the study of patron-client systems. After defining and explaining the utility of systems theory in the analysis of clientelism, Professor Stuart concludes that "weakly centralized" societies are most likely to stimulate patron-client relations. He adds " . . . I wish to suggest that most societies or cultures have been and are able to work out more concrete, substantive, and permanent adaptations to environmental challenges than the short-term and precarious resolutions we are calling "patronage" (p. 32). His conclusions are not entirely unprovocative, since Professor Stuart is arguing that these systems are transitional, only to be liquidated by the " 'rational' bureaucratization of the structure . . . " of the state (p. 39). Except in the most idealized and perhaps unattainable sense, one suspects that no society in our empirical universe would qualify; the author mentions that "wardhealers" (p. 40), and ghetto bosses still remain even in the United States. One might, however, raise the possibility that modern, urban men in highly bureaucratized societies could continue to find the personal character of clientelism attractive in expediting their affairs, and thus the setting, the exchange "currency," the actors, and indeed the objectives of clientalist ties could change but the preconditions and the style of these relationships remain. Professor Stuart's vantage point is amplified in his other contribution to the book "On the Nonoccurrence of Patronage in San Miguel de Sena." Here he analyzes a Colombian *municipio*, concludes that patronage-clientage is relatively unimportant in this district, and argues for a limited definition of the concept and its use only in special instances.

Nancie L. Gonzales, in her article "Patron-Client Relationships at the International Level," while focusing on the interactions between an institution in the Dominican Republic known as the Development Association (client) and foreign lending agencies notably AID (patron), also essentially uses changes in the system's ecology as the crucial condition determining the future of clientelism. Professor Gonzales, speaking about patron-client relationships, says, "The system, highly specialized and highly adapted to the expanding industrial complex of the nineteenth and early twentieth centuries, may prove to retard future development in countries such as the Dominican Republic" (p. 206). This article goes beyond the conventional use of the framework of clientelism by studying cross-national ties. In doing so, patron-client analysis emerges as a potentially crucial concomitant to "dependency theory" which has become so valuable a perspective in international politics studies.

Turning to more familiar ground we find several chapters which deal with microlevel cases of clientage such as Sidney Greenfield's "Charwomen, Cesspools, and Road Building: An Examination of Patronage, Clientage and Political Power in Southeastern Minas Gerais." This piece is an intriguing and often amusing description of the lace-like complexities of pay-offs, the competition and often bitter political struggles for the preferments generated by clientelism.

Arnold Strickon's article "Carlos Felipe: Kinsman, Patron, and Friend," discusses the intricate behavior of a patron as he attempts to manipulate those people upon whom he is dependent and who at the same time are subordinate to him. One of the most significant observations Strickon makes in his study of this Argentine case is that one of the consequences of patron-client relationships (providing links across class lines and minimizing lower-class solidarity) has nothing to do with the motivations of the actors themselves. Thus, in the case he describes, Carlos Felipe became a patron largely to ensure himself a dependable labor force.

Dwight B. Heath's chapter, "New Patrons for Old: Changing Patron-Client Relationships in the Bolivian Yungas," discusses clientelist networks in various specific fields of action and stresses changes in these networks over time. Heath points out that the impact of the Bolivian revolution on forms of social structure is significant for understanding patron-client relations. He argues that traditional clientelism and " . . . a new patron-client type of functional relationship . . . " (p. 133) centering primarily around the *sindicato* (syndical organizations organized by the MNR Party) coexist and that often clients consciously choose to retain the old patron-client relationship. His conclusion is that clientelism most often is role-specific and that one must be careful to look at "crucial individuals" as well as "various statuses" (p. 133) in analyzing patronage/brokerage. Professor Heath cautions the reader by saying, " . . . many relationships that appear, in structural terms, to be impersonal and universalistic are, in functional terms, personalistic relationships of patronage and clientage," (p. 134) thus, apparently siding with those scholars who are arguing for using the patron-client model generously in analysis rather than restricting it to classic situations.

The application of patron-client analysis in an unconventional way is found in Chapter 7 "Ponchos, Weaving, and Patron-Client Relations in Northwest Argentina," by Esther Hermitte. Ms. Hermitte describes the clientelist links in the town of Huarco involving women weavers. Her findings suggest most basically that women placed in the strategic position of having a valuable resource (weaving) can become desirable clients. They also indicate that a client may have more than one patron to fulfill her existing and potential future expectations, a finding which considerably amplifies the more usual indication that a *patron* has more than one client.

In a different vein and addressed to a more general level of the system we find Robert W. Shirley's article "Patronage and Cooperation: An Analysis from São Paulo State" which is an interesting discussion of the conflict between traditional clientelist politics in Brazil and the obstacles it presents to the development and efficient functioning of that country's cooperative movement part of which has been seized by power holders and part of which may apparently be operating under a distinct disadvantage vis-à-vis the loosely structured, clientelist networks of private commercial firms.

I have briefly reviewed the content and conclusions of all the articles because in doing so one can appreciate the range of cases and the varieties of situations which have been studied. Thus it becomes clear that the book represents a major contribution to the growing collection of empirical material available on the subject. Its great shortcoming, however, is that neither the individual contributors nor the editors make an effort to construct comparative insights into the study of patron-client relations. This omission is perhaps in part a consequence of the exclusion from the volume of material by political scientists and other scholars representing diverse disciplines. While anthropology has pioneered in this type of analysis, the range of interest today includes all of the social sciences.

Another weakness of the book is the underrepresentation of urban life in the discussion, a fact which again reflects the tradition of studying patron-client ties in villages, small towns, and rural areas. This omission is especially crucial in light of the assumptions put forth by some that clientelism is a traditional form of interaction, subject to change or disappearance with perhaps the achievement of some optimal level of "development." It is also unclear what effect, if any, the different levels of development characterizing various Latin American countries (comparing Argentina with Bolivia, for instance) might have on the understanding of patron-client bonds. Still another crucial question requiring attention is, "If one finds political clientelism used in a certain social setting to expedite matters, are *all* people tied into networks? If not, what sorts of people and situations are non-clientelist; why and with what consequences?" Moreover, since one of the variables which the volume was to examine consisted of the "Ibero-American cultural traditions" as it bears on the exchange relations under study, one must ask what, if any, conclusions were reached regarding the role of this and how Ibero-American culture would differ in this respect from, let's say, Southeast Asia, which James Scott and Carl Landé have been so intensively studying.

Thus while the book is indeed valuable to Latin Americanists and to scholars working on clientelism, it fails to address itself except in a tangential or accidental way to some of the theoretical questions which political sicentists are eager to begin answering.

STEFFEN SCHMIDT
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Book Publishing in the USSR: Reports of the Delegations of U.S. Book Publishers Visiting the USSR, October 21–November 4, 1970, August 20–September 17, 1962, Second edition, enlarged. (Cambridge, Mass.: Harvard University Press, 1971. Pp. 182. $4.95, paper.)

These reports, prepared by a small group of American publishers following their visits to the Soviet Union in 1962 and 1970, make available to the English-speaking public a limited body of data on Soviet publishing; sixteen sections cover such subjects as the organization of publishing houses, production, marketing, royalties, and foreign translations. Unfortunately, however, the volume gives a rather formal, superficial picture based almost entirely on impressions and Soviet responses during official visits, augmented by some statistical tables compiled from Soviet publications. Most notably, as the delegation themselves were well aware, the crucial subjects of political-ideological control and of the mechanism of censorship were carefully avoided, and the authors were unable to delve into these matters. The section of each report devoted to copyright (pp. 35–38, 168–75) is of particular interest in indicating the evolution of Soviet official thinking and dialogue with Western publishers on the subject, and hence gives some of the background for the recent adherence of the Soviet Union to the international copyright convention; but again, the censorship and ideological dimensions of this issue are overlooked. For reference and readability, it would have been much more satisfactory had the new material from the 1970 visit been integrated with the earlier report. As it is, the 1962 report (previously published in 1963) is merely reprinted (pp. 61–176) unrevised with the separate 1970 report (pp. 1–59) serving to indicate changes and corrections.

PATRICIA K. GRIMSTED
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The Private Sector in Soviet Agriculture. By Karl-Eugen Wädekin. 2nd revised edition of *Privatproduzenten in der sowjetischen Landwirtschaft.* Edited by G. F. Karcz. Translated by K. Bush. (Berkeley: University of California Press, 1973. Pp. 432. $17.50.)

No one concerned with understanding the nature of the contemporary Soviet system can afford to miss this book. This is, first, because Professor

Wädekin's method is a model of genuine cross-disciplinary research. The ideological, legal, political, and economic foundations of private agricultural enterprise in the Soviet Union are treated with impressive thoroughness and analytical sureness, and each aspect is expertly woven with the others into an integrated portrait of this "dark corner" of the Soviet system. Second, the illumination of the private sector in Soviet agriculture affords new perspectives and insights that have implications that extend well beyond the agricultural domain. Third, Wädekin demonstrates, by example, the continuing necessity for, and the scholarly productivity of, painstaking, empirical research for expanding our understanding of Soviet affairs. In order to overcome the paucity of reliable data in readily accessible sources, Wädekin has examined virtually every conceivable source, from contemporary Soviet literature to official statistical handbooks, and he has put these disparate fragments together into an intellectually meaningful and highly readable form. The late George F. Karcz, whose encouragement and assistance are acknowledged as instrumental in the appearance of this edition, and Keith Bush, the very able translator, also merit thanks and congratulations for a job well done.

As Wädekin repeatedly and quite correctly stresses, the private agricultural sector is not merely an historical remnant appended to the public (socialized) sector. It is rather an integral and vital component of the agricultural sector. Although there are areas in which private and public agriculture compete for resources and markets, these two sectors are mutually dependent, and cooperation is therefore essential to the satisfactory performance of either. There is, in fact, a symbiotic relationship between private and public agricultural enterprise, and the exchange of both intermediate and final products is extensive. Specialization is also highly visible in deliveries to the state and to the kolkhoz market. Thus, a certain division of labor has evolved between private and public agricultural sectors that permeates the production, marketing, and consumption of agricultural products. The degree of interdependence is such—and this is one of Wädekin's main points—that any attempt to constrict the private sector necessarily entails the impairment of public argicultural (and distributional) performance as well.

The division of labor that currently exists comprises the following salient features. In the first place, a large proportion of private agricultural output is produced for own consumption, which both reduces the burden on the state distribution system (especially as many of these products are highly perishable and/or expensive to transport) and affords a higher standard of consumption than would otherwise be possible for the population at large. Second, the share of private output that is sold on the kolkhoz market by the producers themselves (as opposed to consumer cooperatives) serves the same ends. Third, private agriculture draws mainly upon manpower sources that it would be difficult if not impossible for state agricultural enterprise to tap, and it utilizes land resources that, to a considerable extent, are not amenable to large-scale mechanized farming. Fourth, private plot farms, and particularly rural household plots, exchange inputs with public farms to mutual benefit, e.g., animal husbandry products against fodder. Finally, household access to private plots and the consciousness of mutual dependence remain powerful incentives to participate in the public agricultural sector.

Although not stressed quite as much by Wädekin as the interdependence of public and private agriculture, the pervasive influence of the private sector in the Soviet economy is equally important and well-documented. According to Wädekin's careful estimates, some fifty million Soviet households—including a growing fraction of both rural and urban nonagricultural workers and employees—participate directly in private small-scale agricultural production. The greater part of the output, composed primarily of livestock products, potatoes, fine vegetables and fruit, is consumed on the private farm. But a nontrivial proportion is also marketed either in extra- or intrarural trade. Thus, in one way or another, virtually every household in the Soviet Union comes in daily contact with the private agricultural sector.

The existence and economic state of the private agricultural sector are of great consequence, therefore, in the planning and regulation of many aspects of the Soviet economy. It is true that the output of the private sector has been shrinking relatively during the last two decades, thanks to the growth of public farm output, but the absolute volume of private output has remained constant. Given the growing sophistication of consumer demand for agricultural products in the Soviet Union and the comparative advantage of the private sector in the production of the goods required to satisfy it, Wädekin is certainly correct in concluding that private agricultural enterprise is likely to remain an economic necessity for the foreseeable future.

Hence the ambivalence of Soviet leadership toward this sector and the kolkhoz market, which Wädekin documents thoroughly. Private enterprise is ideologically anathema, and the pervasiveness of its unwelcome influence has evidently been a source of concern to the leadership. Moreover, its performance stands as a nagging reminder of the incompleteness of socialization of the means of production and trade and of the lagging performance of the socialized agricultural sector as well. Nonetheless, as the unhappy consequences of Khrushchev's attempt to restrict the private sector by administrative measures (reductions in plot size and in allowable livestock holdings) show, increasing the quantity and quality of agricultural products requires fostering *both* the private and the public sectors at once.

Wädekin devotes three full chapters, plus an addendum, to an examination of official policy toward the private sector in Soviet agriculture between 1953 and 1971. These chapters are among the best in the book, and they are interesting not only for what they reveal about the making and implementation of agricultural policy, but also for what they reveal about the character of Soviet politics generally during the post-Stalin period. Brezhnev and Kosygin, of course, immediately reversed the restrictions Khrushchev had imposed on the private sector during the last five or six years of his power. This was, naturally, a popular policy, but it was almost certainly introduced in the hope of reviving both public and private agricultural performance, which implies that the current leadership appreciates the fact of interdependence. Even so, the private agricultural sector and the kolkhoz market are certainly anomalies in the Soviet system and a constant source of irritation on ideological and practical grounds. Thus, whether or not Brezhnev will be moved eventually, as was Khrushchev, to seek prematurely and thus administratively to restrict the private sector remains an open question.

This is a very important book, and it will doubtless remain the standard reference work on the private sector in Soviet agriculture for many years to come. Moreover, it contains both explicit and implicit suggestions for additional research on the private sector of the Soviet economy and its points of tangency with the public sector, and I hope that others will follow Wädekin's lead in exploring these questions.

James R. Millar

University of Illinois

The Fourth World: Victims of Group Oppression. Edited by Ben Whitaker. (London: Sidgwick and Jackson Limited, 1972; and New York: Schocken Books, 1973. Pp. 342. $10.00.)

This book does not lack worthy and ambitious intentions. A publication sponsored by the Minority Rights Groups (M.R.G.), an independent and international nongovernmental body headquartered in Britain, it aims to develop on "international conscience" with regard to minorities' treatment, to increase knowledge about the factors creating prejudice and intergroup hostility and, generally, "to help the position of persecuted or disadvantaged ethnic, religious or cultural minorities (or majorities)" (p. 9). But these lofty aims are to a large extent subverted by the way in which the book is organized.

Eight reports on minority rights commissioned by the M.R.G. constitute the core of the volume. These reports examine the position of the Asians in East and Central Africa, the Blacks in the Sudan, the Moslem Eritreans in Ethiopia, the African majority in Rhodesia, the Blacks in Brazil, the Catholic minority in Northern Ireland and the Protestant minority in the Republic of Ireland, the adherents of the Russian Orthodox, the Roman Catholic, the Protestant, the Jewish, the Moslem and the Buddhist religions in the Soviet Union, such nationalities as the Crimean Tatars and Volga Germans in the Soviet Union, and the outcastes of Japan. Most of the studies are competently presented. Special mention should perhaps be made of Anani Dzidzienyo's investigation of the Blacks' place in Brazil and George DeVos's piece on Japan's outcastes. Little, however, in the way of new information or interpretation is provided in any of these case studies; indeed half of these are really only updated summaries of more extensive treatments available elsewhere.

But more frustrating is the lack of any integrating framework or concepts. The individual cases are not presented according to any single scheme, though most authors adopted a historical-descriptive approach. Worse still, the reader is never informed what the various oppressed racial, ethnic, national, religious and caste groups studied have in common, aside from their oppression. How, for instance, is the Asian "problem" in East Africa similar to the position of Blacks in Brazilian society or of Catholics in the Soviet Union? Since no one provides an introductory statement suggesting how the various studies interrelate, the reader is simply left to flounder in a welter of disparate empirical detail. The only piece to offer a general perspective—a four-page "Afterword" entitled "Who's to Blame?" by Richard and Hephzibah Hauser—is not only nugatory in implying that scapegoating is *the* cause of group oppression, but is also confusing in failing to refer to any of the preceding studies.

One is bound to conclude, therefore, that the good intentions of the editor and the Minority Rights Group were aborted by the organization, or lack of organization, of the book. This failure to come to grips with the generic features of oppression is perhaps explicable in terms of an assumption which seems to underlie the M.R.G.'s approach—that a dominant majority (or minority), once it is awakened to its own shortcomings and oppressive conduct by scholarly treatises and "informed world opinion," will relent and reform. As the editor rather naively remarks (on p. 10), the

M.R.G. is "concerned with the problems [of minority rights], not the politics." But any person or group who wishes to change the world cannot avoid a concern with politics, especially with the structure of power and vested interests. The facts of oppression in themselves do not provide an explanatory framework or guidelines for action; these facts do not speak for themselves.

RICHARD SANDBROOK

University of Toronto

Weapons Technology and Arms Control. By W. F. Biddle. (New York: Praeger Publishers, 1972. Pp. 355. $20.00.)

The author, described by his publishers as "a nuclear scientist in British government service," has, while falling short of his apparent aspiration, nevertheless written a useful book. His professed aim (pp. 3f.) was to attempt "to bridge the gap that exists between the scientists and technologists who are involved in the design, procurement, and use of weapons, and the diplomats and other experts who are responsible for designing and negotiating agreements." He goes on to say that although many other studies have attempted to examine the effects of new weapons on international relations, "as far as is known this is the first time that a comprehensive study has been attempted."

Well, the book lacks much in comprehensiveness, and it is certainly stronger on the technological side than it is on the political or diplomatic. One senses its weakness in the latter area with the first substantial quotation in the book, which opens chapter 2. The *aims* of arms control the author believes to be quite adequately stated in a passage by Herman Kahn and Anthony Weiner which reads as follows:

> [the aims are] . . . to improve the inherent stability of the situation, decrease the occasions or the approximate causes of war within the system, and decrease the destructiveness and other disutilities of any wars that actually occur. One may also add to this last, "decrease the cost of defense preparation," but we would argue that this would take a rather low priority to the first three objectives. (quoted on p. 9)

Bose, p. 42:

Inasmuch as political scientists have not been very good about re-examining the aims of that arms control to which many of them devote so much attention, one can hardly censure members of the physical science guilds for taking it for granted that the appropriate aims are always in the direction of saving the world. I would on the contrary argue that saving money is usually the most feasible goal and one never to be despised. Certainly the SALT accomplishments thus far have been in the direction of saving money—the only goal that both sides had fervently in common—and no one should disparage those accomplishments on that account. At any rate, the issue here can hardly be dispatched with a sentence or two, quoted or otherwise.

The utility of the book lies in its being simply a layman's guide to some of the military technologies that arms control is all about. It is devoted overwhelmingly to nuclear weapons of all kinds and their associated delivery (including defensive ABM) systems. There is also, however, some modest discussion of conventional weapons and a chapter devoted to "Chemical and Biological Warfare."

One notices a few minor errors. For example, it is not true (p. 179) that the difference between the Sentinel and the Safeguard ABM systems was that the former "was designed as a thin system" and that the latter was designed as a ICBM defense force. The earlier system was called "Sentinel" long before Secretary McNamara made his half-hearted proposal of September 1967 to deploy it thinly against China only, and Safeguard was exactly the same system with a different name adopted for political reasons by President Nixon when he wanted to emphasize a change in the aim. This and similar errors, however, are of no importance to the political scientist who is interested only in getting a better understanding of military technologies, especially those of nuclear warfare.

The price of the book will frighten away all but the most devoted, and along with the dull style will make it likely that the book will be used as a reference book on library shelves rather than as something that one reads straight through.

BERNARD BRODIE

University of California, Los Angeles

The Superpowers and the Middle East. By Tarun Chandra Bose. (New York: Asia Publishing House, 1972. Pp. 208. $6.50.)

This work is an excellent illustration of how *not* to write a book. It contains so many elements proscribed by style manuals that it probably would not have been passed by a Ph.D. committee in any reputable university. It is all the more surprising, therefore, to find that the volume is not the work of a beginner but of a seasoned professional, a Senior Reader in International Relations at Jadavpur University, who undertook "research for the major portion of this book at Harvard University and at the John F. Kennedy Library" as a Fulbright-Hays Senior Professor (the jacket).

The stylistic errors include occasional misspellings as well as other liberties with the English language which should have been detected by professional editors. There is rampant sloppiness and inconsistency, as, for instance, in the use of capitalization. Fedorenko is Federenko throughout. Mahmud Riyad (pp. 93, 207) sometimes becomes Mahmoud Riad (pp. 134, 145). Whereas on pp. 27 and 34 Nasser is said to have nationalized the Suez Canal, on pp. 32 and 43 it is the Suez Canal Company, leaving the uninformed reader wondering which is correct and why the distinction was made in the first place.

Even more serious is the author's apparent difficulty in distinguishing between partial quotations and paraphrases. A check of only two sources yielded at least four instances in which Bose's language is uncomfortably close to that of the author cited. For example:

> Bose, p. 42:
> Soviet action during the Suez crisis, as a matter of fact, made a very strong impression in the Arab world where events in Hungary passed practically unnoticed. Nasser officially thanked the Soviet people and government stating that the Soviet help "reflected Soviet-Egyptian friendship."

> Laqueur, *The Soviet Union and the Middle East*, p. 239:
> . . the Soviet action made a very favorable impression in the Arab world, where the events in Hungary passed unnoticed. . . . Gamal Abdel Nasser, in a talk with Soviet journalists, officially thanked the Soviet people and government: "The help that we received reflected Soviet-Egyptian friendship, . . ."

In addition to stylistic shortcomings, the book contains numerous factual errors. The assertion that Stalin was trying to "subvert" Greece (p. 3) has been refuted by Milovan Djilas. The use of Soviet "volunteers" was threatened not during the Suez war (p. 37) but on November 11, five days after the end of hostilities. The statement that "practically everybody in the Arab world believed" that Moscow's threats had achieved the cease-fire in Egypt (p. 42) cannot include Nasser who, in a speech of March 22, 1959, accused Khrushchev of indecision in Cairo's hour of crisis. Kassem was a colonel, not a general, in 1958 (p. 52).

The narrative is further weakened by serious omissions and highly questionable interpretations. In the first category, to list only the most blatant examples, the discussion of Khrushchev's diplomacy during the Lebanese crisis (pp. 53–54), of the implications of the 1959 Cairo-Moscow crisis (pp. 75–76), and of the Premier's 1964 visit to Egypt (p. 76), is, at best, scanty. The author also displays a lack of awareness of the connection between the deployment of Soviet naval vessels in the Mediterranean and the permanent stationing there of *Polaris* submarines.

Equally disturbing, in terms of interpretation, is the author's inability to get away from the stereotyped clichés about the nature of US-Soviet interaction in the Middle East. Thus, the Baghdad Pact is presented as a bulwark against possible Soviet aggression, while the serious threat to Soviet security posed by the US Sixth Fleet is disregarded. Similarly, the presence of US air and missile bases in the Mediterranean countries is treated in a cavalier fashion: only in one instance (p. 82) does the author admit that "the establishment of military bases by the West in . . . [Iran and Turkey—but not elsewhere] made Soviet 'southern belly' *somewhat* vulnerable" [Italics added]. Similarly incomprehensible are the references to Middle East oil as being "no longer . . . of crucial importance to Western security" (p. 123) or to the Soviet Mediterranean squadron acquiring "a tremendous position of power at both ends" of that sea (p. 116, n. 79).

In summary, the book shows no evidence of thoroughness in the study of available sources, and it contains no new insights or information. After a vain search for some redeeming features, I am driven to the inevitable conclusion that both the author and the publisher have contributed to further "pollution" of a market which abounds in second-rate studies of a complex and, in some respects, still relatively unexplored subject.

O. M. Smolansky

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The Retreat of American Power. By Henry Brandon. (New York: Doubleday & Co., 1973. Pp. 368. $6.95.)

This contribution to the nascent literature on Nixon foreign policy made by Henry Brandon, the chief American correspondent of the London *Times*, has the material strength of sound information; gains firmness from intelligent selection by a seasoned observer; and is characterized by the journalist's flair for reporting stories with an eye for personalities, detail, and color. The central theme of the nature of American involvement in the world and the secondary theme of the new character of American policy are allowed to emerge from the discussion rather than be formed through systematic, closely focused and sustained argument. Brandon forces attention to the vexing question of whether America has the capacity or will for leadership in a multipolar world. A serious thematic flaw is that since the author almost exclusively focuses attention on the difficulties created by the phenomenon of the diminution of American involvement, he does not adequately develop the Nixon global strategy's positive thrust toward a new structure of international politics.

Brandon describes and comments upon the personalities and understandings of Kissinger and Nixon; decision-making and crisis management processes; the episodes of the Cambodian incursion, the Hussein-Fedayeen conflict, the India-

Pakistan war, and negotiations over devaluing the dollar; the sequence of steps toward detente with Russia and China; and the broad areas of European, Middle-Eastern, Vietnamese, and defense policies. He concludes that the systematic Kissinger National Security Council analyses are superior to those which emerged from the lunch meetings of the Kennedy and Johnson years. His outline of official thinking on Vietnam is succinct and convincing. While his commentaries are not always complete, in all cases a journalist's method joins with care, accuracy, and solid data.

The thematic title, the retreat of American power, refers not only to modification of American international involvement, but also to diminished American power, daunted American will, and accompanying trauma. On the policy level, the word retreat indicates a shift to "safer and more solid positions" (p. 81) and a movement from dominance to mere leadership. The author believes that this transition and the new practice of unsentimental multipolar balance of power politics will be very difficult for the United States.

Brandon's information gives additional credence and specificity to certain conclusions or observations of the type which only can be deduced or inferred from public statements. For example, with respect to buying time as a policy goal, he reports that Kissinger "believed that an interval of about two or three years was necessary to shield the United States from events which could occur between the departure of the American troops and the collapse of the South Vietnamese regime" (p. 62). Another example, illustrating the inferrable Nixon-Kissinger sense of molding policy with attention to evolving conditions, is his report that Kissinger observed that if "the North Vietnamese would take the same long term view of their future relationship with Saigon as the Chinese do in regard to Taiwan, there would be no problem of finding a settlement" (p. 193).

The rationale of the Nixon foreign policy has been well articulated in the President's annual reports to Congress (which should remain the point of departure for the serious student). A "new era" presents opportunities as well as limitations. New power and interest configurations and the practice of Realist diplomacy and accommodation could constitute a stable structure of international relations. Brandon does not explore this argument.

Among the deficiencies which follow directly from not doing so is Brandon's inability quickly to see the place of the initiation of the dialogue with Peking. Brandon says that the Peking visit announcement "left one wondering how this latest move fit into a coherent picture of his world outlook" (p. 181). In fact, the place of accommodation with China, the importance of initiating a dialogue, and the nature of the steps to be taken had been indicated in the annual reports to Congress.

Another deficiency is that while Brandon is concerned about whether anyone else will be able to assume management of the "intricate weave of the Nixon-Kissinger diplomacy" (pp. 358–359), he does not focus on that which sustains it. He does not systematically articulate the base upon which successful management or manipulation, or the grander goal of restructuring international relations, ultimately rests: that understanding of the application of principles of international politics which, in fact, has given coherence and effectiveness to the Nixon-Kissinger foreign policy.

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The Peace Conspiracy: Wang Ching-wei and the China War, 1937–1941. By Gerald E. Bunker. (Cambridge, Mass.: Harvard University Press, 1972. Pp. 327. $13.50.)

In this important study, Gerald Bunker unravels the tangled attempts of a small number of Chinese and Japanese to bring the Sino-Japanese War to a negotiated end, and shows how this process involved the transformation of Wang Ching-wei from a nationalist leader to a despised traitor. Wang advocated peace from the very beginning of the war, but at first it was only one weapon in the Kuomintang's arsenal of defensive maneuvers, the counterpart of Chiang Kai-shek's advocacy of armed resistance; the Kuomintang used one or the other, depending upon the immediate situation. Only later, after the Japanese had declared their refusal to deal with Chiang under any circumstances, did Wang conceive the notion of realizing peace through the establishment of a rival Kuomintang government which would exact maximum concessions from Japan, retain maximum rights for the Chinese, and in that way avoid the utter destruction of Chinese civilization which Wang thought total Japanese military victory would certainly bring.

Wang failed because his assumptions were false. He believed that the Japanese really wanted to cooperate with a stable, independent China, a belief that completely misjudged the extent of Japanese appetites. In a sense, Wang was mistaken to think that he could deal effectively with the Japanese in any way; Bunker shows that the Japanese government and military were so divided by factionalism and rivalries that it was difficult for the Chinese to find anyone who could declare a policy that somebody else would not promptly repudiate. Wang's crucial assumption was that Japan would inevitably defeat China if the matter were left to the battlefield. That premise might have proved correct if events at Pearl Harbor had not suddenly altered the situation by bringing the United States into the struggle. From that time, the ultimate defeat of Japan seemed certain, and resistance was the only feasible goal for nationalistic Chinese. Whatever logic Wang's actions originally may have had disappeared.

How could a man of Wang Ching-wei's experience think he could conciliate the Japanese by himself, and persuade them to make concessions sufficient to make their presence acceptable to Chinese nationalists? Bunker answers this question in an intelligent fashion, but he is not completely persuasive. He acknowledges that Wang's policy was one of "make-believe" (p. 205), but he emphasizes the "nobility of Wang's desire for peace and the sincerity of his devotion to Chinese interests" (p. 257). Generally, Bunker views the participants in the peace movement as men of good intentions who sought genuinely nationalistic goals in impossible circumstances; they failed, but their motives were pure. But can either nobility or sincerity explain Wang's attempting to persuade Chiang Kai-shek to sue for peace by lying to him about Japan's readiness to make concessions? Can they explain the persistence of Wang and his associates when repeated experiences proved that the Japanese would provide them with not a tittle of the substance of power, and little enough even of the trappings? At one point, Bunker himself is forced to explain their persistence in terms of their being "unemployed politicians" who wanted "to play the game of politics that they loved on the great stage" (p. 215).

Bunker provides much new detail about the people and events involved in the peace movement from its beginnings in vague speculation to its tragic end. It is a complicated story, and the general reader may not appreciate Bunker's extensive use of transliterated words and phrases and his inclusion of the names of many secondary characters who appear only fleetingly. Bunker's careful research, however, not only answers many questions about Wang Ching-wei, the nature of Japanese factions, and many other important aspects of Chinese and Japanese history, but also indirectly illuminates perenially significant questions about the ways in which politicians deceive themselves and ignore realities, and about the moral uncertainties which inevitably confront men as they try to cope with war and revolution.

JAMES E. SHERIDAN

Northwestern University

America and the World Political Economy. By David P. Calleo and Benjamin M. Rowland. (Bloomington: Indiana University Press, 1973. Pp. xii, 371. $12.50, cloth; $2.95, paper.)

America and the World Political Economy could not have been published at a more opportune time. Even a cursory glance at the front pages of any newspaper reveals that as some of the more pressing conflicts of the Cold War are receding from our attention, they are being replaced by new international problems that have an increasingly visible economic component. The symptoms of mounting difficulties are manifest: taxes imposed by Washington to discourage American investment abroad, repeated devaluations of the dollar, collapse of the Bretton Woods monetary system, worldwide inflation, "gold fever," questions about the domestic and international role of ITT and other multinational corporations, shortages of foods that were recently a glut on the market, the "energy crisis"—to say nothing of veiled and sometimes not-so-veiled threats and charges of economic warfare emanating from officials in Washington, Tokyo, Paris, and other major world capitals. The present international economic difficulties and their intellectual roots are the subject of this timely book.

In a brief introductory chapter, Professors Calleo and Rowland describe the American vision of a world economic order as an imperial one, but of a far different sort from that attributed to Washington by some other critics of American policy. The American vision is pernicious in ideology and rhetoric, but it has often been benign or even generous (e.g., the Marshall Plan) in practice. Part II is addressed to the intellectual roots of the Atlantic Community, with special attention to three contributing schools of theory: free trade (liberal economic theorists, Cordell Hull), geopolitics (Mahan, Acheson, Dulles), and federalism (Madison, Clarence Streit). The economic and political sources of contention within the Atlantic Community are discussed in Part III, with chapters devoted to the monetary system, trade, and international corporations. Although the main focus of the book is on the Atlantic Community, in Part IV Calleo and Rowland turn their attention to other areas, especially Japan and the Third World. In a brief conclusion the authors make clear that they are not opposed to a liberal world economic system, but only to the rigid version that has dominated official American thinking at least since the first FDR administration, if not that of Woodrow Wilson.

Underlying the analysis of the present economic crisis in the Atlantic Community are several themes:

(1) The difficulties that beset American policy can be traced back to failures of intellect and imagination—"superannuated and rotting idols that still enchant the American mind"—rather than to inherent flaws in liberal-democratic institutions. At the core of the problem is an ideology that fails to distinguish between liberalism and antinationalism, federalism and hegemony, and interdependence and dependence. The result is a

specious imperialism masquerading under the cloak of internationalism.

(2) The political economy is too important to be left to economic theorists, who often fail to appreciate Edmund Burke's distinction between a nation and a commercial contract. Political theory has tempered the classic laissez-faire system at home; it must, according to Calleo and Rowland, perform a similar role with the real or potential excesses of liberal free-trade doctrine.

(3) Above all, the integrity of the nation-state must be preserved. The liberal-democratic nation-state—the "highest form of rational organization man has yet developed, or will develop in the foreseeable future" (p. 253)—is the only alternative to chaos. The Calleo-Rowland critique of free trade ideology is not so much that it is bad economics, or that it doesn't "work." It is, rather, that free trade may threaten the ability of the nation-state to maintain rational control over the domestic economic environment—for example, when one nation uses trade as a means of exporting its domestic problems and inbalances to others. Calleo and Rowland accept neither the internationalist theory that the nation-state is an obstacle to peace, nor the functionalist thesis that economic interdependence might contribute to peace by diluting some of the effects of political rivalries. Indeed, they assert that the planned, democratic welfare state (which is inevitably mercantilist) is the greatest hope for peace in today's world.

The strengths of the book are many, not the least of which is its felicitous style. The work is well documented, although the placement of many tables at the end of chapters or in the back of the book does not add to their utility. By tracing the intellectual roots of present problems from the classical liberal economists through the British imperial experience, Wilson, Hull, and the Cold War era, Calleo and Rowland have achieved an impressive perspective on the problem.

The prescriptions set out in *America and the World Political Economy* will not please everyone. The authors assert that, just as the developed nations of the Atlantic Community must learn to absorb their own production, so must nations and regions of the Third World. This may be rather cold comfort for nations with one-crop or one-commodity economies. Aside from possible economic difficulties this solution might encounter, one might question whether its acceptance will be politically feasible for leaders of Third World nations. Second, Calleo and Rowland are unquestionably correct when they assert that many areas of American life (e.g., urban transportation, housing) have been neglected. It may be less evident that replacing market mechanisms with a political reordering of resource allocation will prove to be a wholly effective alternative. Finally, the authors end with a declaration that, "The age of the minimal state should now be over." The assertion may be true, but in the post-Watergate era perhaps one can look upon this tendency as a mixed blessing.

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Fundamental Guarantees of the Parties in Civil Litigation: Studies in National, International and Comparative Law. Edited by Mauro Cappelletti and Denis Tallon. (N.Y.: Oceana Publications, 1973. Pp. 821. $25.00.)

Increasingly, American political scientists have expressed interest in moving the research on public policy and decisional behavior beyond the parochialism of national study and into a comparative framework. But the obstacles facing any such project are great. With the way cleared years ago by such comparative institutionalists as Rudolph von Ihering and Edward McWhinney, forays into comparative judicial behavior have successfully been made lately by Theodore Becker, David Danelski, Glendon Schubert, and others. But comparative public policy largely remains *terra incognita*, and where guiding theory is concerned, we have neither chart nor compass. We stand in the prescientific stage of requiring case studies. There are not many, and those who produce them—legal scholars in the field of comparative law—are largely strangers to political scientists. Their categories of analysis are different from those used by social scientists and, as a literature search through the *Index of Foreign Legal Periodicials* readily shows, they often write in French, the language of international law.

To the specialist interested in the newly developing field of comparative public policy, procedure, and behavior, a very useful beginning with the staples of basic information can be made with the volume edited by Mauro Cappelletti and Denis Tallon. Prepared at the request of UNESCO, it reflects the papers, proceedings and discussion of the Florence Conference of the International Association of Legal Science (1971). The lengthy and awkward title of the book suits the two-inch thickness of the weighty volume. But it should not deflect from the vitality and sharp focus of the work. The constitutional rights of litigants in civil proceedings have never before been the subject of systematic study on a worldwide comparative level. National and regional reports are presented in practical and in some instances "realist" terms, not merely doctrinal terms. National reports were sought from quite varied political systems: economically developed liberal democracies, people's democracies, and third world systems. The descriptive quality of these reports is generally high, though the surveys of Danish and Senegalese developments are sadly lacking in documentation. Fundamental guarantees for civil proceedings in most of the essays are separated into (a) guarantees concerning the judiciary, e.g., independence and impartiality; (b) guarantees concerning the parties proper, such as their right of action and defense, their right of effective access to courts, their right to counsel and legal aid; and finally (c) guarantees concerning the unfolding of the proceeding and proof-taking, e.g., adequate notice, admissibility or exclusion of illegally obtained evidence, publicity and orality of the hearing, and the written form of the decision including their grounds.

Sixteen reports are published, nine of which are in French. The balance are in English, as are the introductory comments, the editor's conclusions, and the very interesting and lively summaries of discussion. Twelve of the essays concern single national systems: Austria, Canada, Denmark, England, France, Germany, Italy, Japan, Senegal, Switzerland, the Soviet Union, and the United States. A particularly detailed analysis of rights associated with civil proceedings under the European Convention of Human Rights is offered by Jacques Velu of the University of Brussels. Zhivko Stalev of the University of Sofia has written a survey of the law of the European People's Democracies offering details of the East European judicial process nowhere else available in English and including some fragmentary serial data on the speediness of judicial proceedings. Two lengthy essays are concerned with Latin American systems. That by Héctor Fix-Zamudio is the more profound, and it is notable for achieving sophistication at the level of Latin American intersystem comparisons. Fix-Zamudio analyzes the gradual modernization of civil procedure transformed from one country to another in terms of the progress of traditional systems of formalistic justice into modernized, egalitarian, and responsive processes.

Several of the essays should be instructive to political scientists in emphasizing that judicial behavior is by no means the only relevant decisional behavior of importance in policy fields related to rights and liberties. If "justice," as Prof. Stalev of Yugoslavia commented, is "the solution of legal disputes with *res judicata* effect, using a procedure granting specific guarantees to the parties, then all bodies conducting such activities can be called 'justice-making bodies' " (page 783). Systematic comparative study of decisional behavior and policy among boards, commissions, tribunals and other parajudicial "justice-making bodies" has yet to be initiated by political scientists, though it is familiar territory for students of comparative law as illustrated by the Cappelletti and Tallon volume, especially the essay on Canada by Garry Watson of York University.

The concluding essay by Mauro Cappelletti, though by no means a summary of the various national reports, offers some effective comparative analysis of litigation processes among systems worldwide, and convincingly restates a theme problem which clearly absorbs the interest of all the contributors, viz.: how to make justice accessible to all. He writes: "As a consequence of profound societal changes, the principal issues of our day . . . reflect the problems of an industrial, dynamic, pluralistic society—including such matters as strike and labor conflicts, social and traffic insurance, pollution, consumer protection, and multinational corporations" (page 725). The conclusion, addressed from many points of view throughout the volume, is that law in rapidly changing societies must be open to reform and institutional change in keeping with its new "social role." Cappelletti describes that role as embracing the entire society: "One essential—possibly the essential feature of a really modern system of administration of justice must be its effective and not merely theoretical accessibility to all" (page 726). With such language permeating this interesting volume, it should be clear that it is about time for political science students of comparative development, behavior, and policy to get on speaking terms with students of comparative law.

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A World Elsewhere: The New American Foreign Policy. By James Chace. (New York: Charles Scribner's Sons, 1973. Pp. 108. $5.95.)

"Postwar American foreign policy has been set on a new azimuth," says the managing editor of *Foreign Affairs* and author of this little volume; "there is no turning back" (p. 10). Of what does the new direction consist? It appears to be at once both Gaullist and Monnetist, Nixonist, and Kissengerist. It is rooted in awareness that the worldview formulated to contain Communist territorial expansion is obsolescent and that a more centralized international system, better able to cope with problems like monetary stability, trade barriers, pollution, and population control is preferable. It is intended to improve relations with our ideological rivals, to be compatible with changing relationships with old allies, and even afford a position within the global balance ultimately favorable to us. The specters of Metternich and Bismarck lurk in the background as if to give the process a measure of historicity and legitimacy. It is with these roots of the new foreign policy that the first chapter of Mr. Chace's book is concerned.

The second is devoted to the problem and process of balancing the powers, an admittedly difficult matter with nationalism on the rise in the world. At least one takeoff point is the Nixon Doctrine, in its starkest form averring that "we will help those who help themselves." Its ephemerality becomes evident, however, with the observation that "It's his doctrine, and he can damned well do what he wants with it," attributed to Professor Kissinger. That substantive change has occurred is undeniable in light of the re-establishment of relations with Communist China, an economic stand that withdraws the dollar from use as the currency for financing world trade, the conclusion of agreements with the Soviet Union to limit the deployment of weapons of mass destruction, the attempt to play the game of balancing the powers on a world scale, and the retreat from maintaining a global gendarmerie while pretending that we can maintain our commitments. The accords of 1972 tend to establish the new direction, of course, but it is well to remember that doctrine, like the actors, is manipulable. One need only advert to the Yom Kippur War of 1973 and the tension aroused, detente notwithstanding.

The third chapter is "On Navigation" insofar as it deals with piloting through the tides and currents, the shoals and "shelfy Coast" that threaten to make the course difficult. The current we must ride is the new attempt at equilibrium; a world wholly new to the postwar imagination, no longer bipolar, lies ahead. It will involve many overlapping coalitions and power relationships like the central strategic balance between America and Russia; the North-South axis, such as the European-African connection; and the movement of transnational forces, like economic agents and agencies, personnel and skills, even ideologies. America, it is concluded, in her search for new directions in her foreign policy, must pursue two lines simultaneously: she cannot disregard the realities of the nation-state and the "balance of power" politics that it implies; she must, concurrently, avoid the excesses of neo-nationalism—with its concomitant unilateralism—while finding ways of devising new relationships that will cut across existing alliances.

This is a slender volume, dispassionate, analytical, lucid, and logical in structure. From those associated with *Foreign Affairs* we expect solemnity and respectability, and this is what we find here; in a certain sense what is contained in Mr. Chace's book smacks of the prevailing apologia for current policy as proclaimed and practiced by the Nixon-Kissinger Administration. Conceivably more in the nature of criticism might have been forthcoming, obligatory as is such function on the part of academicians and theoreticians like Mr. Chace. He says little, for example, about the economic future, emphasizing throughout and perhaps alone the undeniably important political security area. He offers little in the way of explanation of why the Vietnam incursion was desirable or necessary as part of the total mosaic, and yet eschews criticism of it. He does not dare to venture courageously beyond the nation-state principle, as, indeed, man may yet have to, to overcome the insecurities of all the earlier ages. Nor does he underscore the power of both justice and morality in the resolution of international tension and conflict, wanting in no small measure as they are in the affairs of men, abstract as they frequently are in the minds of men. He does, nevertheless and notwithstanding, provide us with a set of valuable insights into the views that vivify the foreign relations sector of the United States government.

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Soviet Policy toward Black Africa: The Focus on National Integration. By Helen D. Cohn. (New York: Praeger Publishers, 1972. Pp. 334. $17.50.)

This valuable volume presents a thorough analysis of Soviet theories on the nation-building process in black Africa. Professor Cohn makes excellent use of her training in both Soviet and African studies. Her book is one of three recent studies which have done much to deepen our knowledge of Soviet policies toward Africa, the other two being Robert Legvold's *Soviet Policy in West Africa* (Cambridge, Mass.: Harvard Univ. Press, 1970), and Edward T. Wilson's *Russia and Black Africa before World War II* (New York: Africana Publishing Corp., 1974). All three studies document the thesis that Russian national interests are paramount over Soviet ideology in shaping policy toward Africa.

In contrast to Legvold's general analysis of Soviet policy efforts in six West African countries and Wilson's detailed historical treatment, Professor Cohn has concentrated on one main topic—problems of national integration in Africa as viewed by Soviet academic and governmental theorists. She questions Western views which stress the lack of objectivity in Soviet writing and the subservience of African studies to government policy. She acknowledges the doctrinaire ideological approach of the Stalinist era when the "zigzag" course of the International Communist Movement resulted in Communist loss of "any real influence in the colonies" (p. 24). The Khrushchev regime, however, inaugurated a more pragmatic assessment of nationalism, a trend that flowered when Leonid Brezhnev and Aleksei Kosygin came to power. In place of the usual fostering of African nationalism as a weapon in the anti-imperialist struggle, nationalism came to be regarded as a "positive phenomenon" only if it strengthened the Soviet position in international affairs (p. 35); otherwise it was opposed, the Biafran case being the prime example. Although the Eastern Nigerian leaders had earlier been praised as the progressive forces who were resisting "the feudalists and comprador bourgeoisie of the Northern and Western regions" (p. 169), in the Biafran rebellion the Soviet Union supported the stronger Federal Government against the weaker Ibo rebels in order to secure access to Nigerian markets, to displace British dominance, to impress the OAU and many black African governments, and for other pragmatic reasons.

The author points out that the death of I. I. Potekhin (the leading Soviet Africanist) in September 1964, and the deposition of Khrushchev in October made possible a flourishing debate on the applicability of the Soviet nation-building model to Africa. The 1966 declaration of the Brezhnev-Kosygin regime that the domestic development of the Soviet Union was its primary internationalist duty, was accompanied by a consequent waning of official interest in Africa, which freed the academic community from some of its earlier constraints.

The analysis of the ensuing debate on national integration in Africa is the heart of Professor Cohn's book. The Soviet model on nation building had emphasized a dominant nationality group, universal education, a single national language, and the eradication of religion and traditional cultural values. The inapplicability of this model to most African countries was already apparent to Soviet scholars who, toward the end of the Khrushchev era, had begun to urge a gradual approach toward social reform. Now the Soviet Africanists, somewhat like those in the United States, began to acknowledge the progressiveness of some of Africa's military leaders. Soviet scholars pointed out that the vanguard parties used in the integration of the U.S.S.R. could degenerate in Africa into institutions with little contact with the masses. The development of socialist economies and political parties would therefore be a slow process.

The right of self-determination continued to be stressed, but the Brezhnev-Kosygin regime allowed increased pragmatism and objectivity in the academic world because they were compatible with such official objectives as reducing the economic drain to Africa, promoting African stability so that official attention could concentrate on the higher priority Middle East and Southeast Asia, and establishing stronger government to government relations with as many African states as feasible. The author thus relates academic writings to government policies, giving her study a wider significance.

Although the task of organizing her material on Soviet nation-building theories was not easy, Professor Cohn has done it effectively in two thorough chapters (pp. 77–180) on short-term solutions to African centrifugalism in the social and political spheres, followed by two chapters on longer-term answers (pp. 181–255). The short-term solutions deal with educational, religious, cultural, and political measures suggested by Soviet writers to accelerate the nation-building process, while the long-term answers concentrate on economic development (preferably noncapitalist), and on the elevation of the progressive sectors (intelligentsia, bureaucracy, and military) to the dominant role in the political system.

These four chapters illustrate with abundant quotations how Soviet scholars moved away from their preconceptions toward a more realistic assessment of African complexities. The author also shows how Soviet rethinking paralleled in some respects the reconsideration by Americans of their own views on Africa.

The dominance of Soviet pragmatism over ideology is demonstrated not only in the Biafran case, but in the willingness to set aside the Stalinist definition of a nation when the African states were striving for independence, and to support unification of French and British Cameroons. This flexibility was also evident in the views of Soviet analysts on the role of religion in Africa, the question of a national language, the problem of cultural relativism, and the acknowledgment that the military and the intelligentsia play a more important role than the workers or peasants in nation building.

The author has well demonstrated her initial hypothesis that "the decade of the 1960s has witnessed an increasing impartiality in the works of Soviet Africanists" (p. 4). Moreover, she suggests that, aside from possible conflict over Somalia because of its strategic location vis-à-vis the Middle East, there are unlikely to be enough incidents in Africa to induce the Kremlin to refocus its attention on that continent in the next decade. This, she concludes, "may well have the fortunate consequence of allowing scholarly objectivity to flourish among Soviet Africanists" (p. 269).

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The Anatomy of Influence: Decision-Making in International Organization. By Robert W. Cox, Harold K. Jacobson, Gerard and Victoria Curzon, Joseph S. Nye, Lawrence Scheinman, James P. Sewell, and Susan Strange. (New Haven: Yale University Press, 1973. Pp. 497. $15.00.)

It is difficult to know who has done greater damage to the study of international organizations—their euphoric friends or skeptical critics.

Thus it is especially useful in this area to have more dispassionate, scientific analyses such as the book reviewed here.

This work does not pretend to cover the whole waterfront. It concentrates on the decision-making process in eight important specialized agencies—more specifically, "to identify influence, to weigh and assess it, and ultimately to explain how influence is acquired and exercised in international organizations." This, in itself, is a major contribution to a more sophisticated analysis of international institutions and is part of the broader stream of comparable studies of all political organizations, both national and international. The authors and chapter subjects are: Robert Cox and Harold Jacobson, "The Framework for Inquiry" and "Power, Polities and Politics: The Environment"; Jacobson, "ITU: A Potpourri of Bureaucrats and Industrialists"; Cox, "ILO: Limited Monarchy"; James P. Sewell, "UNESCO: Pluralism Rampant"; Jacobson, "WHO: Medicine, Regionalism and Managed Politics"; Lawrence Scheinman, "IAEA: Atomic Condominium?"; Susan Strange, "IMF: Monetary Managers"; Gerard and Victoria Curzon, "GATT: Traders' Club"; Joseph S. Nye, "UNCTAD: Poor Nations' Pressure Group"; and Cox and Jacobson, "The Anatomy of Influence."

Unlike the usual panels, this team of eight authors, led by the chief participants, Cox and Jacobson, worked long and diligently to agree on a rigorous and integrating comparative methodology which has made this work more internally consistent and productive than most such enterprises. As each author has dissected his organization, he has concentrated on several major aspects: general historical evolution, current structure and functions, relative emphasis on different types of decisions (representational, symbolic, boundary, programmatic, rule-creating, rule-supervisory, and operational), types and roles of major actors (representatives of governments, agents of private organizations, international bureaucrats, etc.), and patterns of interaction with national states and the international environment.

As a result of this analysis, this volume penetrates more deeply and objectively into the boiler room of international organizational decision making than any other work available. The authors emphasize that, contrary to easy generalizations about the sources and exercise of influence by either major or minor powers, the situation is extremely complex, varying greatly from agency to agency, issue to issue, and stage to stage of the international environment. While the most powerful states generally dominate, especially in agencies and on questions considered vital to their interests, even their influence is often hobbled by numerous factors, including the inadequacy of their own capabilities—financial, human and substantive—and the opposition of other nations. Other states may wield greater influence than their material condition would suggest because of other sources of influence, including skilled representation and the assumption of key roles as initiators, controllers or brokers. The relatively new and growing cadres of international bureaucrats, especially those at the top of their hierarchies, are capable of exercising crucial leverage because of their strategic positions at the helms of these agencies, their intimate knowledge of their agencies' business, and their roles as servants, and therefore confidants, of all the members.

Other findings include the following: (1) a general shift among the eight organizations toward aiding the less developed countries, with a consequent increase in field operations and less emphasis on using such "functional" institutions to move toward general multilateral integration; (2) the creation of "shadow organizations," outside the agencies with more universal membership, to handle problems of direct and major concern to the more developed countries; (3) increasing "bureaucratization" of the eight organizations with more initiative and influence being exercised by those who have been long and intimately associated with the agencies and greater momentum toward self-motivated growth; (4) a difference between agencies with greater consensus on organizational ideology (e.g., IMF and GATT) and those with greater ideological conflict (e.g., UNESCO and IAEA); and (5) a difference between organizations which are considered to be of more vital importance to the major powers and are thus more closely controlled by such governments (e.g., ITU and IAEA) and those regarded to be less crucial and thus allowed greater internal autonomy (e.g., WHO and UNESCO).

Strong as this work is, it has certain weaknesses, some of which are inevitable in such an enterprise. The sample of agencies studied must be selective, and the selection necessarily affects the conclusions. In these, as in any institutions, the patterns of human interaction are tremendously complex and cannot be easily or precisely analyzed. The simple typology of national political systems used by the authors (competitive, mobilizing, and authoritarian) scarcely does justice to the variety of polities which populate the international landscape. While the authors rightly emphasize that international organizations are part of networks of relationships which intermesh with national societies, the authors give little attention to the intranational structures and processes which relate most directly to multilateral agencies. And they draw no conclusions regarding the linkage between their analysis and the larger body of related studies concerning intranational organizational behavior, although the two aspects of human interaction are closely connected and relevant to each other.

The authors also join the perennial debate over the "functionalist" approach to international integration. Unfortunately, they concentrate their fire primarily on the traditional concept of functionalism rather than the "neofunctionalist" approach. The classical version argues that the most promising path to international integration is via decentralized multilateral enterprises whose activities are technical, functionally specific, essential, and divorced from more general, political agencies—both national and international.

This volume criticizes this thesis by pointing out that such enterprises cannot escape politics, that what is considered "essential" varies according to different national concepts and conditions, that most governments—large and small—do not want international integration, that the trend is not toward integration but toward using international agencies to help the underdeveloped nations, and that the nation-state is still the most effective instrument to protect the interests of its citizens. There is much validity in these arguments. Politics must, and should, be part of any system dealing with governmental institutions and policies. Nationally oriented groups do generally resist merger with other nations. And the nation-state is still the strongest political agency—for good or ill. Nonetheless, the evidence in this volume, and in other sources, demonstrates that functional agencies, such as those studied here, have made substantial and measurable progress in dealing with some of mankind's most pressing problems. No one can predict whether this trend will predominate over increasing intranational cohesion, but there is, at least, absolute movement toward greater international communication and cooperation.

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A Socioeconomic Profile of South Africa. By William R. Duggan. (New York: Praeger Publishers, 1973. Pp. xviii, 181. $15.00.)

This book is of interest to the student of U.S. policy toward South Africa because of the experience of its author. William Duggan has had a distinguished career in the Department of State, serving as African affairs specialist in the Policy Planning Council during the presidencies of Kennedy and Johnson and for seven years in South Africa, where he was consul general in Durban before his retirement in 1971. He has written a largely factual handbook, covering familiar ground. (The title is misleading since the book is as much concerned with politics and international relations as with society and the economy.) Of special interest, however, are his evaluations and judgments and his proposals for change in U.S. policy. Regrettably, the proposals are sketchy, and their implications are not worked out.

Mr. Duggan has no illusions about the quarter-century since the Nationalist Party came to power in 1948. The regime has become "increasingly repressive" in maintaining "absolute White domination" (pp. xiv–xv). Political freedoms have been "ever more severely curtailed" (p. 27). Although *verligte* and *verkrampte* Afrikaner Nationalists do not disagree fundamentally on White domination, their dispute has forced the government to move further toward the right. Meanwhile, the liberal White opposition has become weaker, and the opposition United Party has become "increasingly conservative" (p. 38). Although White fear has "intensified," Whites make the "false and dangerous presumption" that the "deceptive calm" means "a 'happy' acceptance by non-Whites of the status quo" (pp. 50, 166). Indeed, while emphasizing the strength of the regime, Duggan compares "racial turmoil in southern Africa" to a "boiling cauldron" that "could boil over at any time" (p. 172).

Duggan welcomes the government's Bantustan program of separate development because of its unintended possibilities. Although it is thus far "only a temporary rural palliative" that is useful for international propaganda, it has reawakened African nationalism and provided an official platform for a new Black leadership (p. 62). The heroic figure who has emerged is Chief Gatsha Buthelezi of the Zulu "homeland," a "responsible" leader. What is curious in Duggan's treatment of African politics is his slighting of the national appeal of the older African leaders and his characterization of the non-White majority as apathetic in the years before the African National Congress and the Pan Africanist Congress were banned. Buthelezi himself has recently described such leaders as Nelson Mandela (imprisoned for life), Robert Sobukwe (under ban), and Oliver Tambo (in exile) as "heroes to Blacks" and men whose participation in dialogue is essential if there is to be any hope of peaceful change in South Africa.

Despite the apparently hopeless prospect of such participation and despite his own analysis of South Africa's backward trends, Duggan retains a liberal optimism that the South African situation can be defused. Because the regime is not only morally offensive, but also endangers international peace, he believes that the ambivalence of U.S. policy, which has "softened" under Nixon, should be replaced by an "orchestrated strategy . . . of carrots and sticks" conducted jointly by the United States and some other Western countries (pp. 147, 172). Since Duggan considers South Africa a part of the "free world," however, his policy does not appear radically different from Nixon's (p. 71). "Dissociation" is "impractical" because the United States and Britain are "heavily involved with South Africa in financial, trade, and defense areas," for example, in jointly countering Russian naval influence in the Indian Ocean (p. 172). One carrot, designed to promote "more constructive association," would be the resumption of naval visits to South African ports (pp. 171, 174). At the same time, Duggan would press for social and economic amelioration (not political equality) and building up the Bantustans, which would improve South Africa's "international image" (p. 171). The strategy would include diplomacy, persuasion, and time for "Western business ethics" to exert influence (p. 168).

Duggan's reference to such sticks as visa retaliation and penalties on substandard business and his suggestion that two-year deadlines be linked to stronger pressures whet one's appetite for fuller analysis. One hopes that he will provide such analysis in the future as well as deal with some fundamental questions that the book does not, for one reader, satisfactorily answer: Does not his approach strengthen rather than weaken White domination? And are not the risks of identification with a racist regime too dangerous for the United States to accept in the last quarter of the twentieth century?

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Creating a New World Politics: From Conflict to Co-operation. By David V. Edwards. (New York: David McKay Company, Inc., 1973. Pp. xvi, 191. $2.95.)

Whatever the final evaluation, one must welcome Professor Edwards's bold attempt to apply theories of international relations and social psychology to long-neglected normative issues. He poses as his central question: "What can be done to improve the quality of international relations?"—to reduce the high incidence of violence, restrictions on freedom of action and choice, and the continuing gross inequalities between rich and poor? At the level of basic values, he suggests, differences among international theorists are secondary, and none offer promising policies for the realisation of these values. The "idealists" offered fantasies, the "realists" see no remedy for continued violence and injustice, and the "neo-realists"—those who place their hopes in non-intervention and reduced American involvement in world affairs—offer no convincing prospect of improved global relationships but may inadvertently worsen them. If involvement is inescapable, how can it be made to serve constructive ends?

He finds his answer in the notion of the social construction of reality. Social relations are not immutable but are continuously created by the thoughts, choices and actions of individuals. Where this is not conscious, outcomes tend to be determined by rigid images and routine modes of action ("processes"). Greater consciousness leads to awareness of opportunities, in particular opportunities for fundamental change. Perceived social constraints can be overcome when their mechanisms are understood. For example, the action-reaction processes discussed by theorists such as Lewis Richardson and Kenneth Boulding do not inevitably lead to the escalation of hostility and arms races: if we understand the processes involved we can bring them under control. Understanding includes self-criticism: "realists" should long since have recognized that their arms policies are not only costly in terms of welfare goals but self-defeating in terms of security goals. The threat of annihilation is not an acceptable long-term basis for security.

The author's basic conclusion is that only a radical change in the international system can improve the quality of international relations, but that this is far more feasible than is commonly recognized, since the constraints generally believed to sustain the system as, in the last analysis, a threat system, are not inherent in international reality as such, but in orthodox images of reality. What is needed, therefore, is a new image of reality, in particular a new conception of world order transcending the usual power-oriented conceptions, and reflecting the cultural differences in contemporary international society. The development of the youth culture is a promising sign but is no guarantee that new values will prevail.

A summary cannot do justice to Edwards's closely reasoned argument, but part of the difficulty is in the argument itself, which remains at a highly abstract level. There are no more than faint hints at what the new reality image and the new conception of world order might be. Since specific policy proposals are omitted, even for illustrative purposes, Edwards fails to expose the problems of his general approach, in contrast to earlier authors whom he cites with approval such as Charles Osgood and Amitai Etzioni. The problems may not be insurmountable, but they certainly need to be surmounted. *Are* the constraints of power politics, and the conflict patterns studied by game theorists, solely in our images? There is no discussion of the reasons which have led most students of international relations to think that they are not.

A second set of questions concerns the book's preoccupation with super-power relations. The issue of international justice, raised at the outset, rapidly fades from view, and the new reality seems to promise much more domestic than international welfare. It is true that the goal of survival in the nuclear age should not be complacently taken for granted, but the modest efforts at reality creation on the part of Kissinger, Nixon, Brezhnev, Brandt, and Chou En-lai suggest that accommodation among adversaries is now becoming easier than agreements among allies or the resolution of the new issues which are now presenting themselves: issues concerning energy or the oceans, the stability of the international economy or the wider environmental and ecological issues. In all of these, the role of the material world and material interests are inescapable, the role of "reality creation" less than in the relations of nuclear adversaries. The one-dimensional quality of Edwards's analysis does not do justice to multidimensional, and changing, international reality. In the last analysis, his book is more important for the questions it raises than for its answers to them.

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The Fortunes of the West: The Future of the Atlantic Nations. By Theodore Geiger. (Bloomington: Indiana University Press, 1973. Pp. 304. $10.00)

Those who write of the future run certain risks. Some of these may be obviated by setting the "future" sufficiently ahead in time so that neither writer nor reader can verify the correctness of the predictions. By the time the work is published, however, the baseline for forecasting may already have shifted, if it was set in the "present." Such a problem can presumably be met by pushing the baseline far into the past. This solution was tried by Theodore Geiger, director of international studies for the National Planning Association. He has chosen to discuss "the future of the Atlantic nations" by stressing "the wider social-institutional and the deeper psychocultural aspects of human decision making and action." In doing so he admits that "the determinative relationships" will be "so diverse, complex and ambivalent" that the projections are bound to be "qualitative and generalized" (p. 5).

Through a survey covering 2,000 years, the author devotes about one-fifth of his study to the origins of "Western redemptive activism" and "Western rationalism and individualism," qualities which he finds characterize the behavior and outlook of those living in Western Europe and North America. After comparing the sociocultural sources of the Soviet Union and the United States and finding that the foreign policy of both countries tends to be messianic, in their competing drives to transform the world, he then seeks to discover whether or not another world war is in prospect and comes to the tentative conclusion that it is not. From that point he assesses the possibility and likelihood of European unification, which he distinguishes from "integration," defined as "the removal of barriers to trade and capital movements among a group of countries" (p. 130). Next he examines how the countries of Western Europe will be related to those in North America, with special attention to the economic aspects. Finally he takes a big jump into the 21st century to make some admittedly speculative projections of the possible transformations in Western society.

One of the underlying themes of the book is that changes will flow from the emergence of a new breed of "technocrats" just coming of managerial age in the United States and soon to emerge in Western Europe. He seems to expect that these professionals, well trained in the requisite skills to handle the complex problems of a complex society, represent an improvement on the political amateurs who preceded them. Nevertheless, he occasionally expresses some concern that their own sense of world mission may need to be disciplined. His observations on the technocrats are one of many signs that the book was written with a perspective from the Kennedy years. Since the revelations of Watergate, a decade later, the reader is not so certain that the skilled manager of public affairs may not sometimes be deficient in some other qualities than professional modesty. The old-fashioned politician is better than he appears in this book.

Thus an analysis which purports to look ahead to a time several decades from the present tends to be peculiarly unhelpful on the years just past or about to come up. Whether or not actually written before the 1970s, this book provides relatively little which could explain such developments as SALT, MBFR, and West Germany's *Ostpolitik*. There is only a side-glance at environmental issues, the questioning of economic growth, and the demands for population control. Although the author mentions the Vietnam War from time to time, and although he states that it was a prime cause of Western European dissatisfaction with American policy, he seems to underestimate its very deeply disruptive effects upon all aspects of society, economics, and politics in the United States. Nor does he allude to another form of violence which has increasingly afflicted advanced countries in a frightening form of transnationalism, the terrorist attacks on innocent people in the name of political causes espoused not by governments but by extremist minorities. "Europe" means Western Europe, but even in Western Europe Scandinavia is scarcely mentioned, and the relationships of Western European countries to those in Eastern Europe are almost totally ignored.

The author is by profession an economist, which may explain why he overlooks the contributions of political scientists in many ways, (True, that in his 21st century projections he argues for a new kind of academic specialist who will in fact be a *generalist*, illustrating this need by referring to his own experience in finding his expertise inadequate for handling some technical questions.) It is not just that in listing the new breed of technocrats he includes all the social sciences except political science, nor is it the general absence of political scientists among the authorities he cites. His analysis ignores the recent literature on the subjects he treats, and he repeats old patterns of dealing with international relations. He makes use of the outmoded billiard-ball model of state behavior even if occasionally referring to elite groups within some states to distinguish them from the masses. In discussing the possibilities of a united Europe he speculates on whether, once formed, it might also try to expand its power. Thus he unconsciously adopts the "power politics" view of the state inevitably seeking to raise its position in one hierarchy of competing powers.

Theodore Geiger accepts unquestioningly the idea that to be a very great power (to distinguish such from the superpowers he coins the term "proto-superpower") the possession of nuclear weapons is necessary. This assumption leads him into somewhat absurd speculations, such as imagining Mexico or Argentina or "perhaps even Canada" as nuclear powers, thus completely ignoring the particular characteristics of each of these countries which make the notion grotesque. Rather than hard evidence Mr. Geiger often offers highly impressionistic statements; for example he writes that "the majority of Europeans are aware that . . . " and refers to "the American attitude toward Europe" (pages 172 and 72). His treatment of NATO concentrates on its military aspects, without attention to the role of the North Atlantic Council in promoting exchanges of foreign policy views and in seeking coordinated stands on such common concerns as arms control, a subject he omits.

Mr. Geiger explicitly denies any immediate policy implications for this work, although he was in the government service for many years. Whatever its shortcomings in the field of politics, however, this book could serve other policy analysts as it probably served the writer, by providing an historically based philosophical foundation for their choices. It should certainly make its readers more conscious of deeply rooted tendencies in Western society. They had better not rely on the index, which has many remarkable omissions.

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Social Science Literature: A Bibliography for International Law. By Wesley L. Gould and Michael Barkun. (Princeton, N.J.: Princeton University Press, 1972. Pp. xiii, 641. Index of authors. $15.00 hardbound, photo-offset.)

The purpose of this annotated bibliography of close to 3,000 items is to make it easier for students of international law and practicing international lawyers to uncover and make use of social science writings that might be relevant to the international law field. A related and loftier goal is to help bridge the disciplinary gap between lawyers and social scientists.

The book is not organized along lines readily familiar to international lawyers, but rather the organization reflects social-science methodological concerns that, in the authors' view, would be useful to students of international law. The latter would have to be somewhat behaviorally sophisticated to look up entries under "system mapping" or "factor analysis," to mention two of them, but then perhaps the authors are wise in assuming that any international lawyer who buys this kind of book will already have some of this sophistication. Even so, to be on the safe side, the authors have included more of the traditional categories of international law (such as "Crimes," "Treaties," "Aliens," "Air Space") as subheads, and these categorizations are usually accompanied by very helpful cross-references to items listed in other categories.

Naturally any reader or reviewer could quarrel with the selection of entries, but in the main one must acknowledge that the authors have done a yeoman job and have produced a very valuable reference work. The selection of entries seems best in the area of political science, and perhaps poorest in social psychology; in the latter, entries tend to be chosen, it seems, on the basis of titles rather than substance, since a number of significant articles and books on areas that have nothing superficially to do with international law or politics are missing. But then, to include everything relevant would be to require theories of relevance that would probably engage the authors full-time and leave them no time to compile a bibliography such as this one.

This book and a companion volume by the same authors (*International Law and the Social Sciences*) grew out of panel discussions sponsored by the American Society of International Law, and that Society was helpful in getting the ventures started. This leads me to an observation that may not accord with prevailing establishment views. I have found that international lawyers and students of international law have been trying very hard to "plug in" to behavioral sciences; they recognize as relevant and important the findings of social scientists and usually assume a continuing self-educational responsibility in reading and studying the behavioral contributions that might be relevant to international law. They initiate studies such as the book under review, and they always acknowledge the relevance and importance of behavioral science. But what about the other side of the coin? Do behavioral scientists bother to find out what international lawyers are talking about? Do they take the time and trouble to learn about the language of law? Do they care about the kinds of questions raised by international lawyers? Here, with few exceptions, I have found an amazing provincialism. Behavioral scientists seem to assume that there is absolutely nothing about "law" that they need to know about in order to plunge right in with their acquired methodologies and proceed to describe what is relevant and important for international law. (To be sure, matters were even worse in the past: Hans Morgenthau started everything by saying that international law did not exist, and it has taken political scientists quite a while to overcome that false start.) Maybe what we need is another couple of books by Gould and Barkun on international law for social scientists.

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Japan and East Asia: The New International Order. By Donald C. Hellmann. (New York: Praeger Publishers, 1972. Pp. 243. $8.00.)

"Japan must play a central and vastly expanded role in East Asia." This opening sentence of Professor Hellmann's analytical study of Japanese foreign policy is currently an almost universally voiced theme. As Japan continues to build up her position as a global economic power, questions concerning political and strategic aspects of her foreign policy become more insistent. Japanese reluctance to play a more active international role has seemed increasingly out of keeping with the potential influence afforded by her economic strength. How does one account for "low posture" diplomacy? Is it a stance which can be continued? If change is to occur, whence comes the impetus for it? What form and direction is the change likely to take? These are some of the questions with which Professor Hellmann is concerned.

It is one of his major contentions that Japanese foreign policy is essentially reactive, the product of external forces rather than internal initiatives. There are both external and internal reasons for this pattern. Externally, the nature of the postwar international order and of Japan's particular relationship with the United States made virtually inevitable the foreign policy adopted. Internally, the nature of the decision-making process, the style of leadership, and the partisanship surrounding foreign policy questions have produced a tendency to "immobilism." Professor Hellmann sees little prospect of significant change internally (reviving nationalism is of secondary significance), but he perceives in global and regional forces the agents of change. As the international order shifts from bipolarity to multipolarity, as China's nuclear force increases, and particularly, as American policy moves toward reduced commitment in Asia, Japan is being brought face to face with critical policy decisions.

In East Asia, Professor Hellmann argues, shifts in the power positions of the superpowers and the increased power of China and Japan are producing changes in the international order. Since the outlook for peace and stability within the region is poor, Japanese policy will have to come out of its "pacifist cocoon" (p. 140). Japan will need to act on the principles of *realpolitik* rather than on those of a "trading company" (p. 62). An active and influential role in the region will demand a level of military power beyond the current level of rearmament, which is determined by budgetary rather than strategic considerations. Since nuclear power is essential to such a role, Japan's ultimate decision on nuclear weapons will be derived from her decision on whether or not to play an active role. The author rejects the view that the development of a Chinese nuclear force will have little effect on Japan's decision. International competition between China and Japan is one of the chief characteristics of the emerging East Asian "subsystem." Japan will, therefore, keep its options open and the definite implication of Hellmann's view would be that of "going nuclear." He warns, for example, that the American stance on the Non-Proliferation Treaty could produce an issue leading Japan to break with American policy.

The American security tie, says Hellmann, has been the single most important factor shaping Japanese policy and has made quiescent diplomacy possible. Now, however, the end of blanket containment and the search for détente have changed the nature and scope of the American commitment in Asia. This raises questions about the continued commitment to Japan, for points of divergent interest are likely to proliferate, abetted by Japanese nationalism, which has a distinctly anti-American flavor. The need for Japan to develop "clearly formulated strategic objectives" (p. 151) becomes more urgent.

It is Hellmann's contention that such clearly formulated objectives have been lacking in Japan's economic relations with East Asia (defined as China-Taiwan, Korea, Japan, and Southeast Asia). Her preeminent economic position in the region in which she is the first or second trading partner of virtually every other state, forms the major part of the "entangling web" between her and the rest of the region (cf. Chapter 5). Foreign aid and investment, while comparatively small in size, are region-centered. The critical point, however, is that all Japan's economic policies have been without comprehensive policy design. They reflect the "rationale of a trading house" (p. 99). In the author's view, politics and economics are not ultimately separable, and Japanese foreign policy must move beyond its narrow economic orientation. While economic factors will not dictate Japanese policy in the region, they will exert a powerful influence. The "dynamics of East Asian politics" will ultimately determine whether Japan will "join the dance" of international politics (pp. 81–82).

The argument of the book is persuasively presented; the style is attractive, and Hellmann has a fine gift for phrasemaking. It is a book bound to raise controversy. Critics of balance-of-power politics will reject the basic premise. Sympathizers with the political opposition in Japan will have their hackles raised by some critical comments. Many would challenge the assertion that China has replaced the Soviet Union as Japan's greatest security threat. Is the threat of a Chinese nuclear force and of Chinese competition in the region viewed as seriously in Tokyo as the book indicates? In spite of seeming inertia, are there not instances of initiative? Incrementalism, which the author correctly sees as one characteristic of Japanese policy, can involve originality as well as reactivism. I would question the Asia-centeredness implied in this analysis. Granted that Asia will grow in importance to Japan economically and otherwise, it is difficult to see her relations with the advanced nations becoming relatively less important. Already more than one-half of Japanese imported raw materials come from advanced countries; and the problems of oil and sea lanes can hardly be met on regional terms. Professor Hellmann admits that Southeast Asia does not occupy a high priority in Japanese policy or political consciousness. For Japan, the vital areas are China and Korea. Does a regional subsystem remain a very meainngful concept for Japanese foreign policy? The case for regional identity is, at best, thin, and whatever the "identity" which modern Japan finally achieves, it will inevitably transcend regional terms.

To pose these questions is to indicate that Professor Hellmann has produced a stimulating book which confronts the critical issues. For idealists and realists, for generalists and specialists, this is an important work worthy of careful reading.

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Security in Europe. By Robert Hunter. (Bloomington: Indiana University Press, 1972. Pp. x, 281. $8.95.)

Dr. Robert Hunter's *Security in Europe* was first published in 1969. This second edition is based on the same chapter headings as the first, which is here updated and somewhat lengthened. Both editions are substantially padded out with appendices consisting of relevant documents, which take up almost a quarter of the book: as such, the present edition is not cheap. On the other hand, its subject, the problem of maintaining a feeling of confidence in the continuance of peace in Europe, is sufficiently weighty. Dr. Hunter begins his book, quite properly, with the question: What is security? (p. 1). But he does not answer it but instead goes on, in a curiously indirect manner, to say that (p. 3) "in post-war Europe the sense that somehow security was absent derived to a great extent from the lack of any clearly defined rules by which states could conduct their relations with one another, secure in the knowledge that everyone knew what he could and could not safely do." But this is putting the cart before the horse. Rules for conducting international relations and the assurance that they will be observed derive just as much, or probably more, from a feeling of security generally prevailing in the international system, as the other way about. If Dr. Hunter had stuck to his original question he might have told us more about the relations between rules and security, or rather security consciousness, for that is what we are really talking about when we discuss security as an international concept.

Instead we go on to "the beginning of the Cold War." Here it is stated that the Cold War was not inevitable and that is certainly a common opinion among contemporary writers "who"—Hunter says—"like me, will only know (the Cold War's) beginning from their elders." Then how did it start? The fact was, we are told, "that the onset of the Cold War really reflected the inability of both Russia and America to find ways either of overcoming their misunderstandings or of deciding where there was scope for bargaining." If Dr. Hunter learned this from his elders he could not have found them very helpful; one would have thought there was more to be said than that.

We are then taken on to consider the Threat (Chapter 2), to which the Western Powers' defensive movements, first decisively expressed in the Atlantic pact of 1949, were a reaction. Dr.

Hunter argues that in Western eyes the Soviet threat was not at first perceived as a military one and that it was only after "the context of military confrontation was established" (p. 39), that is, after the formation of the Brussels treaty in 1948, that Soviet forces in Eastern Europe were seen as a "positive threat" to the West. It is hard to see here how you can have a "context of conflict" between states unless each side, or at least one, perceives the other, whether rightly or not, as constituting a threat of some sort to itself. But it is true, of course, that once you have reason for perceiving the other side as threatful, it is difficult to interpret any of its actions as other than sinister.

Dr. Hunter is on safer ground when he goes on to write about "the role of institutions" in Chapter 3, in which he describes how the very institutional structure of the NATO and Warsaw pact systems contributed both to the rigidity of the Cold War and at the same time to its unloosening. But again Dr. Hunter's form of expression raises questions which plague the reader. "The format of unity in the North Atlantic Council," he contends, "inhibited the making of changes—as when the French, in 1958, proposed that responsibility for general Western interests, not just those within the NATO area, be divided among America, Britain and France" (p. 53). We can just imagine President de Gaulle's reaction if someone had proposed that "general Western interests" be divided among America, Britain and Germany, and if he was told that his reaction simply reflected a "format of unity" in the North Atlantic Council.

The main substance of the book, however, is contained in a long discussion of the East-West détente since 1962 in Chapter 6. It is a pity that there is not much analysis here of how the détente came about: light might then have been thrown on the probable viability and future of the détente, But there is quite a subtle and well-informed consideration of the many issues raised by détente. Some of Dr. Hunter's suggestions, as for instance for a European Security Commission (already proposed by the Russians), may strike some readers as evoking visions of one more lifeless East-West organisation. But the general tone is sensible and at times penetrating. Oddly enough, Dr. Hunter ends his book (p. 223) with a reassertion of the status-quo. "This system" (that is, the self-confronting system of NATO and the Warsaw pact) "has its tensions and problems, but to abandon it without finding something substantially better to put in its place might itself be the surest means of denying Europeans their present hopes." And it cannot be said that Dr. Hunter has invented anything substantially better.

This is a thoughtful book which raises questions as much about itself as about its subject. Because the standards of analysis it sets are high, it demands equally high standards of evaluation. Dr. Hunter has certainly shown that his subject, often used for the purpose of making glib or trendy judgments, demands a concentration of mental effort to master it. It is a pity that so often Dr. Hunter's own efforts are open to criticism.

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International Relations and World Images: A Study of Norwegian Foreign Policy Elites. By Helge Hveem. (Oslo: Universitetsforlaget, 1972. Pp. 336. $12.50.)

Is a nation's foreign policy based on elite perceptions of its role in the international system? This is the question which Helge Hveem raised in his thesis at the University of Oslo, further elaborated at the Oslo Peace Research Institute, and now presents in book form. His data are generated from a 125-item questionnaire presented to representatives of four elite groups in Norway—politicians, bureaucrats, interest groups, and people from the mass media. The purpose of the study is to see how the foreign-policy elite perceives the world and the nation's place in it. In a concluding chapter, he uses his interview data for observations and predictions about discrepancies between the perceived world of the elite and the "real world" and the probable consequences for future Norwegian foreign-policy making.

Considering that Hveem enters fresh into a very crowded field where foreign policy analysts for years have been chasing the key variables and arguing over their relevance, the study has many merits. It is technically well done and properly organized, most major propositions are illustrated by tables, there are some useful appendices, and the author shows a reasonable awareness of existing literature in the field. Apart from some occasional jabs at research activities that are not "peace-research oriented," the argumentation is on the whole sober and levelheaded. Hveem's distinctions among what he calls elite groups, opinion makers, and public opinion is done with understanding of the complexity of the problems of foreign-policy decision making. The book suffers from all-too-common tendency to draw too wide and sweeping conclusions from a single case. More serious doubts, however, appear when one looks more closely at the central issues in the book—the impact of elite images on foreign policy making, and the extent to which such perceptions can be substantiated by interviewing some one hundred members of a country's national elite.

To take the last question first, whose images did the respondents actually reflect? Did they respond as independent individuals or do they reflect the national system? Hveem has clearly assumed that the interviewees would respond independently of the organizations to which they belonged. Therefore, he seems surprised to find a majority of the respondents reflecting the country's official foreign policy. He also makes a point of the very high degree of elite consensus on major policy issues. A further major observation is how the degree of consensus increases with increased social position. Far from being surprising, this reaction within the elite is what one would normally expect, particularly in a small countrry like Norway. Consensus in the author's terms really means conformity. The elite groups are relatively small and keep a close watch on each other. Most of them have arrived at elite positions by accepting the nation's official policy. The highest respondent scores appear on the questions where the interviewees have a neutral option, where they can avoid taking a clear stand or position on a certain issue, and there is less risk of sticking one's neck out. The respondents knew which were the "right" positions to take in terms of official Norwegian foreign policy, and most of them reacted correspondingly. One cannot avoid some doubts about conclusions drawn on the basis of the images which these answers reflect.

One should also be aware of another modifying factor: "interview fatigue." In some countries (Norway is one of them), social scientists have conducted survey research on elites for more than fifteen years. Because these groups are fairly small, the same persons have been exposed to questionnaires over and over again, in some cases several times a year. They know the interview techniques and also the attention that some of the resulting publications may get. This seems bound to affect their responses and "perceptions." Their reactions may vary. Some respondents welcome interviews as a break in an often drab bureaucratic routine and take great delight in adopting exotic and eccentric positions to test the reaction of the interviewer (often quoted among amused friends at subsequent parties). In such cases, which are no longer rare, one may be measuring moods and whims, rather than perceptions. Other respondents, probably a majority, are more concerned with possible consequeneces and take a vague, cautious approach, sticking close to the dotted line of official policy and qualifying their answers where there are no accepted, known guidelines. Given this background of consensus/conformity, one cannot really know whose images are being recorded in the survey.

Another, more fundamental question concerns the relevance of elite perceptions for the making of a nation's foreign policy. Hveem's major thesis is that the images which a nation's elite has formed of the international system will have a decisive influence on its foreign policy. It may even have a predictive dimension. If one knew the images of the relevant elites, one could then forecast the directions of its foreign policy. Suppose that it was possible to obtain a set of "correct" elite images, is really the relationship between what elite groups think of the world and what their nation will do sufficiently established to warrant predictions!

These two questions which actually form the core of the study are not satisfactorily resolved. Hveem's study implies that the elite national decision makers are mainly concerned about the place and ranking of their nation in the international system. He assumes that they will define their nation's role in systemic terms. Some of the one hundred respondents might do so, but it seems highly improbable that more than a scattered few of these hard-pressed politicians journalists and bureaucrats would give much thought to such abstractions as the international system. Further, it is even more improbable that such perceptions as might exist about Norway's place on the international ranking lists and its impact on the system should be a major factor in its day-to-day foreign policy decisions.

The world looks different when seen from a very small and a very large state. Small states do not generally see the working of the international system as their national responsibility. Their verbal activity on the global level is often more a reflection of domestic considerations than a preparation for national foreign policy actions. Instead of asking members of the elite what they *think* should be done on the world scene, Hveem might have asked them what their nation was prepared to do to make it happen.

Images versus reality is in fact another way of spelling out the old dichotomy between declaratory and action policies. If the goal of the exercise was to establish a nation's declaratory policy, one might suggest other and more direct approaches than by interviewing a sample of its foreign policy elite. A major goal in foreign policy analysis is to reduce the subjective element to a minimum and increase the amount of hard data, including such observable and measurable examples as treaties, agreements, organizational affiliations and behavior, public statements, and domestic political constellations. Until such sources are exhausted, it would seem a detour to use elite perceptions to establish what might be a nation's declaratory policy. Recording thoughts and beliefs on world affairs may be interesting and worthwhile as an intellectual exercise, but Hveem has not shown that there is a close relationship between confessed elite perceptions of international relations and the nation's foreign policy as it actually is carried out. This might be kept in mind as a possible and more rewarding second stage of the study.

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China and the West: Mankind Evolving. By Robert Jungk, Ernan McMullin, Joseph Needham, Joan Robinson, Stuart Schram, William Sewell, and Bernard Towers. (New York: Humanities Press, Inc., 1971. Pp. xvii, 144. $5.50.)

China and the West: Mankind Evolving is the fifth volume in the Teilhard Study Library dedicated to the memory of the Jesuit paleontologist and thinker Pierre Teilhard de Chardin who died in 1955. Father Teilhard was in the same tradition as that group of worldly-wise Jesuits who went to China in the last decades of the Ming dynasty to proselytize but stayed on to write a splendid page in the history of cultural relations between East and West. Teilhard carried on much of his professional research in China, and the same breadth of vision that characterized his predecessors can readily be seen in his observations on communism in China. In 1947, at a time when the Vatican felt besieged and persecuted by what was then regarded as a monolithic anti-Christ, world communism, we find Teilhard speaking of communism in China as "that extraordinary and irresistible movement which, suddenly turning the old, traditional confucianist zest *for* man into a wholly modern faith *in* man, makes of the least Chinese peasant a proselyte zealous to the point of death" (p. xv). This faith in man, he wrote to a friend two years later, "could only be matched or surpassed by a Christianity where faith in God has incorporated the new human faith in the early future of Humanity. . . . " Regrettably, Teilhard observed, Rome did not yet see that " 'faith in man' is a distinctly new element in the human soul" (pp. xv–xvi).

Although only one of the seven contributors to *China and the West* (Ernan McMullin, "Teilhard, China and Neo-Marxism") focuses directly on Teilhard, all of the essays "explore the theory of convergence in human evolution" (p. xvii) which permeated the writings of Teilhard. Thus, the reader who imagines that the title of the book promises discussions of such topics as the Sino-Soviet conflict or the waxing and waning of Sino-American relations will be disappointed. The book is more about "mankind evolving" than it is about "China and the West" in the sense that that phrase ordinarily suggests. Indeed, two of the contributions and the appendix do not relate to China at all.

Joseph Needham, however, needs no introduction to China specialists. His massive multivolume study *Science and Civilization in China* is one of the most ambitious and valuable scholarly projects undertaken in the field of Sinology in this century. Presented in the book under review is a highly condensed (16 pages) summary of some of the themes which run through his *Science and Civilization in China*—for example that Chinese scientific accomplishments far outshone those of the West for at least a thousand years and that Europe was discovered by the Chinese rather than the other way around. But there are also some interesting reflections on the Cultural Revolution which Professor Needham believes is the "descendant of some of the ethical insights of Confucianism and Taoism" (p. 26). He traces, for example, the Maoist command to "fight self-interest and struggle against privilege" to the Taoist insistence that the Sage must make himself lower than the people so that, whether he leads or follows the people, the people will feel no offense.

Stuart Schram in his "Social Revolution and Cultural Revolution in China" presents here little that is not more fully set forth in his *Mao Tse-tung* (1966) and *The Political Thought of Mao Tse-tung* (1969). It is nonetheless a thought-provoking essay in which Schram gets to the heart of Mao's contribution to Marxism—the notion, born in the grim siege years of Chingkangshan in the late 1920s, that "by changing people's mentality through indoctrination and study, one could also change objective social reality" (p. 66). The test of the axiom came in the early land reform attempts of the 1930s in Kiangsi when Mao discovered that mere changes in the formal structure of property control in the countryside would not bring about lasting change. Power inevitably gravitated back into the hands of the old privileged classes. The only way that this reversion could be prevented was to direct efforts not so much at the social structure but at the minds of the poor peasants in order, as Schram writes, "to develop their self-confidence and allow the conviction to take root that henceforth they were the masters of society" (p. 67).

This goes far towards explaining the meaning of the Cultural Revolution which, as Schram views it, was brought about to insure that positions of authority are to be allocated to individuals delegated by the masses. "[T]he whole point of the Cultural Revolution," he writes, "is that neither the technical skills of the engineers, nor the political skills of the Party and government cadres, shall ever give them a permanently privileged status in society" (p. 77).

Essays on "Society and Economics in China Today" by Joan Robinson and "Religion in China Today" by William Sewell round out the contributions directly related to China. Also included in this volume is "Evolution and Revolution in the West" by Robert Jungk, and "Mankind Evolving" by Bernard Towers, the General Editor of the Teilhard Study Library. While I have concentrated on only two of the essays for reasons of space, all of the essays in this book are of uniformly high quality and make a distinguished addition to the Teilhard Study Library.

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Soviet Relations with India and Pakistan. By Davendra Kaushik. (New York: Barnes and Noble, Inc., 1972. Pp. 119. $5.00.)

In 1920 a member of the Soviet Commissariat of Nationalities suggested that a quick assault upon India by Soviet forces might Bolshevize that country. Had Davendra Kaushik dealt with that incident he would have relegated it to the collection of "misunderstandings" which litters his history of Soviet relations with the Indian subcontinent. For Dr. Kaushik, of the Department of History, Kurukshetra University, has attempted to prove that "right from the very beginning, Soviet policy towards India and Pakistan has been consistently aimed at strengthening friendship with both, while trying to bring them closer together" (p. 111). The value of this book lies in the argument it presents to uphold such an erroneous thesis: an argument which has gained currency among both Soviet and Indian defenders of the increasingly close relations between the two states.

The problem for Kaushik is to align the Indian version of Indo-Soviet relations with current official Soviet historiography. The "ultra-leftism" of the 1928–34 and 1948–50 periods has to be explained and criticized without damaging the official understanding of revolutionary Leninism. Similarly, the overtures towards Nehru's government made by Khrushchev and his successors must be justified according to the Leninist principles. Kaushik jumps in at the deep end. The change of line at the Sixth Congress of the Comintern is considered to be "unfortunate," its overthrow in 1935 was "not effected by a self-critical evaluation of the negative course . . . which allowed an easy relapse into the anti-Leninist ideological posture after the end of the World War II . . . . It was only much later, in 1956 at the Twentieth Congress of the CPSU . . . that the correct Leninist ideological stand towards national revolutions was restored" (pp. 17–18). The "correct Leninist" stand is here seen as a simple distinction between "the socialist movement of capitalist countries and the national movement of the colonial countries" (p. 14), a distinction, of course, which should be more justly attributed to Stalin.

Having constructed the outline of his mythology, Kaushik conceals its weaknesses with a gallery of characters who have either promoted or jeopardized Indo-Soviet relations. Lenin, Gandhi, Nehru, Khrushchev, Brezhnev and Kosygin belong to the first category; Roy, the early Dyakov and Balabushevich, the Chinese leadership, and all Pakistani governments are relegated to the latter. The only contention with the official Soviet history is over precisely what opened the way to mutual understanding: Nehru's nonalignment policy or the adoption of the "correct Leninist" line by Khrushchev in the 1950's. Kaushik tends to the view that with India's demonstration of nonalignment the "earlier Soviet mistrust of its intentions began to give way" (p. 54). The ensuing friendship survived despite the "severe strain" (p. 63) imposed by the Soviet intervention in Hungary and the "near storm" (p. 73) caused by Pravda's famous editorial during the 1962 Sino-Indian border conflict.

Kaushik echoes the diplomatic justifications for the relationship, quoting Nehru: "Let our coming together be because we like each other and we wish to cooperate and not because we dislike others or wish to do them injury" (p. 60). Great stress, however, is laid upon Soviet support for the liberation of Goa and for India's case against the Kashmir plebiscite. The MiGs delivered in 1963 were, according to Kaushik, "to be used for India's defence against China" (p. 74). Indeed, the focus of the book revolves around the 1965 war and consequent Tashkent peace, in which the author appears to relish the contradiction between descriptions of Soviet support for the case against the plebiscite, Soviet military supplies to India, combined with Kosygin's diplomacy at Tashkent, and yet asserting that "it was not its [the Soviet Union's] fault if Pakistan did not clasp the proffered hand of friendship, which India not only grasped but sought" (p. 83). Pages are devoted to Indian and Soviet statements celebrating the wisdom of the Tashkent Declaration, while it is noted that "a similar response to the declaration from the Pakistani leaders has unfortunately been wanting" (p. 99).

One purpose of the book is to combat the assertion by some Indian scholars (such as Harish Kapur) that the Soviet policy of scrupulously building equidistant relations with India and Pakistan by the Brezhnev and Kosygin team must imply a partial withdrawal from India, and from India's conception of South Asian security. Kaushik argues that the Soviet Union has always attempted to have good relations with both India and Pakistan and that only the reluctance of the latter has caused the consequent imbalance. Today, with the benefit of hindsight, Indian fears in this respect can be seen to have been effectively eradicated. "Good neighbourly relations between

India and Pakistan," concludes Kaushik, "are viewed by the Soviet Union as the sheet anchor of Asian security" (p. 111). The changed situation, however, in which Moscow's new "sheet anchor" is a militarily assisted and strategically powerful India, is unlikely to disturb Kaushik's mythologically conceived history of their relations. Indian scholarship on this question is rapidly becoming a mirror of its Soviet counterpart; a reflection of contemporary political and historical requirements rather than an objective inquiry into the purpose of such a relationship. But, as with Soviet work, it is no less valuable for the light it sheds upon the current justifications, motives, and effects of the relationship between the Soviet Union and its largest non-Communist arms recipient.

PAUL GERHARDT

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The United States, Communism, and the Emergent World. By Bernard P. Kiernan. (Bloomington: Indiana University Press, 1972. Pp. 248. $7.95.)

In a perceptive and cogently argued essay Professor Bernard Kiernan outlines what he considers to be the major errors underlying U.S. policy in the emergent world of Africa, Asia, and Latin America. Most important, according to Keirnan, is the tendency of Americans, including those in decision-making positions, to view revolutionary political activity in these areas as part of a worldwide Communist conspiracy directed by Moscow or Peking or both. Rather, according to Kiernan, Communism and other forms of revolutionary political activity should be seen as national responses to the myriad problems of economic and social development in backward societies.

The United States' effort to suppress this revolutionary process is, according to Kiernan, doomed to failure. In fact, the U.S. policy of containment which has resulted in various types of American intervention in the emergent world has actually pushed national leaders desirous of creating truly independent political-economic systems in the direction of revolutionary solutions and contact with the Communist world. The only alternative, Kiernan argues, to a continuation of a U.S. policy of intervention based upon the assumptions of containing communism can be found in an effort on the part of Americans to learn to live with the revolutionary politics of the emergent world. Such a revolution is contrary to U.S. national interests only when they are defined at the expense of other peoples.

Most of Kiernan's study is concerned with supporting his major argument. Although he does not provide major new insights in the chapters containing the supporting evidence and interpretations, Kiernan has produced a concise overview of the literature on the attraction of "communism" as a development strategy, the problems facing those countries that are developing in the late twentieth century, and the failure of the U.S. policy of containment of communism in the nonindustrialized world.

Kiernan begins his analysis with a survey of the development process in the West during the past several centuries. The most important part of his discussion of this subject, at least from the perspective of his major argument, concerns the undemocratic nature of the modernization process. Fortunately for the Western countries, however, several of the major tasks of modernization—in particular, state building—had already been accomplished before the process of industrialization was initiated. One of the major results of socio-economic-political modernization, as Kiernan argues, has been the major expansion of the role of the state in all aspects of human existence—in other words, a reduction of the independence of the individual vis-à-vis the polity at the very time that new organizational and technological developments have decreased man's dependence on the vagaries of nature.

The major goal of nationalist leaders in the emergent world is the acquisistion of the power to determine their own existence independent of of outside control. This requires the rapid creation of "modern" societies by telescoping the political, economic, and cultural changes that occurred over centuries in the West into a generation. It is here, according to Kiernan, that the Communist model of modernization asserts its attraction on these nationalist leaders. In addition, he maintains, since the United States has defined its policies toward the emergent world in terms of the containment of Communism and has viewed virtually any revolutionary movement as inimical to American interests, U.S. policy assists in pushing nationalists toward Communist alternatives.

On the whole, Kiernan's argument is well developed. His essay does suffer, however, from several rather serious flaws. Most important, to my mind, is his effort to categorize all revolutionary movements in the developing world as "communist," defined as an authoritarian movement, committed to economic development and social revolution pushed through from above, and inherently anti-Western (pp. 14–15). This definition of "communism," as Kiernan himself notes, can be applied both to formal Communist parties and to a wide range of other revolutionary movements. I do not believe that this conscious effort on the part of Kiernan to include all revolutionary groups in a single category facilitates our understanding of the revolutionary process. For example, he ignores the difference between those "revolutionaries" who have allied themselves with the Soviet Union and, in effect, taken orders from the Soviets, and others who have taken an anti-Soviet as well as anti-Western position. He includes Latin American Communists, who have played down revolution, with revolutionaries like Colonel Khaddafi of Libya who have opposed Communist solutions to socioeconomic modernization. Kiernan's approach makes it extremely difficult to understand the reasons for the attempted coup by the Communist Party in Sudan against the government of Numiery which fits Kiernan's definition of "communism." Kiernan himself appears to be aware of this problem, for on several occasions he does distinguish between "left-wing radicalism" and "Communism" (pp. 150, 151).

In addition, Kiernan's interpretation of future developments in the emerging world is too deterministic. For him the possibility of any but an authoritarian, "communist" approach appears to be ruled out and, in order to maintain this argument, he is required to include all countries in the same basic category of authoritarian/totalitarian without taking into consideration their major differences. The possibility that some countries (especially those that are blessed with more abundant resources) may not select the revolutionary approach to modernization is also downplayed.

Finally, the evidence is not clear that U.S. foreign aid has been used, as Kiernan argues, "primarily as a means of maintaining America's informal overseas economic empire" (p. 204). A recent study of U.S. economic aid by Eugene Wittkopf, for example, indicated that U.S. economic aid correlated highly with geographical proximity of the recipient to the Soviet Union and China and was thus directly related to a policy of containment rather than to U.S. business interests. This conclusion fits Kiernan's basic view more closely than does the claim which he actually makes.

These weaknesses in Kiernan's essay appear to be related to Kiernan's effort to present a comprehensive picture of the emerging world in which there are no "loose ends." Still, *The United States, Communism, and the Emergent World* does present an analysis of developments in the Third World and an assessment of U.S. policy there that is generally valid, even though a few countries do not fit neatly into the general model which is presented.

ROGER E. KANET

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Europe in the Twentieth Century. By George Lichtheim. (New York, Praeger Publishers, 1972, Pp. 372. $12.95.)

The late George Lichtheim's book is concerned with the transformation of European society in our century. The text attempts to summarize political, social, and economic history: the first part deals with the years 1900 to 1914, as the background of what is to come; the second discusses the First World War and the interwar years, while the last section of the book takes the story from the Second World War to the sixties. For Lichtheim, Europe is a unity which excludes both the USSR and the United States. This is no properly balanced text, however, but a *tour d'horizon* with one of the most powerful minds of our time, whose untimely death has robbed us of the kind of stimulation which this book so amply provides.

The last chapter of the book is called the "post European age," but it is typical of Lichtheim's turn of mind that he ends this discussion and the book by asserting that European influence might remain powerful in the realm of thought—provided it was based on the principles of theoretical reason as exemplified by the tradition of Kant, Hegel and Marx. This was the tradition within which Lichtheim moved, it set the standard for his judgments; "western standards" as he calls them here (p. 35). Compared with such standards, philosophies which differ such as those of Soviet Marxism or fascism are designated as "philosophical nullities" (p. 213), and the book refuses to linger long over such nonsense. The right kind of philosophy counts, for modern society is dependent upon critical assessment and this in turn must rely upon the human mind.

The dynamic of the twentieth century is provided by Marxism, a very proper viewpoint for the leading Marxist scholar of his generation. As a result, however, movements like fascism are defined in opposition to the left, they emerge as "negative" factors, though Lichtheim recognizes the contribution which fascism made to modernization. Still, this movement was retrograde, "drawn from the sweepings of the gutter" (p. 61), and therefore could not be understood on its own terms. All the fascists did was to take over and caricature the Communist example. Lichtheim was well aware that modern scholarship would dispute such statements, but fascism departed furthest from theoretical reason, and that was enough to characterize it. His judgments upon leading Marxist theoreticians also applied such standards. It seems bizarre to give pride of place to Antonio Gramsci and hardly any consideration to George Lukacs because he "became mired in the Stalinist bog" (p. 144). But for Lichtheim Lukacs abandoned the proper standards, while Gramsci in theory at least, returned to the original Marxist and Hegelian inspiration (whatever mistakes he made in practice).

George Lichtheim had little use for the argument that Lukacs went along with the Communist party line so as not to lose contact with the working classes. The people do not enter this book, nor does popular art or literature. Ideas interested him, and when the people do appear they are motivated by pressure rather then by ideology. Religion, myth and symbol, the motivating factors of twentieth century popular movements are outside Lichtheim's concern, and he seems to despair when he has to explain their triumph (as in fascism). This book is a history of intellectual movements and ideas. Thus, the stress on philosophies like logical positivism and, after 1950, upon a revival of Vico's philosophy of history which, I fear, never really took place at all.

But such weaknesses are also real strengths. At a time when so many scholars migrate into the market place Lichtheim refused to follow. What he himself found interesting became determinant in his historical analysis, if always within a Hegelian, dialectical framework, It does not really matter that at times some judgments are impossible to accept (thus Nietzsche is, above all, the greatest anti-Semite of all time), they are always relevant and thought-provoking. This is true even for his passionate admiration of de Gaulle who was "anachronistic" (as he writes here p. 35) but who seemed to him nevertheless the last world historical figure. His Hegelianism may have gotten the better of him there, but at the price of ignoring the General's philosophy. George Lichtheim loved, however, to provoke, to enter into an intellectual clinch with his opponents; there was nothing flabby about the man or his mind. He writes in this book about "superannuated liberals" who fought a rear guard action against irrationalism: but he himself typified the best of liberalism without its prevarications.

Lichtheim reminds us that intellectuals are not necessarily defined through opposition to all establishments, but rather through the clarity of their arguments and by the standards they are able to set. These certainly depend upon that theoretical reason which informs this book.

George L. Mosse

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International Economic Reform: Collected Papers of Emile Despres. Edited by Gerald M. Meier. (New York: Oxford University Press, 1973. Pp. xvi, 293. $7.50.)

Some twenty years ago, I was telling a friend about a long conference at which Emile Despres had consistently exhibited his mastery of the entire field of economics, his imaginative and constructive substitutes for much of the conventional wisdom, his sensitivity to the political implications of different economic policies, his gift for lucid exposition, and his fine sense of humor. "Yes," said my friend, "Since Emile won't write, he ought to be wired for sound."

This collection of his papers, skillfully selected and grouped by Gerald Meier, is the nearest thing to wiring for sound that we shall ever have. Despres died in the spring of 1973, having made almost no concessions to what he called "my lifelong aversion to writing for print." Nevertheless, his influence—as Professor of Economics at Williams College and Stanford University, as an employee of or consultant to various U.S. Government agencies, as first director of the Pakistan Development Institute, and in other capacities—was great. It is easy to see why.

The papers span three decades, 1938–1968. They consist of memoranda prepared for policy makers here and abroad, reports and recommendations, Congressional testimony, corrected transcripts of lectures, and two or three co-authored pieces written for publication. They deal with fundamental questions in three areas: international trade, international development, and international monetary reform. Remarkably free of jargon, only two out of more than twenty will give any trouble at all to political scientists untrained in economics; indeed Despres' style may come as a welcome relief to the tortured and turgid English of many of their colleagues. See, for example, Despres' 1959 testimony on "The Significance of the European Common Market to the American Economy," or pages 136–38 on import controls and other direct controls in less developed countries, or "Determining the Size of a Development Plan: Pakistan."

Most impressive perhaps are Despres' prescience and originality. He was integrating the problems of the less developed countries into his analyses long before it became fashionable to do so. He was prompt to call attention to the importance of developing agriculture in low-income countries and to the political strains inherent in the development process, including the strains caused by unemployment and by growing income disparities among economic classes and regions within poor countries.

He foresaw accurately the difficulties the European Economic Community would cause and the benefits it would bring. He was not the first to advocate the demonetization of gold, but he was one of its most effective advocates and was recommending the establishment of a two-tier market for gold five years before that policy was adopted.

Monetary economists could illustrate his unconventionality by numerous examples from the papers on monetary reform; I shall use for that purpose two papers on development. In a memorandum to Malaysia's Economic Planning Unit, Despres made what seems to me a convincing case for ignoring the cautious advice to Malaysia's leaders in a World Bank report and pursuing instead "an aggressive policy . . . with respect to obtaining and utilizing external loans" (p. 165). "Vigorous growth," he said, "attracts its own financing" (p. 169). In a brilliant paper on "Inflation and Development: Brazil," he explains why he believes foreign loan capital is normally a much better buy, from the point of view of a developing country, than foreign direct investment. The paper on Brazil, however, is most important for its analysis of why stopping inflation is the wrong immediate goal in the Brazil-type situation (of 1967). The preferable immediate goal is to make the inflation more neutral in its effects by eliminating the major distortions to which it gives rise.

If the book has a theme, it is the need for the world to free itself from the restrictive shackles of some of the most hallowed beliefs of economists so that it can, in good conscience and good cheer, equip itself for renewed, more rapid headway on international trade, international development, and international monetary reform.

ROBERT E. ASHER

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The Study of International Affairs: Essays in Honour of Kenneth Younger. Edited by Roger Morgan. (London: Oxford University Press, 1972. Pp. x, 309. $17.00.)

How does one review books "edited by . . . "? Such books have become a problem. In a recent issue of *APSR* one could find, within the confines of a few book review pages, complaints about "unevenness in the quality of contributions," "discontinuity in the themes pursued," "total failure of contributors to respond to one another," "lack of a unifying theme," "no apparent relationship among the articles"; it was "jarring to move from a critique almost directly to a prime example of what had just been debunked"; and so on. The problem is that, wherever some workers in some subvinyard of our profession congregate for some symposium, lecture series, or panel discussion, a volume of "collected essays" is due, conferring immortality in library catalogues upon the ed(s), but not otherwise appreciably promoting that knowledge which anyway tends to become less and less cumulative in our field. Are we then, increasingly, engaging in *Dichtung* rather than in *Wahrheit*?

Noting that the editor of the present volume describes it as a mixture of *Festschrift* (for the former director of Chatham House as well as the Royal Institute's own 50th anniversary) and *Mélanges* (in the French sense), I expected the worst, i.e., a hodgepodge of unintegrated articles. I was agreeably surprised: Most of the contributors (almost all connected with the Institute) had made an effort to adhere to the objective laid down by the editor: to "survey some of the major aspects of international studies at the present time," to deal with "the methods used to study international affairs," and to "place the relevant research activities of Chatham House in the perspective of international studies as a whole" (p. ix).

About half of the articles are area studies. Not surprisingly, they deal with "substantive" questions of the respective regions and countries rather than with the study of their international and foreign affairs. In most, there is at least a critique of the literature. Thus we have an essay on the Commonwealth (Bruce Miller), on Africa (Dennis Austin), on South Asia, primarily India (Hugh Tinker), and on Western Europe (Roy Price; with a brief survey of new methods to study "integration"). Michael Kaser, in a contribution on the Soviet Union and Eastern Europe, offers a mere survey of literature, mostly on domestic affairs. Would that at least bloc relations and bloc "integration" had been dealt with, not to mention the specifically Communist approach to studying international affairs. The one outstanding article in this group is John Gittings's on "Western Scholarship and the Cold War in Asia." Frankly revisionist, it undertakes to refute the "reactive myth" of a purely defensive U.S. policy in postwar Asia. I would say that, because of their mutual "security dilemma," both sides (as is generally the case in a cold war constellation) were acting in an "aggressively defensive" way, that is, subjectively, defending themselves against an opponent *viewed* as aggressive. But whether cold-war traditionalist, revisionist, or what not, the historian of the cold war will have to take notice of Gittings's arguments and the sources he meticulously assembles. Even this splendid piece, however, pays little attention to *how* to study a foreign policy (e.g., the new approaches by Allison et al. to foreign-policy decision making).

In-between the area studies and the methodological pieces there are some that straddle the approaches. D. C. Watt, writing about one of the chief glories among the Institute's products, the *Survey of International Affairs*, probes problems and pitfalls in the study of contemporary history, as illustrated by the variegated history of the *Survey* itself, with an outlook on how the "generalist" historian may cope with the problem of covering an ever more extensive and complex flow of data. Two articles by economists—Susan Strange and Caroline Miles—fall likewise in the in-between category. Strange issues a clarion-call for overcoming disciplinary divisions, calling specifically for a study of the "politics of international economics" and of the "economics of international politics." Miles, among others, calls for a new approach to the study of multinational corporations. One misses something on the neo-Marxist approach and theories of "economic imperialism." Two more articles deal with human rights and international law, respectively. J. E. S. Fawcett's on "Human Rights in International Relations" is a hodgepodge of random issues (dealing, in addition to human rights, with "aggression in Vietnam," "self-determination" in Rhodesia, etc.). Rosalyn Higgins's on "International Law and the UN System," on the other hand, is an admirable, concise yet profound probing of the place, role, and problems of law in present international relations and organization. Even this author touches the problems of study and method only incidentally. Yet this piece alone is (in Justice Holmes's words) "worth the price of admission."

Finally, the introductory and concluding methodological articles, by Andrew Shonfield and Roger Morgan, respectively: Shonfield emphasizes the importance of overcoming preconceived "explanatory models" in favor of agreed-upon methods and images. It is the business of scholars "to provide models of the international system which can be used by practical men of affairs to interpret new events in a way which assists them to make rational decisions" (p. 12). But how to arrive at such uncontested and undistorted "models"? Morgan finally arrives at what one had been waiting for all along: What about the new methods developed by the (primarily American) "quantifiers" and "behavioralists," the systems analysts, the students of decision making, etc.? Morgan perceives that there has already been a recession of the "new scientism," and a recent modesty in the claims of its protagonists. He stresses the importance of strategic studies for students of an international system vitally affected by the nuclear factor. He comes out for the development of "futurological" methods and models if the study of international politics is to remain "relevant to the real world" (p. 290). To this the reviewer can only say "Amen," but the question is, Can we agree on what is "relevant"? I have had my say on this (in *Polity*, Autumn 1971). Nothing, in my opinion, is truly relevant in international studies any more unless it is related to the questions of physical survival of mankind, i.e., the dangers of over-population, exhaustion of resources, and destruction of the global environment. Richard Falk has remarked: "Henry Kissinger, Hans Morgenthau, or George Ball will write at length about alliance patterns, bipolar relations, the strategic arms race, but will not devote a single word to the ecological fragility of the world" (*This Endangered Planet*, p. 181). Here is the challenge, to Chatham House as to all of us.

John H. Herz

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Strategic Forces: Issues for the Mid-Seventies. By Alton H. Quanbeck and Barry M. Blechman. (Washington, D.C.: The Brookings Institution, 1973. Pp. 94. $1.95, paper.)

U.S. strategic policy has been the subject of more extensive analysis and debate than all other aspects of our national security policy combined. For more than ten years, deterrence of nuclear attack through assured destruction has been the overriding strategic policy goal, but now this relatively well-defined objective may be fading as the administration and its military planners seek to acquire an increased flexibility to fight nuclear wars. Like a kaleidoscope, flexibility can mean many things to many people; it can be an open-ended invitation to procure all manner of new weapons and devise ingenious methods for their employment.

Quanbeck and Blechman, in their booklet published in the spring of 1973 by the Brookings Institution, have made an important contribution to the study of nuclear strategy at a time when a policy shift is raising many questions. They have not only pulled together in concise and clear form authoritative basic information on the strategic nuclear forces of the U.S. the U.S.S.R. and China, but, more important, they have put in perspective the political factors involved in force planning. In their analyses they add to the expenditures necessary for procuring the weapons themselves the prorated cost of operation and maintenance and extrapolate these costs to the end of the decade, thus providing a realistic picture of that fraction of our total military effort being devoted to strategic forces.

The book presents a series of alternative "higher and lower cost" aggregate strategic force postures, the "higher cost" options being only slightly above the FY74 program. This is not surprising since the administration was already pushing in 1973 for vastly increased strategic weapons procurement, and now the FY 75 Defense Department proposals apparently even surpass this "higher cost" option. Even the "lower cost" options put forth by Quanbeck and Blechman do not involve radical reductions in our strategic programs. The most extreme option, but one that is probably not politically viable, calls for phasing out land-based ICBMs because they may become vulnerable in the 1980s and for relying entirely on submarine missiles and bombers for deterrence.

After developing the factual base, the authors analyze the military, political, and economic issues presented by alternative programs for individual weapons systems. The potential vulnerability of the Minuteman ICBM force is the most critical problem facing strategic planners and affects decisions not only on ICBMs but on other strategic systems as well. Even the expensive and questionable Trident submarine missile program is related to increased concerns for survivability of the Minuteman ICBMs in the late 1970s. The book discusses the rationale for the acceleration of the Trident, but backs away from drawing any conclusions on whether it is needed or whether another smaller submarine, which is now in the new Defense program, would be better.

This refusal to take positions is an unfortunate characteristic of this Brookings staff paper. The authors, seeking the presentation of a balanced point of view, explore the pros and cons for each program without, regrettably, drawing any net conclusions. The reader is therefore left to evaluate for himself the soundness of the stated military requirements. More judgments from the well-qualified Brookings experts would increase the value of this book. Only in the final few pages do the authors advance any opinions, and these are made in such general terms as to reduce the usefulness of the study in guiding the less expert reader toward a reasonable strategic force policy. Therefore, the pamphlet is more valuable as a source of information and basic data than as a prescription for a sound future nuclear strategy.

HERBERT SCOVILLE, JR.

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Hitler's War Aims: Ideology, the Nazi State, and the Course of Expansion. By Norman Rich. (New York: W. W. Norton & Co., 1973. Pp. xliii, 352. $10.00.)

This is a strange book. Its title bears almost no relation to the text. To be exact, the Ideology of Expansion, as laid down by Hitler in *Mein Kampf*, is summarized in the first seven pages. Thereafter it hardly raises its head again except for occasional remarks that Hitler did not conform to his own rules. Another extraordinary feature is that there is no discussion of Germany's economic problems on a practical basis: not a word about the Great Depression or the system of autarchy with which Germany overcame it. Hence there is no attempt to discuss how far German foreign policy was economically motivated, how far, that is, Germany, a great industrial power inadequately endowed with raw materials, was seeking to break out of this strait-jacket. The discussion of the Nazi state is also pretty summary, all done with in 65 pages.

The rest of the book is devoted to the course of German expansion. The treatment of this, too, is uneven. Rearmament, for instance, is dealt with in the simple statement that Hitler rearmed Germany. There is no explanation of the particular character of German rearmament—all for the front line, nothing in reserve, preparation for a series of small wars, not for a great one. Surely this is fundamental to an understanding of German policy before the war and of German strategy during it. Lacking this key, Rich falls back on the banal statement that "Hitler was a pathological egotist whose lust for power and dominion expanded with the prospects that opened before him" (p. xiii). Later we are told that he was "above all a prophet of the cult of nationalism," whereas his real characteristic was a determination to make Germany a World Power. Hitler recognized that Germany was not strong enough to achieve this by a direct assault on the existing order and hoped to tiptoe into world powerhood unperceived.

The course of the war from the German point of view is, on the whole, described correctly. The element of forethought in the final occupation and dismemberment of Czechoslovakia is perhaps exaggerated, and Rich fails to appreciate how deeply Hitler was committed by German sentiment, particularly among his generals, to his demands on Poland. Thereafter Rich judges with a clear grasp. He shows that Hitler was driven against his will to overrun Denmark and Norway by the British plans to cut off Germany's supply of iron ore from Sweden. He also shows that the conquest of France and the Low Countries, as distinct from their defeat, was an aberration. As Seyss-Inquart remarked, when taking up his post in the Netherlands: "In the east we have a National Socialist mission; over there in the west we have a function. Therein lies something of a difference!" Probably Rich exaggerates also the importance to Hitler of the projected invasion of England and its failure. Sea Lion was a try-on by the German army and airforce which Hitler tolerated without showing much interest in it.

It is often said that Hitler made a great mistake by not moving through Spain to North Africa in the autumn of 1940. Rich ascribes this failure almost entirely to Spanish reluctance. But Hitler believed that Great Britain was already defeated, and he feared that a German penetration into North Africa, far from bringing about a British collapse, would provoke the United States into entering the war. The only way out, to Hitler's mind, was an invasion of Russia. In Rich's words, "Hitler's arguments in arriving at that decision made it seem not only the best of the various courses open to him, but less hazardous than many of the gambles he had risked during his previous seven years of power" (p. 26). Finally, Rich tells us, Hitler encouraged Japan to move southward in order to distract the United States from the Atlantic. When the Japanese actually went to war, he even threw in a declaration of war against the United States in order to encourage them further. This, too, was probably his motive, though Hitler's declaration of war against the United States is one of his few acts that cannot be rationally explained according to his own terms.

Rich's general approach is to explain German policy in terms of war. Hitler attacked Poland in order to secure his rear while he attacked in the west. He attacked France to secure his rear while he attacked in the east. He attacked Russia in order to secure his rear while he made a final assault on Great Britain. Only then would Germany become a World Power capable of fighting a great war against the United States. Sometimes Hitler said this would happen a hundred years later; sometimes he proposed to conduct it in his own lifetime. Perhaps therefore Hitler's war aim was to dominate the world—an ambition attributed to other Great Powers, including at the present time Soviet Russia and the United States. But to judge from events and Rich's analysis, Hitler's sole aim in the wars he fought between 1939 and 1941 was to win. As some chess grand master once said: "When I look at the board, I ask—does this move improve my position? If it does, I make it." Hitler's great negative aim was to avoid a World War. In this he failed.

A. J. P. Taylor

Beaverbrook Library, London

Planning, Prediction, and Policymaking in Foreign Affairs. By Robert L. Rothstein. (Boston: Little, Brown & Company, 1972. Pp. 215. $6.95.)

Professor Rothstein of Johns Hopkins University has written a complex, delightful, essay on the interplay of theory and practice in foreign policy making. He is not a disinterested party; his is an unabashedly intellectual point of view. Rothstein writes with candor and subtle humor, as he dethrones ruling ideas and prominent persons. His approach to the problem is in the end too intellectual, I think, but my disagreements with him do not diminish the fun and the profit in the reading.

Rothstein's thesis may be summarized as follows—though no brief account can do justice to the detailed web of his argument or to all of the insights in these pages: The international system, he claims, has changed fundamentally, so that the traditional principles and doctrines (which he describes as Political Realism, rather in the Morgenthau mold) are no longer relevant or applicable. International interdependence is the name of the new game, and developments within it can be influenced not by incrementalism but only by long-range planning. Such planning calls for an intimate relationship between theorists (planners) and practitioners (policy makers)—a relationship which will be possible only after major changes take place both in the way each of these sets of specialists views his proper professional behavior, and in the education of practitioners so that they know how to make use of the (more relevant) work of theorists.

The manifest purpose of the book is to get "true" policy planning enthroned in foreign policy making (though not in the State Department); but I found Rothstein's discussion of the problems of foreign policy making more perceptive than his proposed solutions. Rothstein has done a superb job in confronting the intellective, practical, and affective configurations of the "two cultures" of foreign policy. But while the world of the practitioner of Realism (e.g., pp. 76–77) is as good a capsule explanation of the White House of Watergate as we are likely to find anywhere, I am doubtful that practical remedies are going to be found—or should even be sought—through the work of theorists. Rothstein criticizes Political Realism for being, among other things, elitist and antidemocratic. It strikes me as professional myopia to believe that one can find a democratic alternative to these particular sins among academic foreign policy theorists! A generous definition of "theorist" might turn up a non-elitist (but not a non-elite) group, but Rothstein isn't even inclined to be very generous: Those in the academy who offer advice and opinion on policy are playing the practitioner's role (p. 15) and should not be regarded as theorists. The few who are left, however, can hardly be counted on to fuel a democratic counter-revolution.

This is not a petty point. Rothstein conceives of planning as "an effort to control or limit uncertainty," and it "clearly implies some active intervention into ongoing affairs" (p. 89). It is questionable whether any distinction between theorist and practitioner is viable in the light of this conception of planning. In any event, I would argue that this kind of planning goes to the very heart of political choice (if not of divine intercession) and is therefore far too important to be left to the theorists. If I understand Rothstein correctly, he admits at a later point that what he regards as an inescapable need to rely on expert judgment "surely does some damage to traditional principles of representative government" (p. 197). Had he confronted that problem at the beginning of his essay rather than at the end, the pages in between might have read very differently.

Rothstein has gunned down the foreign affairs bureaucracy with a practiced eye, as if the signal had at long last been given for the revolt of the intellectuals. And we can murmur "Write on!"

because his aim is so true. But I remain puzzled about why intellectuals, so addicted to canons of evidence on other propositions, are so readily persuaded by logic alone of their own unique and indispensable contribution to the political salvation of mankind. Where, indeed, is the evidence that the theorist *qua* theorist can help the practitioner to do better—to "improve his judgment" (p. 147)? One might argue that he can scarcely do worse—but self-serving thoughts of this kind are insufficient basis for the reorganization of national policy making. This is only one of the unexplored normative judgments that inform Rothstein's analysis; he aims for "true policy planning," which could never lead us to disaster, presumably, else it wouldn't be "true." His aspiration is that practitioners will "ask the right questions," and "make better decisions." All of which leads me to the conclusion that it is not policy making but policy itself that is up against the wall, albeit in disguise. (What then of Rothstein's credentials as theorist?)

Let me make the point another way: I do not find it so clearly established, as Rothstein seems to, that interdependence has replaced security as the dominant consideration in world politics. It is one thing to believe that the Cold War is over—and even that one should ferret out the remnants of the Cold-War mentality in the policy folkways of a generation, both in government service and out of it. But it is quite another to argue that the situation calls for an "alternative interpretation of world politics," for a new style of decision making, for changes in basic thought patterns (pp. 78–79). Overkill, perhaps, in the interest of an eventual new post-Vietnam policy doctrine, the likes of which are not revealed to us. If so, we might do better to confront that issue directly, so that we know what we are arguing about. But in the meantime, if we have to move the question "Where do we go from here?" to a higher, more generalized, plane, Rothstein has at least picked one with some substance, and treated it with intelligence and style.

BERNARD C. COHEN
University of Wisconsin, Madison

The Dimensions of Nations. By R. J. Rummel. (Beverly Hills: Sage Press, 1972. Pp. 512. $20.00.)

The *Dimensions of Nations* book has been built around, but is not a collection of, many of the earlier research reports and publications from the Dimensionality of Nations project (DON). This project, started in 1962 at Northwestern, has moved through several stages of development. The initial part of the project was begun to replicate the work of Cattell, who had previously located nations on various attribute dimensions using factor analysis. His study, however, was rather truncated and the DON project aimed at increasing vastly the number of nation and attribute variables processed. The *Dimensions of Nations* book covers this initial phase thoroughly providing a complete description of how the data were collected, processed and evaluated to produce massive evidence for empirical dimensions of nations, for a topology of nations, and for many political, social and economic indicators.

After this initial period (1962–66), attention was turned toward behavioral relationships between nations, a study which finally culminated in Rummel's Social Field Theory and Status Field Theory, which are briefly described in the book.

Since nearly half of the book has already been published in the prolific DON research reports, one may ask why publish a book? I feel that the book is an improvement upon the research reports. First, presumably having had to confront an armada of readers and editors all suggesting revisions and improvements, the book is a tighter product. Second, some of the unorthodox positions taken in the earlier research reports and published papers have been modified. A good example of this is Rummel's earlier view on explanation and prediction and the more acceptable one he puts forward in the book. Third, it is far more convenient to have one comprehensive summary volume covering the whole atheoretical period of the project than having the piecemeal research reports cluttering up one's shelves. The book exhibits mature consideration rather than the unfocused style so evident in the research reports.

And finally, since DON is one of the superprojects in international relations, having spent almost a million dollars, it is essential that there be some permanent and parsimonious record of its research. This volume, along with the others intended by Rummel, will form a series which will trace the development of the project from its atheoretical beginnings through its more theoretical research thrust.

The book contains a monumental amount of data on the attributes of eighty two nations. The attributes of these nations are spread among 230 variables. This whole data matrix is factor analysed to produce the more distilled dimensions of nations in attribute space. In this way nations can be located in attribute space and those clustered together are scrutinized for possible links producing such similarity. The research thus delineates nation types. Foreign conflict behavior is also processed along with the attributes of nations. It thus became possible to determine whether nations with any particular attribute pattern evidenced any foreign conflict behavior pattern.

Although the book is a wealth of data and data-related techniques and will be very useful to the international relations empiricist, I suspect that the volume (and the series) will also be of use to the scholar interested in the development and history of knowledge in political science. The project spanned a period from 1962 to 1973 and at one time had thirty staff members. It moved from Northwestern to Indiana to Yale and finally ended up in Hawaii. Much of the research has attracted fierce criticism, far more than is normal.

All of this evidence can be used to contrast the various ideas of knowledge growth put forward by Kuhn, Lakatos, and Popper. (Lakatos and Musgrave, *Criticism and the Growth of Knowledge* [Cambridge: Cambridge University Press, 1970.]). Without doubt a Kuhnian "normal science" perspective is exhibited in the approach of Rummel, and so too are the "naif falsificationism" of Popper and the "sophisticated falsificationism" of Lakatos. DON provides an excellent case study by which to compare and contrast these various ideas of how political science knowledge is accepted, rejected and accumulated. The book might have paid more attention to why the project began. Presumably no one initiates such a large project without some goal. Harold Guetzkow (one of the early project leaders) has mentioned that his INS and IPS simulations required validating data. If this was the original aim of the project (and it has been used in this way by Chadwick) what produced the swerve toward social field theory? Was it the rampant criticism of the inductive and atheoretical aspect of the earlier work of DON?

In general the book is well written and produced. One is allowed an awe-inspiring glimpse into the detail of the project, from the collection, coding and scaling of the data to the subtle analyses of the effects of data omission and error. As I think later volumes in the series will show, Rummel's incremental research strategy generates much admiration.

For the international relations scholar and the comparative politics specialist, there is a wealth of "clean" data readily available here, with which to test a variety of hypotheses. It is thus as useful a shelf addition as the World Political Indicators book (Russet, Deutsch, Alker, Lasswell. *World Handbook of Social and Political Indicators* [Yale, Yale University Press, 1964]), although it has less general relevance.

Rummel's early atheoretical style is still reflected in the book. For example, the first part of the book is devoted to just listing the findings of the project without any interpretation or fitting into extant knowledge. This is remedied to some extent later in the book, but not nearly as much as I would like. Nevertheless, his attempt at embedding, particularly in the later parts of the book, is some improvement upon the more factual and less interpretative research reports.

A significant part of the book is concerned with the methodology employed—factor analysis. A chapter or so attempts to enlighten the reader as to the various methodological maneuvers that follow. Factor analysis, however, is only one technique that might have been used. There are others, such as multidimensional scaling, all of which are equally credible. Nowhere in this volume does Rummel consider the alternatives, although in fairness to him they are considered in his Applied Factor Analysis book (Rummel, *Applied Factor Analysis* [Evanston: Northwestern University Press, 1970]). Nor is there much discussion of the underlying theoretical assumptions of factor analysis—assumptions such as linearity and additivity. Since the initial aim of the project was to replicate the work of Cattell, who used factor analysis, it may be that DON also had to use factor analysis. If this was the case, why not admit this instead of giving the impression that the use of the techniques was decided purely on research criteria.

Rummel is winding down the project now and is writing up the subsequent volumes in the series. I think that this first volume will be remembered for its methods rather than its theoretical import; for its data rather than its substantive depth. But it has value added because it traces the development of a large political science research project.

Gordon Hilton

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Republic Or Empire: American Resistance to the Philippine War. By Daniel B. Schirmer. (Cambridge, Mass.: Schenkman Publishing Co., Inc., 1972. Pp. 298. $9.50 cloth; $4.50, paper.)

Daniel B. Schirmer feels that the questionable American role in Vietnam has focused attention on past overseas adventures undertaken in the name of humanitarianism. In the 1890s, there developed one of the most fascinating such examples culminating in the acquisition of noncontiguous possessions. Cleveland's Venezuela Message (1895) asserted the force of the Monroe Doctrine in defiance of Great Britian and her South American policy. Latter-day Jeffersonians eschewed an activist, expansionist role for the United States, preferring instead to practice the time-honored policy of twisting John Bull's tail. But the McKinley Administration was more receptive to outward thrusts. The Hawaiian Annexation Treaty and the acquisition of empire that resulted from a war fought to liberate Cuba from Spain are cases in point. A "Great Debate" ensued in the Senate about the retention of overseas territory and especially about what to do with the Philippines. An independence movement thirty years old, and concern over the economic, humanitarian, and constitutional propriety of acquiring remote land made little impression in the thinking of American expansionists who equated taking territory five thousand miles from our shore with the growth of American power and status. Anti-imperialists opposed what they called an unrighteous war with the insurrectionists that lasted until 1902. Their organization, the Anti-Imperialist League, created in 1898, centered in Boston, and dominated by a New England intellectual elite, was the force around which "Little Americans" grouped. In the election of 1900, the League was stymied by a candidate with whom it had little sympathy except for his non-expansionist ideas, and by a cause that ran counter to the *Zeitgeist* at the end of the century. Expansion was the order of the day, the break with tradition was made, and the twentieth century seemed to portend a new vigorous policy for the Republic, one which nevertheless had many serious implications.

Revisionist historians have been criticized for polemical statements and strained analogies. The relation between the Filipino episode and Vietnam, and the American role in the Philippines as inspiration for Vietnam is too similar not to make the comparison. Relating earlier episodes to contemporary events is fraught with danger, but such parallels often show the relevance and provocativeness of historical study. While Schirmer acknowledges that the Vietnamese imbroglio and the Filipino insurrection were both imperialistic, his treatment of the latter involvement and of the anti-imperialist resistance is judicious, restrained, and well documented. Perhaps more controversial is Howard Zinn's introductory observation that because our historical education was so imperfect, we did not immediately associate the Filipino affair with the Vietnam war as the latter became "obviously ugly."

Not surprisingly, Schirmer feels that governmental imperialism was fostered by a complex of industrial, financial, and racist forces. America's economic position, this argument held, necessitated foreign markets, and the Philippine Islands were a convenient base for the illusory Chinese trade. As has been pointed out so often, however, not all business men were of a single mind; prestige and grandeur were strong motivations, and the anti-imperialists were also divided on the subject of economic determination.

Schirmer points out the intresting relationship between anti-imperialism and the antislavery movement of four decades earlier, demonstrating clearly the link between southern white supremacy and the policy of dominating the Filipino natives. Indeed, Schirmer's heroes—the anti-imperialists—were not only opposed to military-economic exploitation but were also the first to show the connection between the black civil rights struggles at home and overseas imperialism.

Despite the initial failure of the Anti-Imperialist League, Schirmer aptly notes that it was instrumental in tempering future policy on colonialism. Reforms did come within the military as a result of the publicity given over the "genocidal" subjugation of the Filipino rebels.

Schirmer's emphasis is on economic and racist motivation. The former is too simplisitic an explanation and the latter is a real contribution. This work is based on solid research in manuscripts, government documents, and periodicals, and the photographs and drawings are a real asset. Unfortunately, it relies too heavily on two leading Massachusetts newspapers, the bibliographical note is too short and hardly reflects this topic as an historiographic battleground, and the sources are listed as backnotes and not footnotes.

Despite these flaws, the author has focused our attention on an important historical episode which he feels has contemporary relevance. Unlike other analyses that attempt to draw parallels, this is basically a balanced and relatively unemotional treatment of an emotionally loaded incident.

MILTON PLESUR
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The Berlin Crisis of 1961: Soviet-American Relations and the Struggle for Power in the Kremlin, June–November 1961. By Robert M. Slusser. (Baltimore: The Johns Hopkins University Press, 1973. Pp. 509. $17.50.)

Professor Slusser's book offers a painstakingly researched chronological account of six critical months in the relations between the United States and the Soviet Union beginning with the Soviet government's insistence in June of 1961 that a peace treaty be signed with Germany by the end of that year. Amplifying this demand, Nikita Khrushchev declared West Berlin should be given the status of a "free city," and, following the conclusion of the peace treaty with the German Democratic Republic (GDR), control over the access routes to West Berlin would be turned over to the GDR authorities because "the sovereign rights of the GDR, through whose territory the communications linking West Berlin with the outside world pass, must be respected" (pp. 15–16).

Concurrently the Soviet government was intent on resuming atmospheric tests of nuclear weapons in the multimegaton class in violation of the 1958 de facto test ban. At the same time the United States was seeking to work out a formal treaty banning such tests. During this period certain other Soviet foreign policy objectives had a bearing on Soviet-American relations; they included the introduction of the troika system for the post of Secretary General of the United Nations, improvement in the relations with the Peoples' Republic of China and Albania, and Khrushchev's desire for the Soviet Union to surpass the United States economically.

Despite various Soviet threats, which prompted the Kennedy Administration to increase defense expenditures substantially and call reserves to active duty, and alternatively confidential personal communications from Khrushchev to Kennedy through letters and other means urging negotiations on the peace treaty, the deadline was abandoned in October, and the troika initiative for the UN given up. However, the Berlin Wall, designed to stop the flood of refugees to West Germany, was built during that period, and the Soviet Union proceeded with atmospheric tests of nuclear weapons up to 58 megatons.

In his analysis and evaluation of the crisis-filled six months Slusser examines in detail the factors responsible for the rise of tension between the superpowers and the pressures building up on Kennedy and Khrushchev to adopt tough positions toward each other. The author probes deeply into the factionalism and ongoing power struggle in the Presidium of the CPSU's Central Committee as it relates to Khrushchev's ability to formulate and carry out policies. His main antagonist and leader of the hardliners in foreign policy was Frol Kozlov, whose strategies for placing constraints on Khrushchev's actions and for imposing his own foreign policy concepts toward the United States and China are carefully analyzed. Clearly, major decisions about the policies to be pursued before and during the crisis were not those of Khrushchev alone but of the collective leadership in the Presidium. In certain instances, however, Khrushchev sought to circumvent the Presidium's collective decision making or to influence it through devious means.

Although in Kozlov's view good Soviet relations with China should receive highest priority, for Khrushchev it was only the Soviet Union and the United States which counted in world politics. *If these two countries unite for peace, there can be no war,* he declared in his confidential letter to Kennedy (p. 255). Khrushchev's idea of a Soviet-U.S. joint rule over the world has continued to attract subsequent Soviet leaders but has upset many of NATO's European members who fear that such a close U.S.-Soviet relationship would diminish their power.

In his evaluation of the interrelationship between the peace treaty deadline and the nuclear tests Slusser implies that the tests constituted a smokescreen to distract attention from the Soviet abandonment of its much heralded plan to sign a peace treaty with East Germany before the end of the year (p. 168). Could the reverse not be true, however? The tests may well have been the primary objective for 1961 and the peace treaty secondary. The threat to sign the treaty with the GDR may have been designed to raise the level of tension in East-West relations in order to absorb the attention of NATO leaders and conceal the chief intention of Soviet decision makers to test new powerful weapons, with the increased tension serving as a convenient pretext for the tests.

Without doubt Slusser's "microscopic" study (p. ix) of the power struggle within the Presidium based on minute details observed during the 22nd CPSU Congress has yielded significant inferences about the activities in the Presidium regarding foreign and internal policies. But the long quotes from the more important speakers during the Congress and the summaries of the speeches of others are somewhat tiresome, and the chapter on the Congress—nearly 200 pages long—could have been shortened appreciably without depriving the reader of the necessary insight into the factional struggle. Despite this minor criticism, Slusser's book makes a major contribution to better understanding the Kremlin's decision-making process, which is far from the monolithic procedure that existed under Stalin. Just as in the United States, there appears to be a definite interplay between domestic and foreign policies. It would have been interesting to learn about the influences of the Soviet bureaucracy upon foreign policy formulation, and compare them with studies made by American scholars on the influence of U.S. bureaucracy on U.S. foreign formulation.

WERNER J. FELD

University of New Orleans

Barriers to Progress in South Vietnam: The United States Experience. By Edward Reynolds Wright. (Seoul, Korea: Pomso Publishers, Ltd., 1973. Pp. 174. No price indicated.)

So many books have been published about the civil war in Vietnam, and particularly concerning American intervention, one might wonder what more could be added. In *Barriers to Progress in South Vietnam: The United States Experience,* Edward Wright, long-time executive director of the U.S. Fulbright Program in Korea, has produced an analysis, although short in length, which is uniquely penetrating and revealing.

Having served as a visiting professor in Saigon during the fateful 1963–1964 period that witnessed the fall of Diem and incipient American belligerency, and again in 1966 during massive Americanization and escalation of the war, Dr. Wright has shown himself a consummate student of the history, culture, economics, and politics of this ill-fated country.

This study is neither a prescriptive policy paper nor a detailed analysis of the traumatic developments since 1963. Nor is any attempt made to treat U.S. efforts to pacify the countryside or to assess the peace movement within U.S. society—the substance of most recent books on Vietnam.

Rather, it is the author's effort "to clarify the underlying issues which should continue to be basic in any policy formation with regard to Vietnam." Accordingly, considerable attention is devoted to developmental programs and progress for the decade 1954–1964, analysis of which ends in 1964 when the conflagration of war enveloped the Vietnamese. The author suggests that this is the critical period in the recent history of Vietnam, for this decade embraced "the barriers to developmental progress" that explain the foreground to war and will persist for years to come. The merit of this study, then, is that it identifies and clarifies the problems that confront strong nations which try to guide economic, social, and political development in weak nations.

The massive United States aid program in South Vietnam entailed the expenditure of more than $2,100,000,000 dollars in economic assistance alone between 1955 and 1964. In assessing the administration of U.S. aid, Dr. Wright poses this question: "To what extent does the dispenser of aid attempt to impose its values on the administration and implementation of the aid program?" This is a problem of "cultural transference"—of transferring money, materials, and skills from the U.S. whose social strata and political culture are radically different from Vietnam.

The author identifies three major categories of "barriers to progress" in utilizing U.S. aid: (1) political instability and military insecurity, (2) widespread waste and corruption in the use of U.S. aid funds, and (3) a drastic shortage of trained technicians and professionally qualified personnel. South Korea, he asserts, is a success story because it has "strikingly different potentialities" for managing its own affairs.

Although discussion of these barriers represents the thrust of this book, it appears to this reviewer that the author makes another singular contribution to the burgeoning literature on Vietnam through imparting his understanding of Vietnamese society and his grasp of its historical background. His second chapter on the French War in Indochina is quite instructive, particularly about political cleavages within France which affected French policy in Vietnam.

It was Georges Bidault's short-sighted anti-Communist policy, and his refusal to consider the basic issues in Indochina, which brought an end to the Laniel Government. The final decisions reached at the Geneva Conference of 1954 were instigated by the Pierre Mendès-France Government in which Bidault and the MRP refused to participate. Dr. Wright's fluency in French enhances his treatment of these developments, as he exhibits equal facility in the use of French- and English-language sources.

If this book has any flaw, it is the brevity of the author's treatment of the Viet Minh and North Vietnam's political culture, especially its ideology. At least equal weight should be accorded to the ideological and nationalistic perseverance and will of the opponents of the French, the Americans, and the Saigon governments.

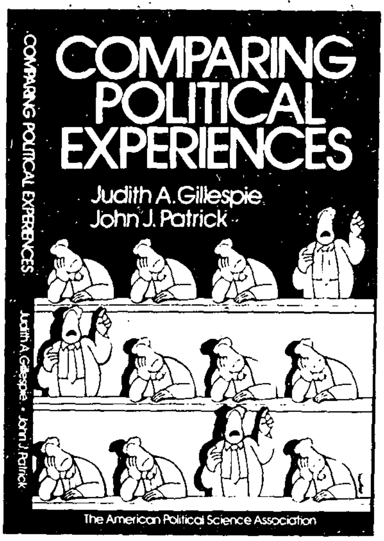
But Dr. Wright has produced a brilliant book, well documented, attractively designed, and clearly written. Added features include a useful guide summarizing major periods in Vietnamese history, ten tables concerning economic factors, an appendix comprising the full text of the Vietnam Peace Agreement of January 27, 1973, and a complete index. A "postscript" on Vietnamization consists of the author's devastating review of the book "No Exit From Vietnam" by Sir Robert Thompson—reputed creator of President Nixon's Vietnamization strategy.

The depth of Edward Wright's analysis is indicated by enlargement of his main theme, in his conclusion: "In the final analysis, the relatively developed nation can only advise, help and protect. By no means should they attempt to impose a unilaterally derived alien system upon a traditional culture and expect it to 'work'." Had such advice been followed, perhaps the tragedy of Vietnam could have been avoided. Here, then, is a lesson that should be heeded by other nations which are recipients of U.S. aid.

William W. Boyer

University of Delaware

This IS NOT another textbook . . .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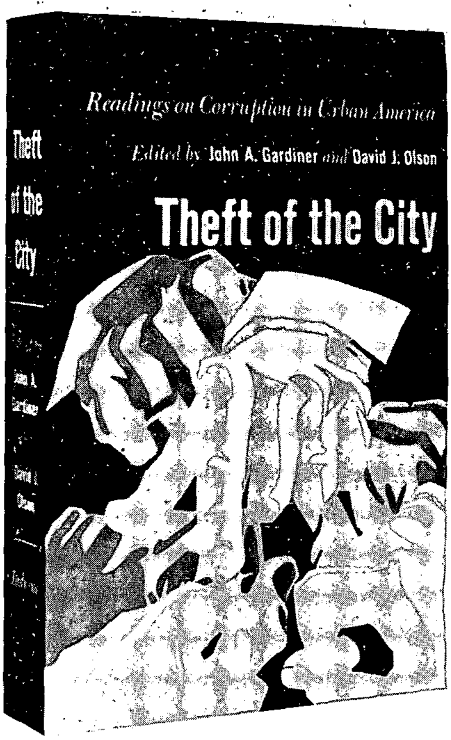
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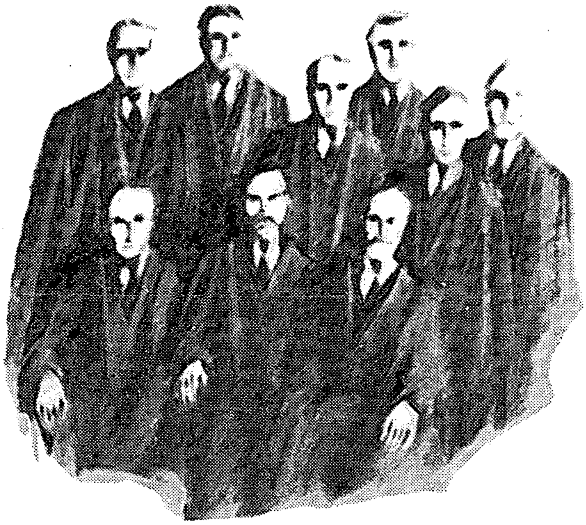

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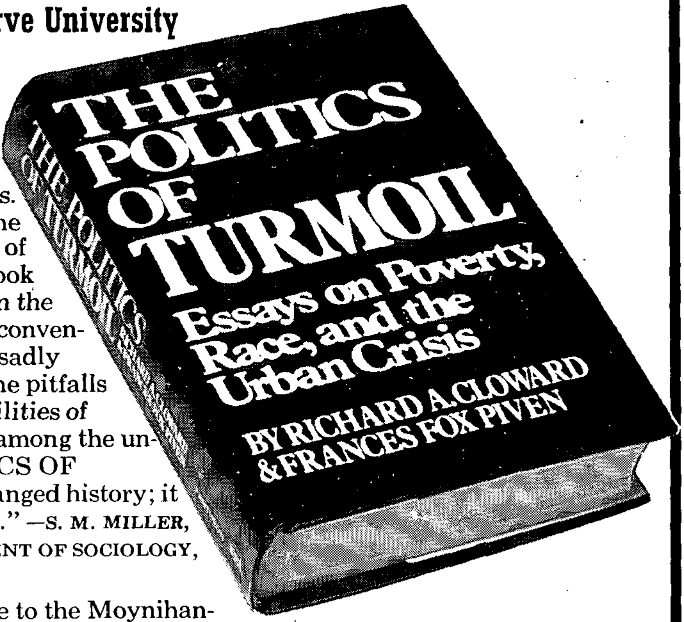
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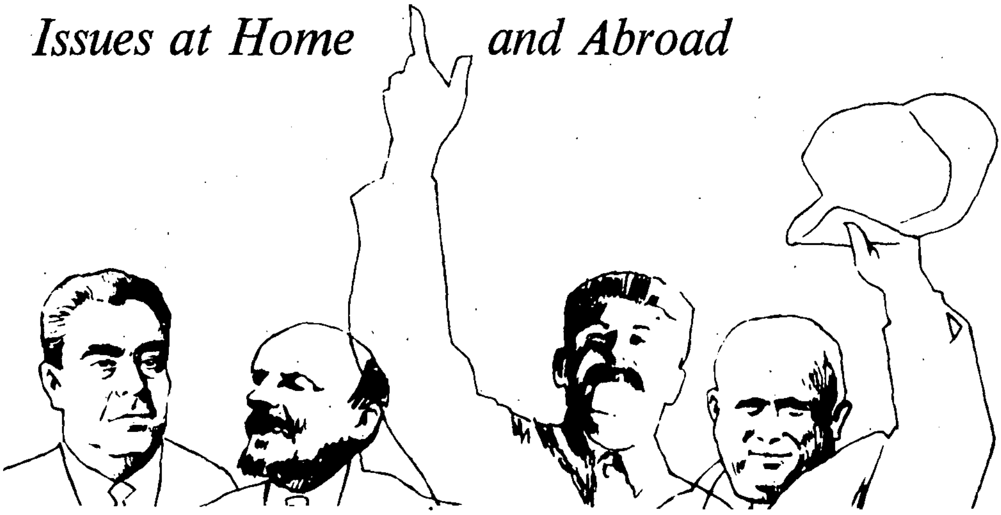

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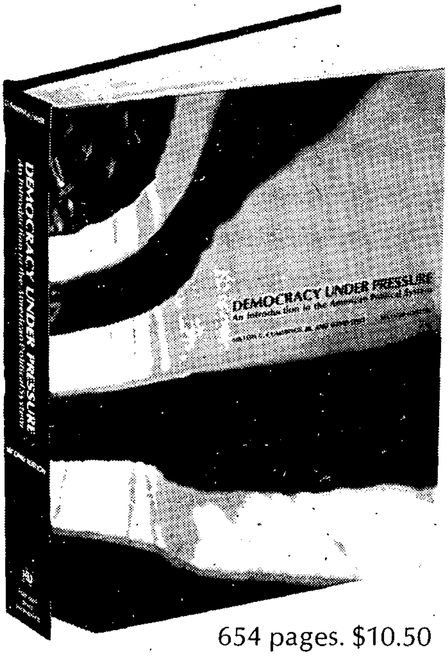

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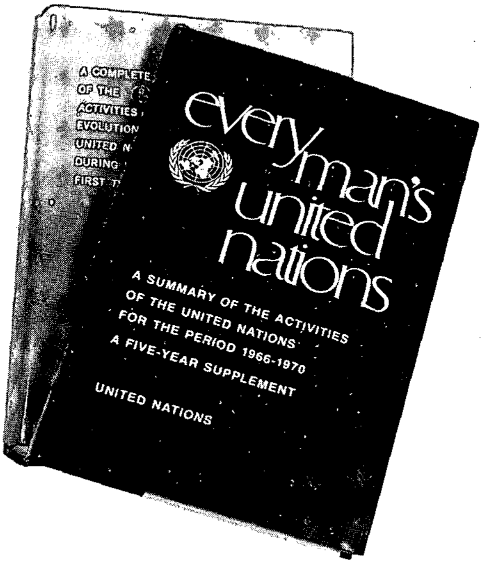
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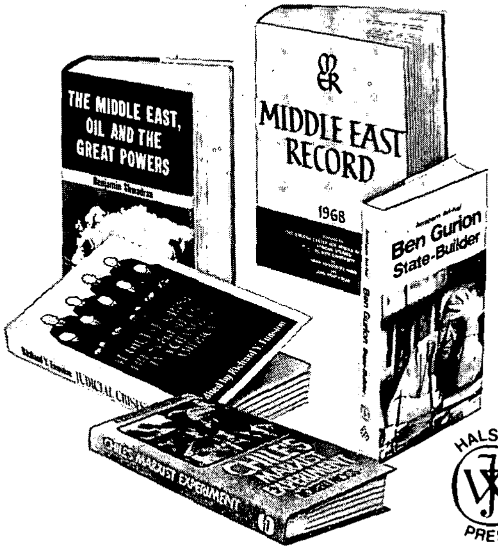

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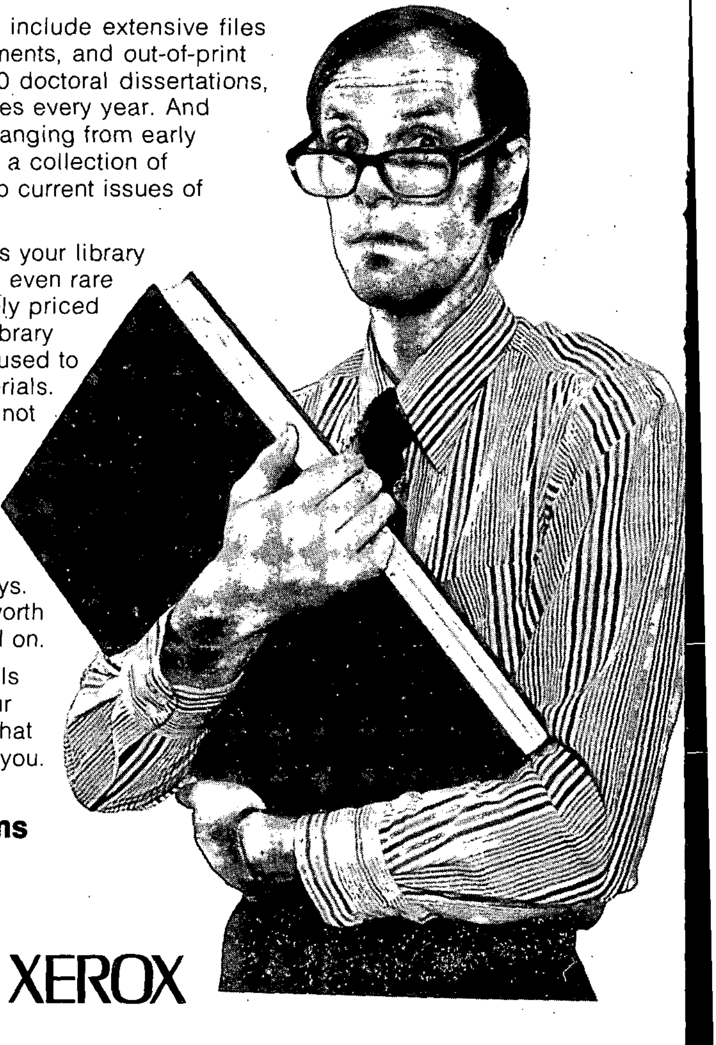

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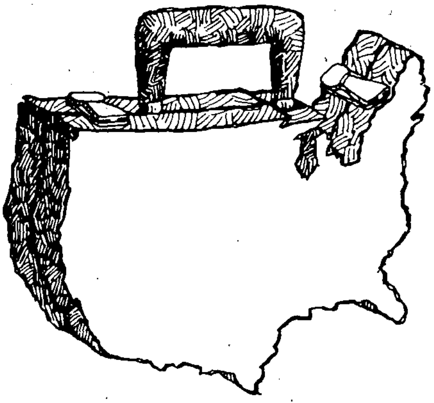

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